PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

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ERRATA.

Vol. XV. page 469, line 6 from bottom, the expression in brackets $\{\ \}$ should be raised to the nth power.

Vol. XV. p. 486, in equation (1) for $\frac{dz}{dy}$ read $\frac{dz}{dx}$.

The equation which occurs at the foot of page 488 is limited to the case where $a=x+$a const.

Vol. XV. page 497, line 12 from bottom, *for* $+0{\cdot}00631$ *read* $+{\cdot}000631$.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

June 6, 1867.

The Annual Meeting for the election of Fellows was held this day.

Lieut.-General SABINE, President, in the Chair.

The Statutes relating to the Election of Fellows having been read, Major-General Boileau and Mr. J. Clerk Maxwell were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the Lists.

The votes of the Fellows present having been collected, the following Candidates were declared to be duly elected into the Society :—

William Baird, M.D.
W. Boyd Dawkins, Esq.
Baldwin Francis Duppa, Esq.
Albert C. L. G. Günther, M.D.
Julius Haast, Esq., Ph.D.
Capt. Robert Wolseley Haig, R.A.
Daniel Hanbury, Esq.
John Whitaker Hulke, Esq.
Edward Hull, Esq.
Edward Joseph Lowe, Esq.
James Robert Napier, Esq.
Benjamin Ward Richardson, M.D.
J. S. Burdon Sanderson, M.D.
Henry T. Stainton, Esq.
Charles Tomlinson, Esq.

June 20, 1867.

Lieut.-General SABINE, President, in the Chair.

Dr. William Baird, Dr. Günther, M.D., Capt. R. Wolseley Haig, Mr. Daniel Hanbury, Mr. Whitaker Hulke, Mr. Edward Hull, Mr. Edward J. Lowe, Dr. B. Ward Richardson, Dr. J. S. Burdon Sanderson, Mr. Henry T. Stainton, and Mr. Charles Tomlinson, were admitted into the Society.

In accordance with the Statutes, notice of the ensuing Anniversary Meeting for the election of Council and Officers was given from the Chair.

The following communication was read:—

I. "Description of an Apparatus for the Verification of Sextants designed and constructed by Mr. T. Cooke, and recently erected at the Kew Observatory." By Balfour Stewart, LL.D., Superintendent of the Kew Observatory. Received May 9, 1867.

In order to test the accuracy of graduation of a sextant, it is necessary to have a series of well-defined objects, the angular distances between which must be accurately known. The sextant under trial is made to measure these angular distances; and the results thus obtained, when compared with the correct values of these distances (supposed to be otherwise determined), will give at once the error of the instrument.

Now with regard to this series of objects, the following conditions are necessary in order that they may be convenient for the purpose of testing sextants:—

α. It is necessary that the objects should be distinctly seen and well defined. Luminous objects would be preferable, if these could be obtained. Luminous points would answer well.

β. It is necessary that they should be at a very great, or virtually infinite distance from the sextant, so that two lines proceeding from any point in the objects, the one to the index-glass, and the other to the horizon-glass, should be virtually parallel to each other.

γ. It is necessary that these objects should be at such angular distances from one another, that by means of them it may be possible to test, say every 15° of a sextant's arc.

δ. It is necessary that these objects should be always visible, or at least that they should be rendered visible easily, and without loss of time.

A series of fixed stars, at suitable intervals from one another, might be made to fulfil the first three of these conditions; but in this uncertain climate it would be inconvenient to adopt any mode of verification depending for its success upon the visibility of the sun or stars;—in fine we must have a source of light which can always be commanded.

A plan proposed by Mr. T. Cooke fulfils this requirement, and as it has now been carried out with apparent success, a short description of it may perhaps be acceptable to the Royal Society.

His arrangement is of the following nature:—

F, G denote two collimators, F having a single vertical line, and G a couple of cross lines, as shown in the figure.

The collimator F is at the principal focus of the lens *a*, and the collimator G at that of the lens *b*.

Elevation of Collimators, showing wires viewed from outside the circle.

Plan of Cooke's Apparatus for Verifying Sextants at the Kew Observatory.

A. Double collimators.
B. Table for holding sextants.
C. Sextant.
D. Candles for illuminating wires.
E. Slate to which collimators are bolted.
F & G. Wires in collimators.

Furthermore, the lenses are so adjusted that the two lines, the one of which is that proceeding from the centre of the collimator F to the centre of the lens *a*, and the other that proceeding from the centre of the collimator G to the centre of the lens *b*, shall be parallel to one another.

This condition is fulfilled in the following manner:—A telescope having an object-glass sufficiently large to embrace at once the two collimators *a* and *b*, is focused by means of a star for an object infinitely distant. It is then used as an instrument wherewith to view these collimators previously illuminated; if they appear in focus, it follows that they are to be optically regarded as infinitely distant bodies, and thus that they are accurately at the principal foci of their respective lenses.

In the next place, things are so adjusted that the vertical collimator shall appear to bisect the cross-wire collimator in the field of view of

the telescope. This adjustment is one which, from the form of the two collimators (a straight line and two cross lines), can be made with great exactness, and when accomplished, it follows that the two collimators, F and G, are to be optically regarded as two infinitely distant bodies, both being in the same direction.

Each of the two collimators has a moveable cover, so that, if desirable, the one can be viewed without the other.

The collimator lines are illuminated in the following manner:—These lines are formed of fine glass threads, and the light of candles symmetrically disposed is allowed to fall upon these threads. By this arrangement the threads are rendered luminous on both their sides at the same time; there is therefore no perceptible parallax, such as would follow from the one side of the thread being lit up at one time, and the other at another time.

It is now necessary to describe the method of fixing the collimators.

A brick erection was made in the basement hall of the Observatory, having the shape of a circular arc. To the flat top of this erection three pieces of slate, all in one horizontal plane, and having their upper surfaces curved, were attached by cement; finally, a slate slab, E (shown in the figure), was laid so as to rest simultaneously on these three curved surfaces. The collimators, being intended to rest on this slate slab, had their lower surfaces made quite flat, and were firmly bolted by means of screws to the slab.

The angular distances between the collimators are (roundly speaking) as follows:—

From	1	to	2	.... 30°
„	1	„	3	.... 60°
„	1	„	4	.... 105°
„	1	„	5	.... 120°

A horizontal table, B, capable of motion, either vertically or in azimuth, and also capable of being rigidly fixed in any required position, is placed in the centre of the circle of which the boundary line of the slate slab constitutes the circumference.

In order to determine accurately the angular distance between the various collimators, a theodolite is placed on the table B, so that when levelled its telescope, as it sweeps round in azimuth, may be able to bring into the middle of its field of view the various collimators.

No care need be taken that the centre of the theodolite is precisely in the centre of the circle, because the collimators being virtually at an infinite distance, it follows that the angular distance between any two of them does not depend on the exact centering of the theodolite.

Now, if any theodolite be taken, and if a number of sets of observations of the angular distances between the collimators be made, each set starting from a fresh point in the azimuth circle of the theodolite, it

is evident that by this means we shall eliminate any error of graduation of the theodolite.

A complete set of determinations of these angular distances ought, therefore, to refer to at least three starting-points in the horizontal circle of the theodolite, these being, say 120°, apart from each other.

The following complete sets have been made at various dates by Mr. G. Whipple:—

Measurements of the Angles between the Collimators of the Apparatus for the Verification of Sextants.

The measurements were made with a 6-in. Theodolite, divided to 30″.

TABLE I.

Angles between collimators.	Dates of Observation.				
	Nov. 23, 1866.	Nov. 26, 1866.	Feb. 27, 1867.	May 2, 1867.	Means.
	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″
Nos. 1 and 2...	29 59 44·4	29 59 35·0	29 59 45·0	29 59 26·7	29 59 37·8
" 1 " 3...	59 59 31·7	59 59 41·7	59 59 46·7	59 59 29·3	59 59 37·3
" 1 " 4...	105 0 1·7	105 0 1·7	104 59 41·7	104 59 46·7	104 59 52·9
" 1 " 5...	120 0 11·7	120 0 11·7	119 59 56·7	119 59 53·3	120 0 3·3

TABLE II.

Differences from Means.			
′ ″	′ ″	′ ″	′ ″
+0 6·6	−0 2·8	+0 7·2	−0 11·1
−0 5·6	+0 4·4	+0 9·4	−0 8·0
+0 8·8	+0 8·8	−0 11·2	−0 6·2
+0 8·4	+0 8·4	−0 6·6	−0 10·0

TABLE III.

Angles between Collimators.	
	° ′ ″
Nos. 4 and 5	15 0 10·4
" 1 " 2	29 59 37·8
" 3 " 4	45 0 15·6
" 1 " 3	59 59 31·7
" 2 " 4	75 0 15·1
" 2 " 5	90 0 25·5
" 1 " 4	104 59 52·9
" 1 " 5	120 0 3·3

It will be seen from Table II. that the observational differences from the means are extremely small, and capable of being accounted for by the uncertainty of reading a theodolite graduated to 30″. We may therefore suppose the positions of the collimators to have remained the same throughout the period embraced by our observations.

In conclusion, it may be desirable to describe in a few words the method by which a sextant may be verified by means of this apparatus.

Let us suppose the collimators to be accurately and quite immoveably fixed, and the angular distances between them to be accurately determined. Also let the distance between the two lenses *a* and *b* of any collimator be such that the collimator F may be seen through *a* at the horizon-glass, and the collimator G through *b* at the index-glass of an ordinary sextant placed on the table B.

In order to test the index-error of a sextant, the vertical line of a collimator is made to bisect the cross lines belonging to the same collimator in the field of view of the telescope of this sextant. If the sextant is accurate, it should read *zero*, since these two lines are infinitely distant and in the same direction.

The sextant is next placed with its horizon-glass receiving the rays from the vertical line collimator F_4 (G_4 being covered), and its index-glass receiving the rays from the cross line collimator G_5, and the telescope arm is moved until F_4 bisects G_5 in the field of view; if the instrument is correct, the reading ought to be (by Table III.) 15° 0′ 10″·4. By pursuing this method it is evident from Table III. that the error of graduation of the sextant may be determined at every 15° of its arc.

In conclusion, it ought to be mentioned that perhaps no artifical light easily obtainable is sufficiently powerful to allow of the darkest glasses of a sextant being examined, and that for this purpose we may ultimately have to resort to other means.

II. "On the Observations made with a Rigid Spectroscope, by Captain Mayne and Mr. Connor, 2nd Master of H.M.S. 'Nassau,' on a voyage to the Straits of Magellan." By J. P. Gassiot, F.R.S. Received May 25 and June 3, 1867.

In a communication I made to the Royal Society on the 18th of May 1865 (Proceedings, vol. xiv. page 320), I described the rigid spectroscope which, at the suggestion of Mr. Balfour Stewart (in connection with a plan jointly conceived by Prof. Tait and himself), I had had constructed, the object sought being to determine by observation whether the index of refraction does not vary with the coefficient of terrestrial gravity, for which purpose it was thought desirable that the observation should be entrusted to some officer on board one of H.M. ships visiting various latitudes on both sides of the equator.

Through the kindness of Captain Richards, Hydrographer to the Admiralty, I obtained an introduction to Captain Mayne, of H.M. Ship 'Nassau,' at that time (August 1866) fitting out at Woolwich, preparatory to making a survey of the Straits of Magellan, and by appointment with Captain Mayne I visited the 'Nassau' in company with Captain Richards.

After carefully examining one or two positions in which the instrument could be placed, Captain Mayne selected a place in his own cabin, and explained to us that it was his intention to place the spectroscope in charge of an intelligent young officer, Mr. Connor, the 2nd Master, by whom the observations would be made; but as the instrument necessarily remained in Captain Mayne's cabin, the observations would be made generally in his presence, and under his immediate superintendence.

Captain Mayne and Mr. Connor shortly afterwards examined the spectroscope at the rooms of the Royal Society, in the presence of Mr. Stewart and myself, when they practised the mode of observing; but, in order to ensure the observations being subsequently made without any bias as to obtaining particular results, no further explanation was given to Captain Mayne or Mr. Connor, the latter being merely requested day by day to note the result of his observations, and to enter the same in printed forms with which he would be supplied, Captain Mayne promising to forward the particulars to Captain Richards at his convenience.

The form supplied was as follows:—

Date.	Latitude.	Barometer.	Temp. of air.	Temp. of prisms.	Reading of micrometer for D line.	Remarks.

Mr. Browning, who constructed the instrument, took charge of it on the 21st of August 1866, and proceeding to Woolwich placed it on board the 'Nassau,' in the position which had been arranged by Captain Mayne.

On the 4th inst. I received a letter from Captain Mayne, of which the following is an extract:—

"H.M. Surveying Ship 'Nassau,' Straits of Magellan,
Feb. 16, 1867.

"My dear Mr. Gassiot,—As we are on our way to the Falkland Isles, and my time will probably be fully occupied when we get there, I write you a few lines to say that I am sending to the Hydrographer a diagram of the observations of the spectroscope taken since we left England; with it I am sending a few remarks. I can only say that our observations have been carefully taken, and I hope a discussion upon them may throw some light upon the subject in which you and others are so interested. Mr. Connor plotted the diagram with great care. Usually the observations have been made by him, but I have taken

them occasionally as a check, and also during the time he was laid up with a wound which these wretched Fuegians gave him. You will see the method pursued in the diagram is to give the whole voyage complete, and also the fluctuations during our stay at the various places named. Let me add, what I also said to the Hydrographer, that we shall be happy to carry out any changes in position of the instrument, or mode of observation, you may wish, so far as our other duties will permit. I shall be very glad also to hear that our observations have been in any way useful. The weather we have hitherto experienced has been rather better than we expected, but gale, gale, gale, the wind seems never tired; if it does for a few hours forget to maintain the credit it has obtained, be sure you will find a current of five or six knots directly opposed to the course you wish to pursue.

"Please remember me kindly to Mr. Stewart when you see him, and

"Believe me yours sincerely,

(Signed) "R. C. Mayne."

"*J. P. Gassiot, Esq., F.R.S.*"

The following is a copy of the letter referred to in the preceding extract:—

(Copy.)

"H.M. Surveying Ship 'Nassau.' Straits of Magellan,
Feb. 15, 1867.

"Sir,—I beg to forward the following remarks on the rigid spectroscope which was placed on board this ship at the request of Mr. Gassiot, V.P.R.S., with the view of determining whether the position of the D line of the spectrum changes with the coefficient of terrestrial gravity. Accompanying the remarks is a diagram, intended to show at a glance the fluctuations which have actually occurred in the line of the spectrum during our voyage from Plymouth to the Straits of Magellan, as well as those of the barometer and "air" and "prism" thermometers during the same period. In addition to this it has been thought advisable to plot in the same way the fluctuations which occurred during the ship's stay at the various ports of call on the voyage out independently, as they cannot of course be in any way due to change of gravitation. They will be seen on the lower part of the diagram, the same number of observations having been plotted after our arrival in the straits as were taken at Plymouth, the two places being so nearly in the same latitude.

"A description of this instrument has been given by Mr. Gassiot before the Royal Society. Its position on board being selected by Mr. Gassiot himself, in concert with Mr. Browning, it was placed, at their desire, on the port side of my cabin, and its position has in no way been altered since. All the observations have been made either by Mr. Connor or myself: Mr. Connor, in whose special charge the instru-

ment was placed, having observed it by far the most frequently, and having given considerable attention to it. The observations were made at noon daily, and, owing to the deficiency of light, have (at Mr. Browning's instance) been made by bringing the moveable micrometer wire into the centre of the right bright space instead of its right edge, as I understand was the use at Kew.

"The diagram seemed to show that the barometer affects the instrument, the micrometer reading increasing as the barometer falls, and *vice versâ*. In this regard it is curious that, from a series of twenty observations, taken since we have been in the Straits of Magellan, Mr. Connor deduced that the micrometer should read 3·84 when the barometer was 30 inches and attached thermometer 54° F., and that a few days since, when the barometer and thermometer were as above, the micrometer reading was 3·86. The fluctuations shown when the ship was stationary seemed to point out that at least the barometer has very considerable effect upon the reading of the instrument: what amount of the general changes shown may be due to change in the coefficient of terrestrial gravity I leave to those who have made the spectrum their study to determine.

"The barometer readings shown on the diagram have purposely not been reduced to the mean temperature of 32° Fahr., as it is thought they show better what is desired than if they were so; should it be thought advisable to reduce them it can easily be done, as the temperature of the air-thermometer plotted hardly differs perceptibly from that attached to the barometer. The observations are still being made daily, and the record of them kept; if any change of position, other method of observing, or any other alteration is thought advisable by those interested in the observations, it shall receive all the attention the nature of the service on which we are employed will permit us to bestow.

"I have the honour to be, Sir, your obedient Servant,

(Signed) "R. C. Mayne, Captain."

"*Captain Richards, R.N., Hydrographer, Admiralty.*"

As the diagrams referred to could not be conveniently engraved for insertion in the Proceedings, they remain at the Royal Society.

The observations of Captain Mayne and Mr. Connor have evidently been made with great care; the diagrams executed by the latter gentleman exhibit at a glance the actual results.

In these diagrams two series of observations are recorded; the first exhibits the spectroscope reading, along with the reading of the barometer, of the thermometer imbedded in the glass prism, and of that showing the temperature of the air around the instrument as the vessel proceeded on its course.

The second exhibits similar records, made when the vessel stopped

at various places during the voyage. In order to make use of these records, it became necessary to ascertain the corrections of the instrument.

Immediately on receipt of Captain Mayne's letters, I forwarded them to Mr. Stewart, to whom I am indebted for the following observations, with the necessary corrections for temperature, &c. These corrections are,—

I. *The Temperature Correction.*

In order to determine the correction, very complete sets of experiments were made by Mr. Balfour Stewart at Kew Observatory, and by Mr. Browning at the Minories.

In January 1866 the spectroscope was conveyed to Kew Observatory, and was there exposed to a change of temperature equal to 30° Fahr. The change was applied very gradually, the experiments for one set lasting nearly one month; in respect to duration these changes were consequently analogous to those to which the instrument would be exposed at sea, but, on the other hand, the instrument, when at Kew, was not subjected to vibrations.

In February 1865 the spectroscope had been subjected to similar changes of temperature at Mr. Browning's house of business in the Minories, which abuts on the Blackwall railway. The results there obtained are recorded in the Proceedings of the Royal Society (vol. xiv. June 1865). These observations extend over a range of 30° Fahr.; and as in the greater portion of the time during the observations the instrument was subject to constant vibration from the ordinary work carried on in the work-rooms, as well as from the abutting railroad, these constant vibrations were so far analogous to the action to which the instrument would be subjected to on board a vessel.

On the other hand, the heating and cooling each took place on the same day, and as far therefore as duration is concerned, the temperature changes at the Minories were dissimilar to those to which the instrument was exposed at sea.

The result of these experiments was that, for an increase of temperature of 30° Fahr., there was observed an increase in the reading of 1·32 revolution of the micrometer screw.

The following Table exhibits the result of the observations made at Kew Observatory:—

Date.	Temperature of prism.	Reading.	Date.	Temperature of prism.	Reading.
1865.			1865.		
Jan. 13.	48°·2	0·67	Feb. 12.	52°·8	1·26
14.	51·5	0·75	18.	52·5	1·22
16.	75·6	2·00	23.	49·9	1·04
17.	80·8	2·24	24.	50·8	1·02
18.	76·0	2·22	March 2.	73·7	2·26
19.	74·8	2·05	3.	76·8	2·43
20.	75·1	2·04	4.	76·6	2·43
21.	70·5	1·71	5.	75·0	2·36
22.	75·0	2·04	6.	78·3	2·64
23.	74·5	1·87	7.	79·0	2·76
24.	75·4	1·86	8.	78·9	2·68
25.	74·6	1·83	9.	76·8	2·46
26.	75·4	1·92	10.	75·5	2·28
27.	77·4	2·15	12.	56·3	1·37
28.	75·9	2·09	21.	51·0	1·26
29.	76·7	2·14			

These experiments consequently consist—

(1) of a set of readings at low temperature.
(2) „ „ high „
(3) „ „ low „
(4) „ „ high „
(5) „ „ low „

If we compare (2) with the mean of (1) and (3), and (4) with the mean of (3) and (5), we obtain as the Kew correction for temperature an increase of 1·44 revolution for an increase of 30° Fahr.

On comparing the temperatures obtained under very different treatment at the Minories at Kew, and we find—

At the Minories 30° Fahr. gives 1·32 revolution.
At Kew . . . 30° „ 1·44 „

Mean . . . . 1·38 „

These two results are therefore extremely consistent with each other, although the treatment to which the instrument was exposed differed materially in the two cases. We should therefore say that the temperature correction should not vary with change of treatment, and have therefore considerable confidence in applying the above value of it to observations made on board the 'Nassau.'

II. *Correction for Change of Atmospheric Pressure.*

It is justly remarked by Captain Mayne that the readings seemed to vary with the barometer.

This correction is, however, small; and as the change of mean barometric pressure between the different latitudes is small also, the correction will not affect the range, but it will affect the comparability of individual observations, and ought therefore to be determined. This is best done by means of the observations themselves after the temperature corrections have been applied.

Thus we find that, when the ship was stationary at Plymouth, there were considerable fluctuations of the barometer, and from the observations made there, it appears that a rise of half an inch creates a fall in the reading =0·18. Similar fluctuations took place while the ship was stationary at Magellan, and from these we obtain a fall of 0·24 for every rise of half an inch.

The mean of the two, or a fall of 0·21 in the reading for a rise of half an inch, may safely be adopted.

No correction has been applied for the hygrometric state of the air. M. Jamin has found that aqueous vapour, at the temperature of 0° Cent., and under the pressure of 0·76 mètre, would have for its index of refraction (if it could exist under such a pressure) the value of 1·000261, which is less than that of air, which is 1·000294; the superior quantity of aqueous vapour would thus tend to diminish the index of refraction of air at the equator, as compared with that of the same pressure at higher latitudes.

This action of vapour, which is very small, will be such that without it the residual range (which will be afterwards exhibited) would appear to be somewhat larger than it at present appears; it cannot therefore account for the residual range, and may be in the meantime neglected.

If we now tabulate from the diagram sent by Captain Mayne, and if, by means of the above-named corrections, we reduce all observations to 60° Fahr. and to 30 inches barometric pressure, we obtain the following readings for the various latitudes between 45° N. and 45° S., the ship being in motion.

Note.—Since the barometer correction is small, and the range of temperature from the equator to the high latitudes of the voyage not much more than 20° Fahr., it has been thought unnecessary to reduce the barometer readings to 32° Fahr.

Latitude.	Readings reduced.	Reading reduced and corrected for fall of zero.	Difference (negative).
45° to 40° N.	4·71	4·71	·0
40 — 35	4·56	4·58	·13
35 — 30	4·50	4·55	·16
30 — 25	4·47	4·54	·17
25 — 20	4·45	4·54	·17
20 — 15	4·35	4·47	·24
15 — 10	4·31	4·45	·26
10 — 5	4·29	4·46	·25
5 — 0	4·20	4·39	·32
0 — 5 S.	4·20	4·42	·29
5 — 10	4·19	4·43	·28
10 — 15	4·22	4·49	·22
15 — 20	4·25	4·54	·17
20 — 25	4·18	4·49	·22
25 — 30	4·16	4·56	·15
30 — 35	4·13	4·49	·22
35 — 40	4·13	4·52	·19
40 — 45	4·29	4·70	·0

On comparing the last number of the second column with the first it will appear that there is a change in the zero of the instrument; presuming that this change took place during the voyage at the same rate, we obtain column 3 corrected for change of zero, and column 4 representing differences.

This residual difference, which remains after all known corrections have been applied, is exhibited for the different latitudes in the following diagram (fig. 1), while the observed temperatures of these latitudes are also exhibited.

Fig. 1.

In the next place, let us take the diagrams sent by Captain Mayne, which exhibit the readings of the spectroscope at the various places of

far as this cause of variation is concerned, by the ordinates of the straight line (dotted), fig. 2; and accordingly Mr. Stewart, *assuming the uniformity of instrumental change*, takes the difference of ordinates of the dotted and curve lines (fig. 2) as a quantity unexplained by known causes. This quantity follows very well the march of the latitude, *or of the temperature which marches with the latitude.* It would be removed by supposing the temperature-correction applied to be too great by about

0·30 or 0·33 for 20°.

"Such an error in the temperature-correction can hardly be said to be too great to be admitted. Still it is greater than we should expect *if the London and Kew determinations apply to the state of things on board ship.*

"I do not think Mr. Browning's conjecture that the progressive change was due to an alteration in the glass of the prisms probable. I should think it more probably due to a slow release from a state of constraint in which the instrument was left by the boltings, &c. If so, I should expect that the instrument would shake itself down to a permanent state—that the instrumental change would be more rapid in the early than in the later part of the voyage.

"In fig. 2 I have drawn a smooth curve by the eye following generally the irregular curve, with a view to clearing in some degree the observa-

Fig. 2.

tions from casual errors. The curve ought perhaps to lie a little lower on the right, but I was anxious to lean rather towards Mr. Stewart's view than the reverse. The smooth curve of fig. 2, lifted so as to cut the axis on the extreme right, is transferred to fig. 3. If we suppose the instrumental change more rapid at first than afterwards, the correction thence arising will be represented by the ordinate, not of a straight line as in fig. 2, but of some such *concave* (upwards) curve as the dotted curve of fig. 3, and the unexplained residue will be reduced to the ordinates intercepted between the curves of fig. 3. If the curves are somewhat as I have drawn them, this residue will follow very well the march

of the latitude, *or of the temperature* which marches with it, and may be removed by supposing the temperature-correction applied by Mr. Stewart to have been too great by about

$$0{\cdot}10 \text{ for } 20^\circ,$$

a quantity well within the limits of uncertainty.

Fig. 3.

"I think therefore that the statement made in the paper—that a residue so and so exists after all known causes have had their effects eliminated—is too boldly advanced.

"The inference I should be disposed to draw from the observations is:—

"That the influence of the variation of gravity (or rather, I believe it is supposed to be, of the potential of the Earth's attraction) does not exceed, in passing from lat. 45° to the equator, a change of refraction for the yellow of the spectrum equal to about three-fourths of the interval of the D-lines; and that even this small apparent change may be referred with great probability to known causes.

"As to future observations I should say—

"I. Let the observations be repeated on the homeward voyage (I presume the survey is not yet finished, and the 'Nassau' is still in the Straits of Magellan). If the instrument has shaken down into permanence the result will be different (I mean the result *uncorrected* for change of zero), and one source of uncertainty will be removed.

"II. Should the observations taken in the homeward voyage lead to the same result, repeat the observations for temperature-correction by means of the change naturally occurring with change of season, relying chiefly on observations taken during pretty *uniform* weather of whatever kind.

"Yours very truly,

"*J. P. Gassiot, Esq.*"

"G. G. Stokes."

"Kew Observatory, 31st May, 1867.

"My dear Sir,—I have read Prof. Stokes's remarks on your proposed communication regarding the rigid spectroscope, and before adverting to any point on which we may have a difference of opinion, it may be well to remark that we both agree that the experiment in its present state is not decisive—in any case more observations must be

made. Acknowledging with him that the variation due to any other cause than temperature must be sought for in the residue left on eliminating the temperature-correction, I yet venture to differ with him regarding the degree of accuracy to which we can trust the temperature-correction.

"Referring to Prof. Stokes's numerical results, I should object to found any theory upon the first-noted reading ·71.

"The rigid spectroscope was unfortunately brought to Kew only one day before the temperature experiments (instituted more particularly for the magnetographs) commenced, and only two readings were taken before the temperature was raised. I believed it only right to include these two in the account given of temperature observations, but I take this opportunity of saying that I do not attach much value to the mean of these two.

"If these two be excluded, Prof. Stokes's Table will be modified in the following manner:—

Mean temperature of set.	Mean reading observed.	Deviation from mean temp. of the four sets.	Calculated reading.	Observed — calculated.
75·5	2·01	+11·2	2·26	—·25
51·5	1·13	—12·8	1·13	·00
76·7	2·48	+12·4	2·31	+·17
53·6	1·31	—10·7	1·23	+·08

giving an error much less than that shown by Prof. Stokes.

"If now we compare together the temperature-corrections for 30° of range, as determined at the Minories and at Kew, we find—

		Difference from mean.
(A) Temperature-correction for 30° at the Minories	=1·32 ...	—·06
(B) " " Kew	1·44 ...	+·06
Mean	1·38	

"I should imagine the set of observations taken on board to be comparable in number and accuracy with either (A) or (B), and I should expect, *on the supposition that the temperature-correction may be determined equally well by land and by sea observation*, to obtain a result differing from the mean of (A) and (B) by a quantity something like ·06. If temperature-correction be the same at land and at sea, I think therefore that the residual difference observed at sea cannot be attributable to an imperfect estimation of temperature-correction.

"Prof. Stokes next suggests that the progressive change of the instrument may have been greatest at the first part of the voyage. Allowing as the greatest possible extreme though very improbable case, that it all took place before the equator, that would still leave a difference of ·09 revolution to be accounted for.

"But this supposition cannot evidently be entertained; indeed, as the

instrument was nearly two years old when it went to sea, there is a difficulty in supposing that the correction was very much greater at the first half than at the second half of the voyage.

"On the whole I should be disposed to state the result in the following manner:—

"That the influence of the variation of gravity does not exceed, in passing from lat. 45° to the equator, a change of refraction for the yellow of the spectrum equal to about three-fourths of the interval of the D-lines; but more observations must be made before it can be asserted that this apparent change is not due to known causes.

"Yours very truly,

"*J. P. Gassiot, Esq.*" "B. STEWART."

So favourable an opportunity of making correct observations with a delicate apparatus like the rigid spectroscope may not again offer, and consequently, in acknowledging the receipt of Captain Mayne's letter of 17th Feb., an extract from which is inserted in the preceding communication, I explained how desirable it will be while the 'Nassau' remains in the Straits of Magellan if one observation on each day is taken, or two when any very considerable range of temperature occurs, for the purpose of being made use of both for change of zero and as checks upon temperature observations to be taken on shore on the return to this country, as otherwise the observations thereon would not be so useful.

Should it be found practicable on the return of the 'Nassau,' it is purposed to take a few days' readings before the spectroscope is removed from the ship to Kew Observatory, as this would much promote the correctness of the final result which may then be anticipated.

J. P. G.

Clapham Common, June 3, 1867.

III. "On some Elementary Principles in Animal Mechanics." By the Rev. SAMUEL HAUGHTON, M.D., Fellow of Trinity College, Dublin. Received May 15, 1867.

There are some elementary principles in animal mechanics which are so natural that they may be assumed as probable, and as such, have not received from observers the attention they really deserve.

Among these principles I select for illustration the two following:—

i. *The force of a muscle is proportional to the area of its cross section.*

ii. *The force of a muscle is proportional to the cross section of the tendon that conveys its influence to a distant point.*

i. In order to test the first of these statements, I made a careful examination of the cross sections of the muscles that bend the fore arm and leg, in a very finely developed male subject, with the following results:—

Neglecting the slight effect of the *Supinator radii longus* in flexing

2. *Semitendinosus* . . . . . .
3. *Semimembranosus* . . . . .
4. *Gracilis* . . . .
5. *Sartorius* . . . .

When the arm was held vertically, and tl
the fist shut and in supination, I found th
weight that could be lifted when suspende
of the elbow-joint; and that the perpendi
tions of the muscles from the same axis w

1. *Biceps humeri* . . . . .
2. *Brachiæus* . . . .

Hence if K denote the force of the mus
section, we have, adding 2lbs. for the weigh
from the axis of the joint,

$$41 \text{ lbs.} \times 12\tfrac{1}{4} \text{ inches} = K \times$$

$$502\tfrac{1}{4} = K \times$$

$$= K \times 5$$

and finally $K = 94{\cdot}7$ lbs.

This represents the force per square in
muscles flexing the fore arm are capable o

In order to measure the force of the mu
the observer lying upon his face upon a t

	Perpendicular
1. *Biceps femoris* (long head)	0·95 in.
„ (short head)	0·56 „
2. *Semitendinosus*	0·40 „
3. *Semimembranosus*	0·65 „
4. *Gracilis*	0·25 „
5. *Sartorius*	0·00 „

Hence we find, for the determination of K (the coefficient of muscular contraction per square inch of cross section),

$$37 \times 16\tfrac{1}{2} = K \times \left\{ \begin{array}{l} [0{\cdot}95 \times 2{\cdot}59 \\ \;\;0{\cdot}56 \times 1{\cdot}14 \\ +0{\cdot}40 \times 1{\cdot}87 \\ +0{\cdot}65 \times 2{\cdot}25] \\ +0{\cdot}25 \times 0{\cdot}89 \\ +0{\cdot}00 \times 0{\cdot}59 \end{array} \right.$$

or,

$$610{\cdot}5 = K \times \left\{ \begin{array}{r} 2{\cdot}460 \\ 0{\cdot}638 \\ +0{\cdot}748 \\ +1{\cdot}462 \\ +0{\cdot}222 \\ +0{\cdot}000 \\ \hline 5{\cdot}530 \end{array} \right.$$

and, finally,

$$K = \frac{610{\cdot}5}{5{\cdot}53} = 110{\cdot}4 \text{ lbs.}$$

It appears from the foregoing considerations that the force of contraction of the muscles, per square inch, is in

The arm	94·7 lbs.
The leg	110·4

These numbers are, perhaps, as near to each other as this class of observations admits of, but I believe that they do not differ so much, really, as they appear to do, for the following reason:—

As it was not convenient to procure a good subject destroyed by a violent death, I made use of a powerful man who had died of cholera *, and who had been a blacksmith by profession. Now, it is natural to suppose that the muscles of the arm of a blacksmith are more developed than those of his leg, so that their cross section would be relatively too great, and the coefficient derived from that cross section, therefore, too

* It is well known that after death by *Cholera*, life continues in the muscles, and manifests itself for some hours by movements, and by the existence of the muscular *susurrus*. This latter fact, the first notice of which belongs to Dr. Collongues, of Paris, I have repeatedly verified, as also the continuance of the *susurrus* in cases of death by *tetanus*. It appeared to me, therefore, that such a subject as I selected was one well suited to the purpose of my observations.

small. I therefore compared the sections of the *Biceps humeri* and *Brachiæus*, found by me, with the only other measurements, with which I am acquainted, for the knowledge of which I am indebted to Dr. W. Moore of Dublin, who translated the results for me, from the Dutch, of Messrs. Donders and Mansfelt * of Utrecht.

Cross Sections of *Biceps humeri* and *Brachiæus*.

	millims.	sq. in.
1. *Biceps humeri* (long head)	530	0·821
" (short head)	452	0·701
2. *Brachiæus*	614	0·952
	1596	2·474

If this estimate of the cross section of the muscles be assumed instead of my own, the coefficient found by me should be increased in the proportion of 3190 to 2474; or

$$\text{Coefficient of muscles of fore arm} \quad . \quad . \quad . \quad 94{\cdot}7 \times \frac{3190}{2474} = 122 \text{ lbs.}$$

The mean of the coefficients found from my own measurement of the muscles of the arm, and that of Professor Donders, is 108·4 lbs., which agrees nearly with that obtained from the muscles of the leg, viz. 110·4 lbs., and the mean of all the observations on arm and leg would be **109·4 lbs.**, a result which I consider to be not far from the truth.

The cross sections of the muscles were found by cutting them across with a sharp scalpel, and marking out their section on cardboard, and afterwards weighing the marked portions, the weights of which were then compared with the weight of a known number of square inches of the same cardboard, and so the cross sections in square inches calculated.

I give here, for the purpose of illustration, the actual sections of the muscles of the leg. (Figs. 1–6.)

The perpendiculars let fall upon the directions of the muscles were measured by stretching strings from the origin to the insertion of the muscles, and measuring, by means of a compass, the perpendiculars let fall upon these strings from the axis of the joint.

The weights of the muscles themselves were as follows:—

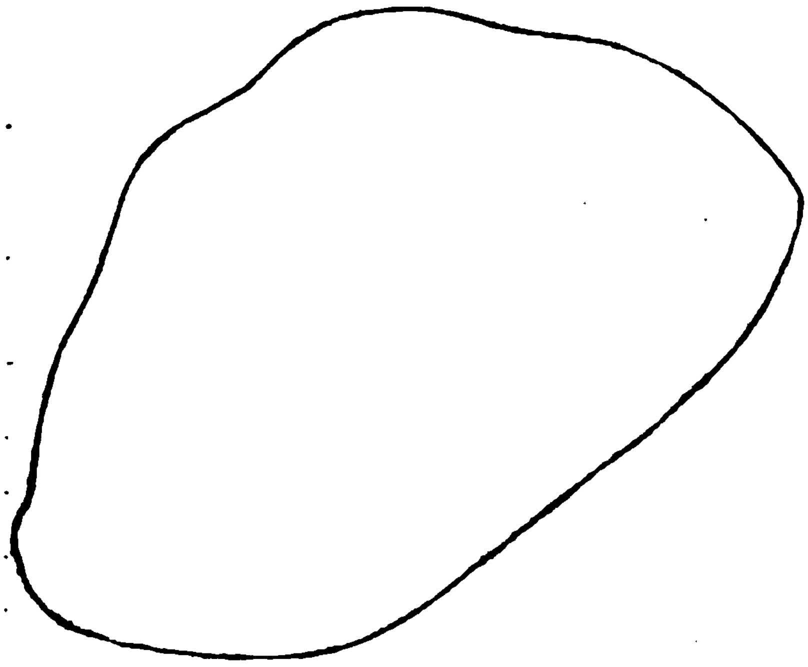
	oz.		oz.
1. *Biceps humeri*	4·22	5. *Semimembranosus*	7·25
2. *Brachiæus*	5·04	6. *Gracilis*	2·98
3. *Biceps femoris*	10·74	7. *Sartorius*	5·66
4. *Semitendinosus*	5·17		

ii. The principle of economy of force or of material in nature would lead necessarily to the principle that each tendon conveying the effect of a force to a distant point should have the exact strength required, and neither more nor less; for, according to the doctrine of *final causes*, it was originally contrived by a perfect architect, and according to La-

* Over de Elasticiteit der Spieren. Utrecht, 1863.

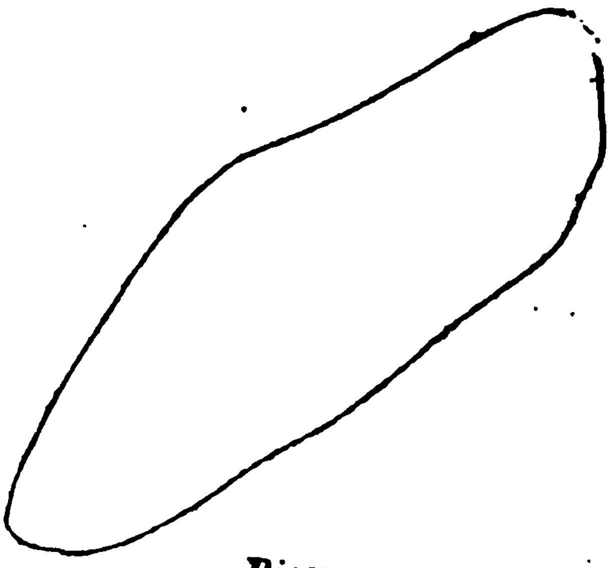
marckian views it must have perfectly accommodated itself to the uses to which it is applied. According, therefore, to either view, if the tendon be too strong, it will become *atrophied* down to the proper limit;

Fig. 1.



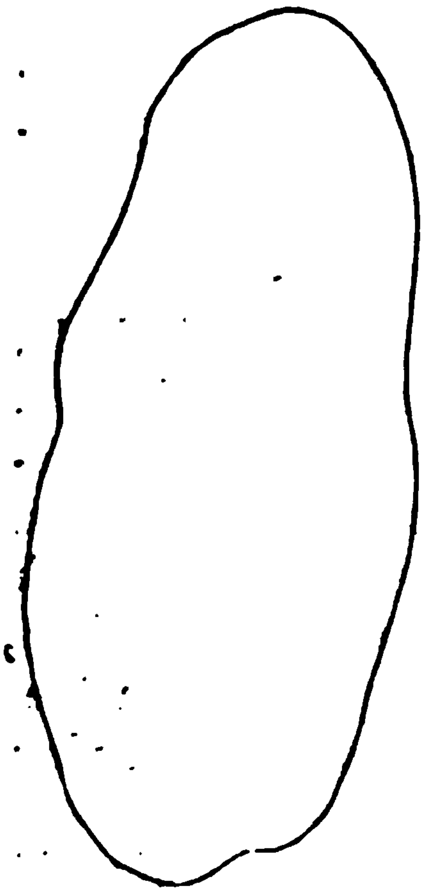
Biceps (long head).

Fig. 2.



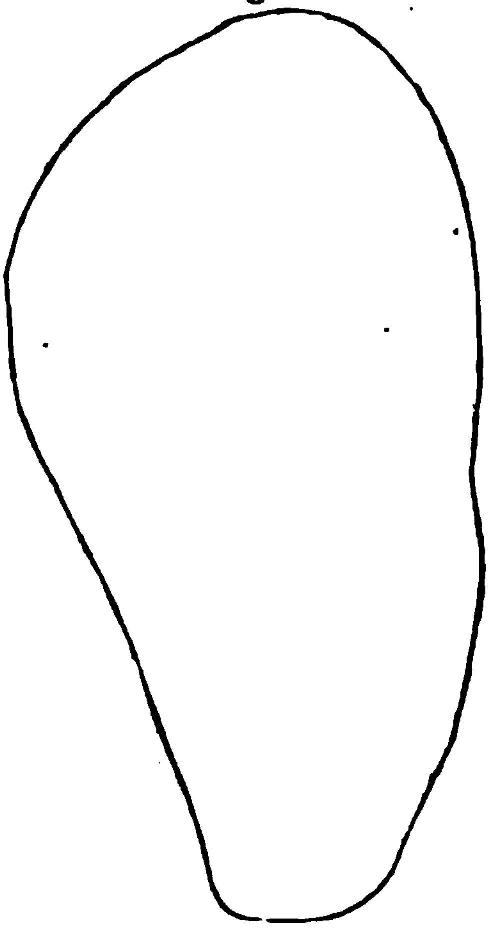
Biceps (short head).

Fig. 3.



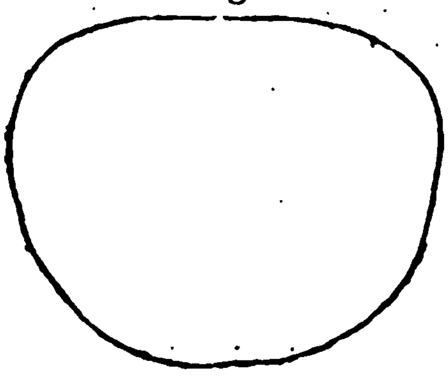
Semitendinosus.

Fig. 4.



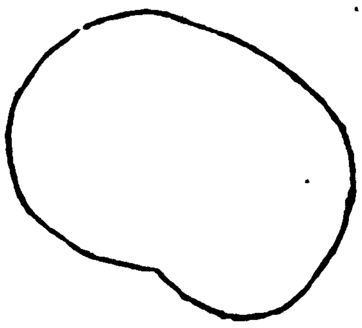
Semimembranosus.

Fig. 7.



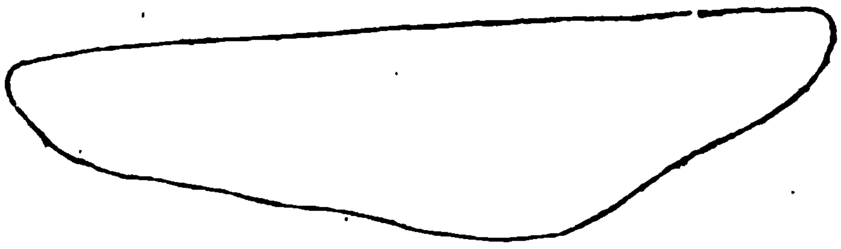
Flexor perforans. (Rhea.)

Fig. 8.



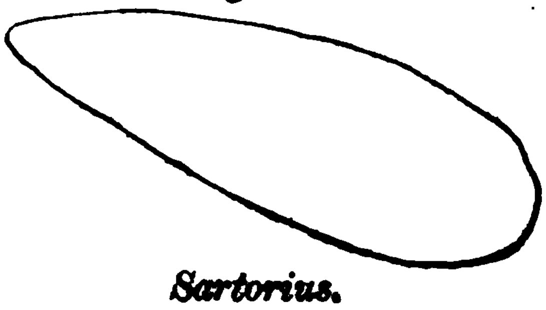
Flexor hallucis. (Rhea.)

Fig. 5.



Gracilis.

Fig. 6.



Sartorius.

and if too weak, it must either break, or be nourished up to the requisite degree of strength. It seemed to me desirable to prove this fundamental proposition in animal mechanics by direct observation; and I selected for this purpose the tendons in the leg of several of the large running birds (*Struthionidæ*); and always with the same result, viz.,

that *the cross sections of any two muscles tending to produce a similar effect are directly proportional to the cross sections of their tendons.*

I shall select as an example the case of the *flexor hallucis longus* and *flexor digitorum communis perforans* of the Rhea, whose tendons unite into a common tendon halfway down the posterior side of the *canneon* bone of the bird.

The cross sections of these two muscles are shown in the annexed figures, taken as in the human subject. (Figs. 7 and 8.)

The areas of these cross sections were found to be as 245 to 160; or the lesser was 65 per cent. of the greater.

Two equal lengths of the dried tendons were then weighed and found to be in the proportion of 845 to 495, which was assumed to be the proportion of their cross sections. The lesser of these numbers is 59 per cent. of the greater; a result that seems to be as near to the former result derived from the muscles, as can be expected in this class of experiments.

IV. "Observations on the Anatomy of the Thyroid Body in Man." By George W. Callender, Lecturer on Anatomy at St. Bartholomew's Hospital. Communicated by Mr. Paget, Received June 8, 1867.

(Abstract.)

Much doubt exists as to the earliest connexions of the thyroid body, whether it is developed, that is to say, with the membranous air-tube, or has a common origin with the thymus gland. There are no reliable observations as to the formation of the isthmus or as to the origin of the pyramid, so far, at least, as man is concerned, although, with reference to the isthmus, its absence in an entire class, that of birds, and the observations of Gray on the formation of the thyroid in the chick, countenance the supposition that it results from the growing together of two lateral masses.

In a human fœtus, between the seventh and eighth week, the thyroid body is closely connected with the trachea and with the lower edge of the larynx, and although consisting of but one piece is deeply notched, and thus looks as though made up of three separate lobes. It is quite distinct from the thymus, as may be further seen in the dissection of a fœtal rabbit or fœtal pig, in which, whilst firmly attached to the trachea, it has no kind of connexion with the thymus. In the human fœtus no distinct evidence of the thyroid appears to exist before the sixth week, up to which time it cannot, I believe, be isolated from the structures in front of the neck. It seems to come out from the blastema in the form of a mass in front of the trachea, and quickly acquires an imperfectly lobed appearance.

In the dissections referred to, the presence of a middle portion and its

equal development with the lateral lobes lead to the inference that this central part is present from the earliest period, and that the thyroid isthmus is not formed by a growing together of two distinct sidepieces. In examining the thyroid in fœtal dogs, cats, and hares, I have always found the middle portion equally developed with the side lobes, and bounded by notches which seem to define it from them. With the growth of the fœtus the central part appears to flatten, losing the rounded, lobular condition, and sometimes disappears. The isthmus is formed from the smaller, middle, division uniting the other two; but there may be an absence of isthmus through failure of this union, the middle portion joining the right or left lobe, or a small middle lobe may remain distinct from the other two. The pyramid is very commonly met with in the fœtus, and is clearly an outlying part of the body, sometimes represented by bud-like projections, sometimes consisting of a process which reaches to the hyoid bone. It is probable that these outgrowths from the fœtal thyroid often shrink and disappear with advancing years.

The dissections of the human fœtus lead to the following conclusions:—(1) The thyroid is developed in connexion with the air-tube, and has no relation with the thymus. (2) It does not consist of two separate lateral masses, and the isthmus is present from the first as a distinct central portion. (3) The pyramid is an outlying part of the body, presenting, during fœtal life, all possible variations as to shape and site.

V. "On the Physical Constitution of the Sun and Stars." By G. Johnstone Stoney, M.A., F.R.S., F.R.A.S., Secretary to the Queen's University in Ireland. Received May 15, 1867.

(Abstract.)

An attempt is made in the memoir of which this is an abstract to take advantage of the insight we have gained within the last few years into the molecular constitution of gases, and the laws which regulate the exchanges of heat that take place between bodies placed in presence of one another, and to apply these new materials to the interpretation of the phenomena of the photosphere of the sun, the appearances presented during total eclipses, and the information about both sun and stars given by the spectroscope.

In an inquiry like this, where we are obliged to put up with such proofs as the materials at our disposal can supply, we must be content to accept results of every variety of probability, from that degree, bordering upon certainty, which commands an unhesitating assent, to that of which the chief scientific value is that it prompts to further investigation and points out a path. Those who read the memoir itself will best judge of the probability of each conclusion from the proofs laid before them; but in this sketch of its contents it may not be useless to indicate what is the value put upon

each result by the author, since the proofs must in many cases be entirely omitted. It will be convenient to do this by numbers.

The probability 4, then, is to be understood to imply that the matter in hand appears to the author to be fully made out. He would, for example, assign this probability to the wave-theory of light, and to the main features of the theory of the molecular constitution of gases which have been worked out by Clausius and others within the last twenty years. The number 1 will be used where an hypothesis agrees so well with such of the phenomena as are known, that it is concluded that it must be either the true account of them, or bear some intimate relation to the true theory; 2 will indicate that we have good ground to conclude our hypothesis to be the true theory, although at the same time the evidence is too scanty or conflicting to free us from hesitation; 3 will indicate a proof so strong that we should be very much surprised if anything were eventually to disturb it; 4, as has been already stated, will mark a conclusion fully made out; and to complete the series, 5 may be used for that demonstrative proof of which few subjects of inquiry are susceptible.

Observations with the spectroscope have made known to us that the sun's outer atmosphere, that is, the part of the atmosphere which extends outside the photosphere, is a mixture of many gases, amongst which hydrogen, sodium, magnesium, calcium, chromium, manganese, iron, nickel, cobalt, copper, zinc, and barium—all of them permanent gases in consequence of the temperature—have been detected. Now it is shown to be a necessary consequence of the molecular constitution of gases that in such an atmosphere, decreasing in temperature from within outwards, the various constituent gases are not everywhere equally mixed, but that in the upper regions those which have the lightest molecules rise the furthest, so that the gases overlap one another in the order of the masses of their molecules (probability 5). It also follows from a consideration of the vapour-densities and atomic weights of the chemical elements, with probabilities which range from 4 to 1, that those which are present in the sun's atmosphere have molecules with masses increasing in the order in which their names have been printed above, the molecules of hydrogen being the lightest. This, then, is the order in which the boundaries of these gases would be met with in descending from the surface of the sun's atmosphere downwards.

This result is abundantly confirmed, and in its main features raised to probability 4, by observations with the spectroscope. Each constituent of the solar atmosphere is opake to those rays which it emits when incandescent, and which constitute its spectrum. In this way all the light of these particular wave-lengths which has been emitted, either by the photosphere, or by the lower and more intensely heated strata of a gas in the solar atmosphere, is stopped in its passage outwards, and the gas substitutes for it the much more subdued light which emanates from its own upper and therefore coolest stratum. Now if the view enunciated in the last paragraph be true, these outer layers of the respective gases, from which the rays as

we see them come, must be at very various temperatures, that of hydrogen being the coldest, and the others in order after it. This is precisely in conformity with the observations. The rays of hydrogen, sodium, and magnesium emanate from a region so cold that the lines of these elements in the sun's spectrum are intensely black in whatever part of the spectrum they may occur; in other words, the light proceeding from the upper layers of these gases is so feeble that it is not in any perceptible degree luminous when placed in contrast with the intense background of light from the photosphere. On the other hand, calcium, iron, and the rest, while they produce only black lines in the violet and indigo, give rise to lines which are sensibly less dark in the blue, and to lines which emit a still more considerable amount of light in the green, yellow, orange, and red, those colours in which a body gradually heated begins to glow.

A detailed scrutiny of the lines emitted by the various gases leads to several interesting results. Hydrogen and iron are the two most abundant constituents of the sun's outer atmosphere, and play in it the same part which nitrogen and oxygen do in the earth's. There is but the merest trace of sodium present. The other gases are met with in intermediate quantities. Again, barium cannot have a vapour-density so high as would appear as first from its atomic weight, and therefore probably belongs to the same class of elements as cadmium and mercury, which have vapour-densities half of what correspond to their atomic weights. To these several results we may attribute the probability 3.

The photosphere consists of two strata which may be distinguished. The outer of these is shown to be cloud in the ordinary sense of the word, that is, solid or liquid matter in a state of minute division, and denser than the part of the atmosphere in which it is dispersed (probability 3). This cloud is precipitated from its vapour by the chill produced by its own abundant radiation towards the sky, a chill which constitutes the shell of clouds a surface of minimum temperature considerably cooler than either the layer above it or the layer beneath (probability 3). The hotter layer, which is outside the luminous clouds, seems to have a depth somewhat greater than the length of the earth's radius (probability 2). Just outside it there is a second layer of luminous clouds, but so excessively thin that they can be seen only during a total eclipse, on which occasions a portion of them has been seen under the form of two arcs of cloud extending for some distance on either side of the points of first and last contact, where alone a sufficiently low part of the sun's atmosphere was disclosed (probability 3). Above these there soar other clouds raised by causes which will be referred to further on.

About the middle of the hot stratum over the photosphere there is a surface of maximum temperature, outside which the temperature decreases almost continuously to the limit of the iron atmosphere. A little outside this there is a second very feeble maximum, the temperature of which falls short of the heat of the flame of a Bunsen's burner; and outside this, through the immense height which is tenanted by sodium, magnesium, and

hydrogen alone, the temperature goes on decreasing till it becomes excessively cold. These results are made out with probabilities 2 and 3.

Within the luminous clouds the temperature very rapidly waxes, and the density, too, appears to receive a nearly sudden increase. All gases with a vapour-density more than about eighty times that of hydrogen are imprisoned within the shell of clouds by the comparative chill which there prevails, cooperating with the intensity of the force of gravity exerted by the sun. Between the film of clouds and the stratum immediately beneath there are violent motions of convection, which both carry up fresh vapour to be condensed into cloud, and carry down the cloud into a region where it becomes mist and rain. It is convenient to restrict the word *cloud* to cloud in that situation in which it can form, giving the names mist or rain to the cloud when carried down, either by currents of convection or by subsidence, into a position from which there is not that abundant radiation towards the sky which is essential to its forming. The clouds, in this restricted sense of the term, are everywhere of a gauze-like transparency to admit of the copious radiation towards the sky which is requisite; and this enables spectators upon the earth to see through them the light emitted by the mist and rain beneath. This mist and rain seem everywhere, except in the solar spots, to be dense enough to be opake, and therefore emit the maximum light corresponding to their temperature. This temperature is higher than that of the clouds, and accordingly the mist and rain constitute a background brighter than the luminous clouds.

Hence the finely-granulated appearance of the surface of the sun, the currents of convection creating a kind of honeycombed structure in the stratum of clouds; the ascending currents carrying up hot vapours in which only excessively thin cloud can form, since under these unfavourable circumstances its lowest parts cannot tolerate even the slight obstruction to their radiating freely which a cloud of the average density would offer; and, on the other hand, the descending currents carrying down those portions which by prolonged radiation have cooled down abnormally, and thus become both more opake by the condensation of more cloud, and less bright. Those portions which by the most persistent radiation cool down the most, seem to furnish the very dark specks which have been taken notice of by observers.

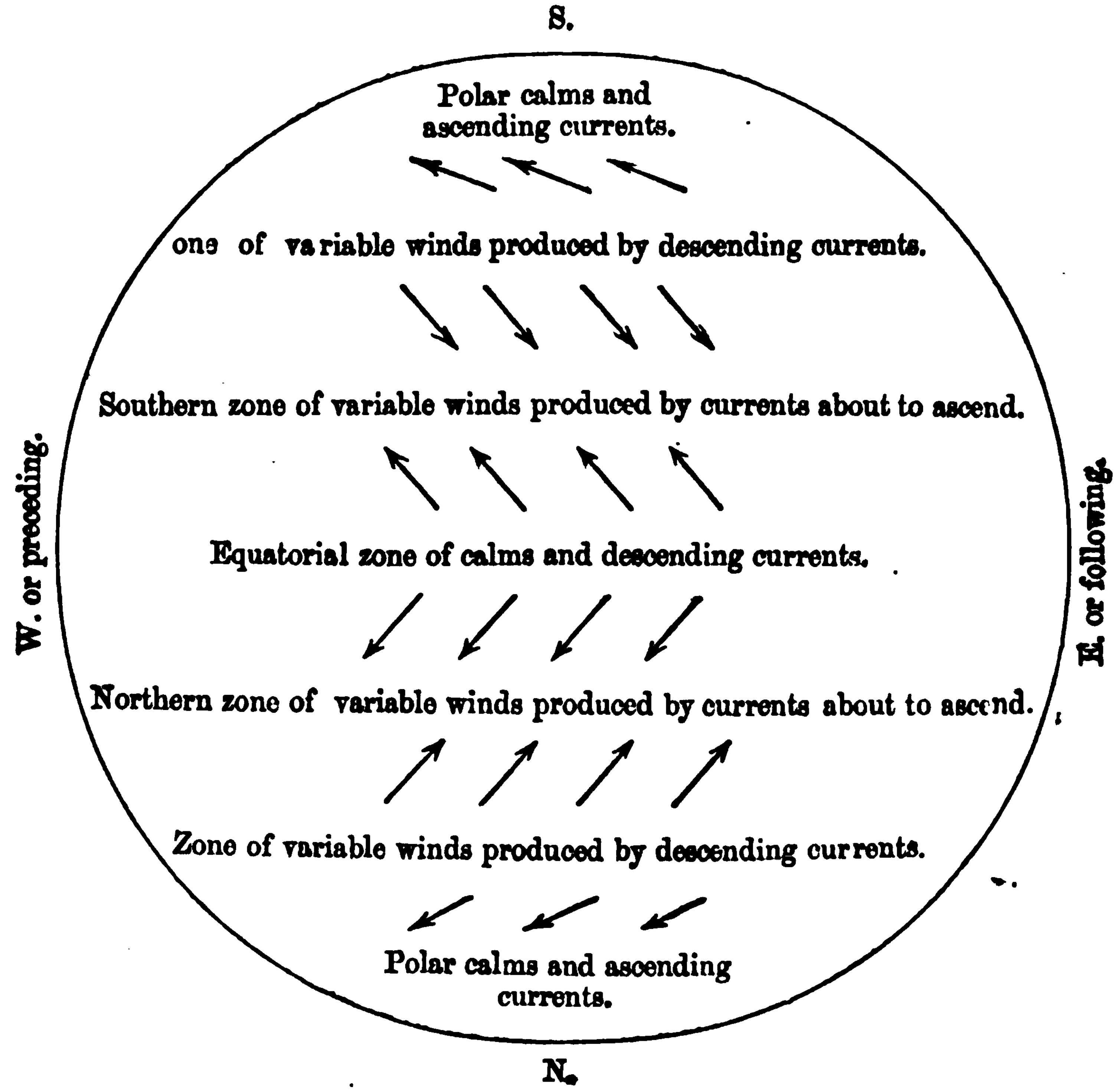
Hence also arises the gradation of light which is observed upon the sun's disk. In the middle of the disk we look vertically through the honeycombed structure which has been described, and see through it the brighter background almost without any intervening obstruction. But as we turn our eyes towards the margin of the disk, we look more and more obliquely across the columns, which progressively intercept increasing quantities of the brighter light from beyond, and substitute for them their own feebler radiations.

If by disturbances in the atmosphere the hotter stratum on either side is made in certain places to encroach upon the luminous clouds, they are unable to maintain in this situation as low a temperature as elsewhere, and

therefore become abnormally thin. If this process is not carried so far as to put a stop to the incessant rain beneath the clouds, their increased transparency will give rise to a facula when the phenomenon takes place on a large scale, and to the coarsely mottled appearance of the photosphere where it presents itself in smaller patches. Hence we see why a facula retains its brightness up to the margin of the sun's disk, a phenomenon which is inconsistent with the usually received hypothesis that the gradation of light on the sun's disk is due to the absorption of the outer atmosphere. If the rain also cease we have the penumbra of a spot; if the cloud itself is dissolved away, we have its umbra.

The dark body which is disclosed in the umbræ and penumbræ of spots must be either an untarnished ocean of some highly reflecting opake substance, or a cloud of some transparent material which scatters light abundantly. Both hypotheses are fully considered.

To most of the foregoing conclusions relating to the photosphere and the adjoining parts we may safely accord the probability 3.



We have strong reasons for suspecting that the luminous clouds consist, like nearly all the sources of artificial light, of minutely divided carbon; and that the clouds themselves lie at a very short distance above the situation

in which the heat is so fierce that carbon, in spite of its want of volatility, and of the enormous pressure to which it is there subjected, boils. The umbra of a spot seems never to form unless when the region in which carbon boils is carried upwards, or the hot region above the clouds is carried downwards, so as to bring them into contact, and thus entirely obliterate the intervening clouds. It is, however, not safe to attribute to the results stated in this paragraph a probability of more than 1.

The trade-winds which blow over the surface of the photosphere are also inquired into. These seem to arise, as Sir John Herschel suspected, from the oblate form of the sun causing a difference in the escape of heat from his poles and equator. There are ascending currents at the poles, descending currents all round the equator. This produces a region of equatorial calms bordered on either side by zones, in the northern of which south-east trades prevail, and in the southern, north-east. These are succeeded by variable winds in the regions of spots, beyond which the polar current blows over the surface of the photosphere in the form of a north-west trade in the northern hemisphere, and a south-west trade in the southern. In the region of spots, both the polar and equatorial currents make their way to a higher level, and in doing so heave up into a colder situation considerable portions of the upper layer of excessively thin cloud, that which is seen only during eclipses. This, though it may at first take place comparatively gently, will be succeeded by a violent upward motion, because the cloud when raised to a cool region will retain a temperature bordering upon that of the photosphere. When this occurs it will both produce the phenomenon of overhanging clouds seen during eclipses, and give rise to a violent cyclone in the regions beneath, immediately over the photosphere. There is no other part of the sun upon which these conditions prevail: hence the limitation of spots to two bands parallel to the equator. To these results we may assign the probability 2.

In the next branch of the inquiry we are obliged to have pretty free recourse to speculation; and the results, though there is much to be said for them, must be received with the caution which becomes us when we are not at liberty to award a probability higher than 1. We are forced to invoke an external agent to account for the periodicity of the spots; and that which is submitted as apparently the most probable, is a swarm of meteors like those which visit the earth in November every thirty-third year, but extended into a much longer stream. These while they pass through the sun's atmosphere would warm the upper regions above his equator, and thus tend to enfeeble the causes which produce the trade-winds. Hence upon each such visit, the trade-winds, the storms which result from them, and the spots which these occasion would all be moderated. It is remarkable that this hypothesis accounts also for the fact that spots prevail more in one hemisphere than the other, inasmuch as the meteors must act more on one hemisphere than the other, and lessen in it the causes that produce spots, unless we make the highly improbable supposition that the axis

major of the orbit of the meteors lies just along the line in which its plane intersects the plane of the sun's equator. It is also very remarkable that the interval of time in which the spots go through their mutations, which we must of course adopt as the periodic time of the meteors in their orbit, assigns to them an aphelion distance outside and close to the orbit of one of the principal planets, Saturn. There is therefore very considerable ground to suspect that there is such a swarm of meteors which was diverted into the solar system by Saturn * at no very remote epoch—just as our November meteors were brought in by the planet Uranus in the year 126 of the Christian era.

Finally, it is shown that an hypothesis which has found much and deserved favour of late years, that the heat expended by the sun is continually restored to him by the falling in of meteors which had been circulating round him, is no longer tenable.

The second part of the memoir treats of other stars. The differences in their appearances are found to depend mainly on differences in the force of gravity exerted at their surfaces. Where gravity on a star is feebler than on the sun, either from the mass of the star being less, or from its being so dilated by heat that its outer parts are further removed from its centre, gases which by reason of the mass of their molecules are imprisoned within the photosphere of the sun, will, when less attracted downwards, be able to stand the coolness of the shell of clouds and pass beyond them. Thus mercury, antimony, tellurium, and bismuth, all of which have too high a vapour-density to exist in the sun's outer atmosphere, show themselves in that of Aldebaran. Again, in these stars all the gases of the outer atmosphere expand until their upper layers, those from which their spectral lines issue, are cooler than on the sun. These spectral lines will accordingly be darker than on the sun, and as this will tell with most effect on the blue end of the spectrum, it will render the light from these stars ruddy.

On the other hand, those stars which, either from being of greater mass than the sun, or from being less hot in their internal parts, attract down the gases of their outer atmospheres with more force, constitute the class of intensely white stars with a somewhat violet tinge, of which Sirius and *α* Lyræ are examples. Several of the substances which in the sun's spectrum give rise to faint lines, are on such stars confined within the photosphere; and the lowest temperature which others of them can withstand, is by reason of the force with which they are attracted downwards, hotter than the corresponding temperatures of the sun. Hence the substances which on the sun cause his numerous dark lines—sodium, magnesium, calcium, chromium, manganese, iron—produce in the spectrum of the star

* The attraction of Jupiter would also have been competent to divert a cluster of meteors into an orbit of the requisite form and dimensions; but the situation of the orbit would in that case have caused the meteors to cross the path of Jupiter, so that the planet would have acted ever since as a powerful dispersing agent, and it does not seem likely that such an influence has been in operation.

lines equally numerous, but faint. There is but one exception to this. Hydrogen has a molecular mass so amazingly low (one twenty-third part of the mass of molecules of sodium, the nearest to it in this respect of the known constituents of stellar atmospheres), that there is probably no star which can exert a force of gravity so powerful as to compel hydrogen to limit itself to temperatures which show in any part of the spectrum a perceptible degree of brightness when placed upon the background of the photosphere. In all stars accordingly in which hydrogen appears at all, the four hydrogen lines are found intensely black.

We see, then, why solitary stars are found of some particular colours only. Stars which exert upon their outer atmospheres a force of gravity as great or greater than the sun's are white: those on which gravity is a less force are of some ruddy tint,—yellow, orange, or red. The foregoing results are adjudged to be of probability 4, that is, fully made out.

Those stars in which the force of gravity is *very much* less than on the sun appear to form a distinct subclass. The four hydrogen lines are not found in them, and at the same time new spectral lines, arranged in bands each of which is closely ruled and fades off on the less refrangible side, make their appearance. May we not here venture the suspicion that when gravity upon a star is below a certain limit, such conditions prevail as compel the hydrogen which would otherwise be free, to enter into combination with some other element of low vapour-density; and that the resulting compound emits that spectrum of the First Order, as Plücker has called it, which we see?

To account for the colours of the companions of double stars we are again forced to enter upon speculative ground. If the sky be peopled with countless multitudes of dark stars, which as well as the small number that are visible, move only in virtue of their mutual attractions, it cannot be an absolutely unusual occurrence for two stars to come into collision. Whenever this happens, either the two stars emerge from the frightful conflagration which would ensue as one star, or, if they succeed in disengaging themselves, they will be found after the catastrophe moving in new orbits. If their previous courses had been parabolic, it can be shown that the new relative orbit will be elliptic. Hence they will return to the charge again and again, and at each perihelion passage there will be a fresh modification of the orbit. It is shown that these modifications will in some instances be such that the perihelion distance will be constantly on the increase, so that the stars will, in their successive perihelion passages, climb as it were asunder through one another's atmospheres. And the distance to which they will ultimately withdraw before they separate will of necessity be immense, since their atmospheres must have been dilated to a vast size by the friction to which they have been subjected. As the stars recede from one another the amount of heat which they generate at each perihelion passage is progressively less and less, until at length the atmospheres of the stars shrink in the intervals between two perihelion passages more than they ex-

pand when the brush takes place. When this happens the final separation of the two stars is imminent, and a new double star is on the point of being permanently added to the sky.

The astonishing appearances witnessed last year in T Coronæ seem to receive an easy explanation upon this hypothesis. They are exactly what we should expect upon the occurrence of one of the last perihelion passages that take place before two stars which are in the state of transition into a double star finally separate. The outer parts of the atmospheres becoming engaged would raise to incandescence the region in which hydrogen only is found, thus transforming what had previously been its four dark lines into intensely bright lines. At the same time the strata that lie further down would be very sensibly heated, though not to incandescence—quite enough, however, to lessen temporarily in a very material degree the extent to which they at other times subdue the light of the photospheres. This extent would of necessity have been very great, inasmuch as the enormous dilatation of the atmospheres must greatly enfeeble the force of gravity upon the outer strata of both stars.

Again, it follows as a consequence of this hypothesis that the circumstances which most favour the formation of a double star are when the two bodies that come into collision are of nearly equal mass. Such cases must be rare; but when they do occur, there is a very high probability that the issue will be a double star. This appears to accouut for the fact that a very remarkable proportion of double stars have constituents of nearly the same magnitude.

Another consequence is that when the stars are very unequal, the companion will, as it plunges over and over again through the atmosphere of the primary, be gradually deprived of several of its lighter gases; so that when it finally gets clear it will not emit the principal spectral lines of a solitary star, but others which emanate from denser gases. This probably accounts for the blue, violet, and green colours which are found in the minute companions of double stars.

Another consequence is that the orbits of double stars will almost always have a considerable ellipticity.

Another consequence is that the conditions are likely not unfrequently to arise which would separate the companion into two or more fragments; and that when this happens, the separate pieces will pursue paths which are distinct from one another and not far apart. This seems to account for such systems as γ Andromedæ.

When the same conditions act with unusual violence they would probably break up the companion into numerous fragments; and it is remarkable that they would at the same time be likely to cause the primary to throw off a number of rings. The fragments and the rings would move all in the same direction and nearly in the same plane, and each fragment would rotate rapidly in the direction in which it revolves in its orbit. When the fragments, as must generally happen, are of inconsiderable mass

their orbits would be almost certain to degrade from ellipses into circles before they got quite clear of the primary. Some would probably be found, when this happens, at the distance of the rings, others within the surface of the primary, none beyond both. Those within the surface of the primary would fall into him and be lost. But one that lay within a ring would gather by its attraction the ring round itself, and so become covered with an immense atmosphere with which it would continue to rotate while advancing in its circular orbit. If this rotation were sufficiently swift, the new planet would throw off rings which might afterwards condense into satellites, with this peculiarity, that they would always keep the same face turned towards the planet, and revolve round it in the same direction and nearly in the same plane in which the planet revolves round its sun.

The speculative element in this hypothesis is so considerable that perhaps we may not prudently yield to it a probability higher than 1. But an hypothesis which carries up so many of the main phenomena of nature to a single source, and which only asks us to admit what is not antecedently improbable, that the number of incandescent stars is but a small proportion of all that exist, seems nevertheless to deserve to be stated.

VI. "Researches on the Hydrocarbons of the Series $C^n H^{2n+2}$.—No. III." By C. Schorlemmer. Communicated by Prof. G. G. Stokes, Sec. R.S. Received May 15, 1867.

1. *Di-Isopropyl*, $C_6 H_{14}$.

Iodide of isopropyl is not perceptibly acted upon by sodium even if the liquid is heated to the boiling-point; but if anhydrous ether perfectly free from alcohol is added, a reaction soon commences without application of external heat; the liquid becomes warm, and the iodide is decomposed with formation of iodide of sodium. The chief products of this reaction are, (1) propylene, from which bromide of propylene was obtained by passing the gases which are evolved through bromine; (2) a gaseous hydrocarbon, which is not absorbed by bromine and which burns with a luminous flame, probably consisting of hydride of propyl; and (3) a liquid hydrocarbon, which, according to its composition and mode of formation, must be considered as di-isopropyl. By the following method I obtained the largest yield of this liquid. A flask holding about 250 cub. centims. was half filled with iodide of isopropyl (which had been prepared by acting with hydroiodic acid upon glycerin); an equivalent quantity of sodium cut into thin pieces was added, upon this a layer of pure ether was poured, and the flask quickly connected with the lower end of a Liebig's condenser. Where the two liquids meet, a brisk reaction soon sets in; the escaping gases carry off a large quantity of the liquid, chiefly of the more volatile ether, and it is therefore necessary to keep the condenser as cold as possible. The reaction goes on generally quietly until the greatest portion of the iodide is decomposed; if it stops after a short time, gentle heat has to be applied as long as gas is evolved. After the reaction is over, the

flask is heated in an oil-bath, and the liquid contents are distilled off. The distillate is fractionated several times, and the portion boiling between 50° and 70° C. collected separately. This consists chiefly of di-isopropyl, but also contains ether, undecomposed iodide of isopropyl, and may also contain diallyl if the iodide was not quite pure. In order to remove these admixtures, the liquid is repeatedly shaken with concentrated sulphuric acid as long as heat is evolved, then rectified, and the distillate treated with a mixture of strong nitric and sulphuric acid as long as iodine separates, then washed, dried, and rectified over potassium.

Di-isopropyl is a colourless mobile liquid, the odour of which cannot be distinguished from that of hydride of hexyl, and which boils constantly at 58° C. The specific gravity was found to be

at 10° C.=0·6769,
at 17°·5 C.=0·6701,
at 29° C.=0·6596.

The analysis gave the following numbers:—

0·2390 of substance yielded 0·7315 of carbonic acid and 0·3525 of water,

		Calculated.	Found.
C_6......	72	83·72	83·5
H_{14}	14	16·28	16·4
	86	100·00	99·9

The formula for isopropyl now generally accepted is $\begin{cases} CH \\ CH \\ CH_3 \end{cases}$, and the constitution of di-isopropyl may therefore be expressed by the following formula:—

$$C_2\begin{cases} H \\ CH_3 \\ CH_3 \\ \overline{CH_3} \\ CH_3 \\ H \end{cases}.$$

This hydrocarbon can be considered as hydride of ethyl, in which 4 atoms of hydrogen have been replaced by methyl and might be called, by accepting the nomenclature for hydrocarbons proposed by Hofmann*, tetramethyl-ethan.

Chlorine attacks this hydrocarbon very easily in the cold, and if the action is stopped before the whole has been acted upon, the principal substitution-product consists of the chloride $C_6 H_{13} Cl$, a colourless liquid which boils constantly at 122° C., and very closely resembles its isomer, chloride of hexyl, the boiling-point of which is 125° C. according to a determination made with the same thermometer. The specific gravity of this chloride is

at 14° C. = 0·8943,
at 22° C. = 0·8874,
at 34° C. = 0·8759.

* Proc. Roy. Soc. vol. xv. p. 57.

The following data give the results of the analysis :—

0·3780 of substance gave 0·4505 of chloride of silver and 0·0023 of metallic silver.

Calculated for $C_6H_{13}Cl$.	Found.
29·46 per cent. Cl.	29·7 per cent. Cl.

If iodine is present, the action of the chlorine is quite different. No trace of a monochloride is formed; the chief product consists of bichlorinated di-isopropyl, $C_6H_{12}Cl_2$, a solid substance, besides a smaller quantity of high boiling products, which are very rich in chlorine. From those the solid chloride may be easily separated either by distillation with water, the steam carrying the solid substance very easily over, or by cooling the mixture of the substitution-products and pressing the crystals which separate between blotting-paper. This compound forms white crystals which smell strongly of camphor, and, exposed to the air, soon volatilize at the common temperature; heated in an open tube they sublime below their fusing-point; in a closed tube they melt at about 160°.

The analysis gave the following results :—

(1) 0·2781 of substance gave 0·4795 of carbonic acid and 0·2030 of water.

(2) 0·1011 of substance gave 0·1846 of chloride of silver and 0·0015 of metallic silver.

(3) 0·1756 of substance gave 0·3136 of chloride of silver and 0·0036 of metallic silver.

(4) 0·1680 of substance gave 0·3040 of chloride of silver and 0·0103 of metallic silver.

(5) 0·1415 of substance gave 0·2515 of chloride of silver and 0·097 of metallic silver.

	Calculated.		Found.				
			(1)	(2)	(3)	(4)	(5)
C_6 .	72	46·45	46·81				
H_{12} .	12	7·74	8·11				
Cl_2 .	71	45·81	..	45·7	44·9	46·6	46·2
	155	100·00					

The higher chlorinated products boil under decomposition between 200° and 300°; the quantity which I obtained was too small to attempt to separate them into definite products

Di-isopropyl is slowly oxidized if it is heated with a concentrated solution of dichromate of potassium and sulphuric acid, a large quantity of carbonic acid being evolved. In order to oxidize 10 grammes of the hydrocarbon it took a week; the liquid was distilled off every day, and the slightly acid distillate neutralized with carbonate of sodium, and thus a sodium-salt was obtained which on recrystallization gave a crop of crystals, whose habitus and reactions were found to coincide with acetate of sodium.

The small quantity of mother-liquor from these crystals was precipitated with nitrate of silver, and the precipitate crystallized from boiling water.

0·2120 of this silver-salt gave 0·1374 of silver, or 64·72 per cent.; acetate of silver contains 64·67 per cent. of silver.

By oxidizing di-isopropyl with chromic acid the only products formed are therefore carbonic acid and acetic acid.

2. *Amyl-isopropyl*, $C_8 H_{18}$.

This hydrocarbon was obtained by acting with sodium and ether upon a mixture of iodide of isopropyl and iodide of amyl. The reaction sets in without applying heat, and is rather violent in the beginning, and it is therefore necessary to keep the flask first immersed in cold water; but to complete the decomposition the mixture has to be heated. When all the sodium has disappeared, the contents of the flask are distilled from an oil-bath, and the distillate is heated with sodium as long as iodide of sodium is formed. Ether and non-attacked iodides are then removed by treating the liquid with strong acids as described above, and thus a mixture of di-isopropyl, amyl-isopropyl, and di-amyl is obtained, from which these hydrocarbons can easily be separated by fractional distillations.

Amyl-isopropyl is a colourless liquid boiling at 109°–110°; its specific gravity was found

at 16°·5 C. = 0·6980,
at 49° C. = 0·6712.

The results of the analysis are,—

0·2040 of substance gave 0·6285 of carbonic acid and 0·2900 of water.

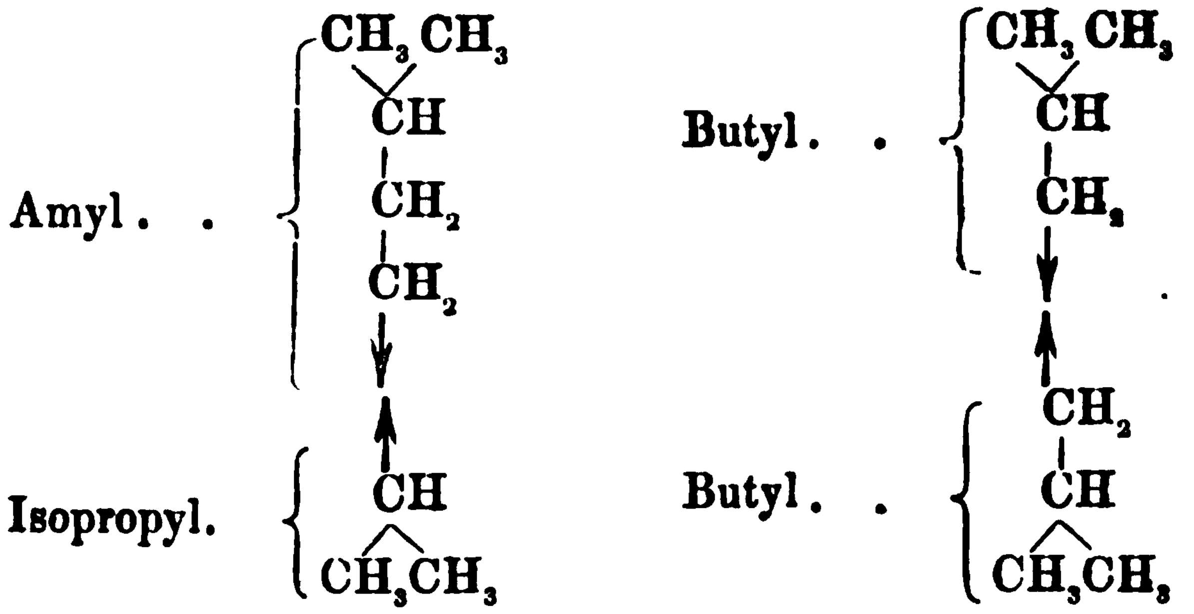
	Calculated.		Found.
C_8	96	84·2	84·0
H_{18}	18	15·8	16·1
	114	100·0	100·1

The constitution of this hydrocarbon can be expressed by the formula

$C \begin{cases} H \\ CH_3 \\ CH_3 \\ C_5 H_{11} \end{cases}$, and it might therefore be called dimethyl-amyl-methan. Its boiling-point and its specific gravity coincide perfectly well with those of dibutyl, which according to Kopp boils at 109°, and has at 16°·4 the specific gravity 0·7001 *. I believe that these two hydrocarbons are identical; for Erlenmeyer stated a short time ago in a preliminary note, that he has found that the butyl-alcohol formed by fermentation is methyl-alcohol, in which one atom of hydrogen in the methyl is replaced by isopropyl, and that fermentation amyl-alcohol is ethyl-alcohol, in which also one atom of hydrogen in the methyl group is replaced by isopropyl †. If this view is correct, amyl-isopropyl must be identical with dibutyl, as the following formulæ clearly show:—

* Ann. der Chem. und Pharm. vol. xcv. p. 336.

† Zeitschrift für Chem. N. F. vol. iii. p. 117.



Chlorine converts amyl-isopropyl easily into the chloride $C_8 H_{17} Cl$, a colourless liquid which boils at 165°, and smells faintly of oranges, just as is its isomer, chloride of octyl. Its specific gravity is

at 10°·5 = 0·8834,
at 36° = 0·8617.

0·2480 of this chloride yielded 0·2380 of chloride of silver and 0·0015 of metallic silver.

Calculated for $C_8H_{17}Cl$.	Found.
23·90 per cent. Cl.	23·9 per cent. Cl.

When chlorine acts upon amyl-isopropyl, a mixture of chlorine substitution-products is formed, from which I did not succeed in obtaining definite compounds. On repeated fractional distillation the largest portion passes over between 170° and 180° as a colourless liquid smelling of oranges.

0·2815 of this substance gave on analysis 0·2772 of chloride of silver, which corresponds to 24·36 per cent. of chlorine. This liquid appears therefore to be a mixture of isomeric chlorides of the formula $C_8 H_{17} Cl$.

A solution of chromic acid attacks amyl-isopropyl very slowly; the only oxidation-products which are formed are carbonic acid and acetic acid, from which latter the sodium-salt was prepared, and this was converted into the silver-salt.

0·1985 of this silver-salt contained 0·1291 of silver, or 65·0 per cent., whilst acetate of silver contained 64·67 per cent.

From the commencement of my researches on the hydrocarbons of this series I have tried to obtain definite and characteristic oxidation-products; but the results of these experiments are as yet but very incomplete. I have chiefly studied the action of oxidizing agents upon hydride of hexyl. This hydrocarbon is acted upon by a concentrated solution of chromic acid in the same manner as the two hydrocarbons described above; the only products formed are carbonic acid and acetic acid. A mixture of manganic peroxide and sulphuric acid, as well as a solution of permanganic acid, give only carbonic acid. Nitric acid also forms carbonic acid by boiling it or heating it in sealed tubes with hydride of hexyl; besides, a small quantity of a solid acid, very likely belonging to the oxalic-acid series, is produced. I

have not yet obtained this body in sufficient quantity, as it is only very slowly formed. I hope, however, to find a method to produce it in larger quantities, and also to obtain characteristic oxidation-products of the different hydrocarbons.

VII. "Researches into the Chemical Constitution of Narcotine and of its Products of Decomposition."—Part II. By A. Matthiessen, F.R.S., and G. C. Foster, B.A. Received May 23, 1867.

(Abstract.)

In this memoir the following reactions have been studied:—

1. *The Action of Hydrochloric and Hydriodic Acids on Opianic Acid.*

When strong hydrochloric or hydriodic acid acts at 100° for some time on opianic acid, iodide or chloride of methyl is evolved and a new acid formed,

$$C_{10}H_{10}O_5 + HCl = C_9H_8O_5 + CH_3Cl.$$

We propose to call this acid methyl nor-opianic acid, as it stands intermediate between opianic acid and the normal opianic acid:—

Normal opianic acid	$C_8H_6O_5$
Methyl nor-opianic acid................	$C_9H_8O_5$
Opianic acid or dimethyl nor-opianic acid..	$C_{10}H_{10}O_5$

The new acid is soluble in cold water, but much more so in hot, from which it crystallizes on cooling with 2½ molecules of water. Like hypogallic acid it strikes a dark blue with sesquichloride of iron; but on addition of ammonia in excess, a light-red solution is produced, differing, therefore, from the hypogallic-acid blue, with which ammonia becomes blood-red. From the analysis of the silver-salt it appears that methyl nor-opianic acid is monobasic.

2. *The Action of Hydrochloric and Hydriodic Acids on Meconin.*

When meconin is heated with strong hydrochloric or hydriodic acids at 100° for some time, it is split up into chloride or iodide of methyl and an acid of the composition $C_9H_8O_4$. The reaction is

$$C_{10}H_{10}O_4 + HCl = C_9H_8O_4 + CH_3Cl.$$

This new acid we may call *methyl nor-meconic acid,* as it stands between meconin and normal meconin:—

Meconin..................................	$C_{10}H_{10}O_4$
Methyl nor-meconin or methyl nor-meconic acid.	$C_9H_8O_4$
Normal meconin........................	$C_8H_6O_4$

Methyl nor-meconic acid is soluble in cold, but much more so in hot water; it is easily soluble in alcohol, and slightly so in ether. It reduces solutions of silver-salts in the cold, and behaves with sesquichloride of iron exactly like methyl nor-opianic acid. From the analysis of the barium-salt, methyl nor-meconic acid is monobasic.

3. *The Action of Hydrochloric and Hydriodic Acids on Hemipinic Acid.*

The action of hydriodic acid on hemipinic acid has been already described in our former communication. The reaction which takes place was found to be

$$C_{10}H_{10}O_6 + 2HI = CO_2 + 2CH_3I + C_7H_6O_4.$$

The body $C_7H_6O_4$ we called hypogallic acid.

It was also mentioned that when hydrochloric acid acts on hemipinic acid the following reaction takes place:—

$$C_{10}H_{10}O_6 + HCl = CO_2 + CH_3Cl + C_8H_8O_4.$$

The formula $C_8H_8O_4$ has been confirmed by further analyses, and from the analysis of its silver-salt we have shown it to be a monobasic acid. This acid may be called methyl-hypogallic acid, as it contains one molecule of methyl more than the hypogallic acid, and may be converted into that acid by the prolonged action of hydrochloric acid on it.

4. Whilst experimenting with hemipinic acid we found that this acid may crystallize in different forms. The crystals were found to contain different amounts of water; thus when it crystallizes from a dilute solution by spontaneous evaporation, the crystals contain half a molecule of water; when from a supersaturated solution, they contain one molecule; and lastly, when crystallized in the ordinary way by cooling a hot solution, they contain two and a half molecules.

From the experiments here, as well as those in our former paper, it appears that the following compounds derived from opianic acid will be found to exist:—

$C_{10}H_{10}O_4$ Dimethyl nor-meconin (ordinary meconin).	$C_{10}H_{10}O_5$ Dimethyl nor-opianic acid (ordinary opianic acid).	$C_{10}H_{10}O_6$ Dimethyl nor-hemipinic acid (ordinary hemipinic acid).
$C_9H_8O_4$ Methyl nor-meconin.	$C_9H_8O_5$ Methyl nor-opianic acid.	$C_9H_8O_6$ Methyl nor-hemipinic acid.
$C_8H_6O_4$ Nor-meconin.	$C_8H_6O_5$ Nor-opianic acid.	$C_8H_6O_6$ Nor-hemipinic acid.
$C_8H_8O_4$	$C_8H_8O_3$	$C_8H_8O_4$ Methyl hypogallic acid.
$C_7H_6O_2$	$C_7H_6O_3$	$C_7H_6O_4$ Hypogallic acid.

Of the above, the following have been prepared:—

1. $C_{10}H_{10}O_4$, $C_{10}H_{10}O_6$ by the action of potash on opianic acid; thus,

$$2C_{10}H_{10}O_5 = C_{10}H_{10}O_4 + C_{10}H_{10}O_6.$$

2. $C_9H_8O_4$ by the action of hydrochloric and hydriodic acids on meconin; thus,

$$C_{10}H_{10} + HI = C_9H_8O_4 + CH_3I.$$

3. $C_9H_8O_5$ by the action of hydrochloric or hydriodic acids on opianic acid; thus,

$$C_{10}H_{10}O_5 + HI = C_9H_8O_5 + CH_3I.$$

4. $C_8H_8O_4$ by the action of hydrochloric on hemipinic acid; thus,

$$C_{10}H_{30}O_6+HCl=C_8H_8O_4+C\,O_2+C\,H_3Cl.$$

5. $C_7H_6O_4$ by the action of hydriodic acid on hemipinic acid; thus,

$$C_{10}H_{10}O_6+2H\,I=C_7H_6O_4+C\,O_2+2C\,H_3I.$$

In the second part of the paper the properties and the preparation of a new base prepared from narcotine are described. When narcotine is heated for from six to eight days with strong hydrochloric acid at 100°, two molecules of chloride of methyl are given off, and the chloride of the new base formed. The reaction which takes place is

$$C_{22}H_{23}N\,O_7+2HCl=C_{20}H_{19}N\,O_7+2C\,H_3Cl.$$

This base we have called *methyl-nor-narcotine*; it forms an almost white amorphous powder insoluble in water and ether, slightly soluble in alcohol; it is easily soluble in carbonate of sodium, by which means it may be separated from narcotine. None of its salts form crystalline compounds (the chloride, sulphate, and nitrate have been made). In the paper of which this is an abstract, mention is made of two other new bases derived from narcotine; these have not as yet been described. They are the dimethyl and nor-narcotines, the first being the product of the action of hydrochloric acid for a short time on narcotine, and the latter the product of the action of strong hydriodic acid on narcotine. The reactions may be written

$$C_{22}H_{23}N\,O_7+HCl=C_{21}H_{21}N\,O_7+CH_3Cl$$

and

$$C_{22}H_{23}N\,O_7+3H\,I=C_{19}H_{17}N\,O_7+3CH_3I.$$

There exist, therefore, four narcotines:—

1.	Ordinary narcotine, or		trimethyl nor-narcotine,	$C_{22}H_{23}N\,O_7$.
2.	„	„	dimethyl nor-narcotine,	$C_{21}H_{21}N\,O_7$.
3.	„	„	methyl nor-narcotine,	$C_{20}H_{19}N\,O_7$.
4.	„	„	nor-narcotine,	$C_{19}H_{17}N\,O_7$.

The descriptions and properties of the first-mentioned new bases will form the subject of a future communication.

VIII. "On the Chemical Intensity of Total Daylight at Kew and Pará in 1865–67." By Henry E. Roscoe, F.R.S. Received May 14, 1867.

(Abstract.)

This communication contains the results of a regular series of measurements of the chemical action of daylight, carried out at the Kew Observatory, through the kindness of Dr. Balfour Stewart, according to the method described by the author in the Philosophical Transactions for 1864, p. 605. The observations extend over a period of two years, from April 1, 1865 to March 31, 1867. The second part of the communication gives the results of observations upon the Intensity of the Chemical action of Sunlight under the Equator, made at Pará in latitude 1° 28′ S. during the month of April 1866.

I. *Kew Observations.*

The Kew measurements do not profess to exhibit the changes in chemical intensity which occur from hour to hour, but they give, with accuracy, the mean monthly chemical intensity, showing the rise and fall with the changing seasons of the year, and they enable us to deduce the mean monthly and yearly chemical intensities at Kew for 1865–67.

Tables showing the daily mean chemical intensity obtained from the daily observations, according to the method described in the above-mentioned paper, are given. The first result which these observations yield is that the mean chemical intensity for hours equidistant from noon is constant; that is, the mean chemical intensities are equal for equal altitudes of the sun; thus the mean of all the observations made about $9^h\ 30^m$ A.M. corresponds with the mean intensity at $2^h\ 30^m$ P.M.

	Mean of Times of Observation.	Mean Chem. Intensity.
Mean of 529 Afternoon Observations in 1865–67.	.........$9^h\ 41^m$	0·105
Mean of 552 Morning Observations in 1865–67.	.........$2^h\ 27^m$	0·107.

Hence the author concludes that when the disturbing causes of variation in amount of cloud, &c. are fully eliminated by a sufficient number of observations, the daily maximum of chemical intensity corresponds to the maximum of sun's altitude. The author then shows from measurements made at varying altitudes of the sun at Heidelberg and Pará, that the relation between sun's altitude and chemical intensity may be represented by the equation

$$CI_a = CI_0 + \text{const.}\, a,$$

where CI_a represents the chemical intensity at a given altitude (a) in circular measure, CI_0 the chemical intensity at the altitude 0, and const. (a) a number to be calculated from the observations.

The agreement of the chemical intensities as found at Heidelberg with the calculated results is seen in the following Table:—

Altitude.	Chemical Intensity. Found.	Calculated.
° ′		
7 15	0·050	0·050
24 43	0·200	0·196
34 34	0·306	0·276
53 37	0·437	0·435
62 30	0·518	0·506

A similar relation is found to hold good for the Pará observations. Assuming the same relation to exist at Kew as at Heidelberg and Pará, the values of the mean monthly intensity at noon have been calculated from the observations at 2.30 and 4.30 P.M., and the mean monthly integrals of chemical intensity for each month, from April 1865 to March

1867 inclusive, have been obtained. Curves exhibiting the daily rise and fall for each of the twenty-four months, as well as a curve showing the biennial variation of chemical intensity for the same period, accompany the paper. The curve of yearly chemical intensity is found to be unsymmetrical about the vernal and autumnal equinoxes; thus in spring and autumn the results are as follows:—

1865–67.	Mean Ch. Int.	1866.	Mean Ch. Int.
March 1867	30·5	March	34·5
April 1865	97·8	April	52·4
September 1865	107·8	September	70·1
August 1865	88·9	August	94·5

Or for 100 chemically active rays falling during the months of March and April 1865, 1866, and 1867 at Kew there fell in the corresponding autumnal months 167 rays, the sun's mean altitude being the same.

The author discusses the probable causes of this autumnal maximum; he finds that it is not due to variation in the amount of cloud, and believes that it is to be explained by a less amount of atmospheric opalescence in the autumn than in the spring.

The yearly integral for the twelve months, January to March 1867 and April to December 1865, is 55·7, whereas that for the twelve months of the year 1866 is 54·7.

II. *Pará Observations.*

All the knowledge we possess concerning the distribution and intensity of the chemically active rays in the tropics is derived from the vague statements of photographers. According to their observations it appears that the difficulty of obtaining a good photograph increases as we approach the equator; and more time is said to be needed to produce the same effect upon a sensitive plate under the full blaze of a tropical sun than is required in the gloomier atmosphere of London. Thus in Mexico, where the light is very intense, from twenty minutes to half an hour is stated to be required to produce photographic effects which in England occupy but a minute. Hence the existence of a peculiar retarding influence has been suggested which the heating and luminous rays are supposed to exert upon the more refrangible portions of the spectrum. The fallacy of these statements has been fully proved by a series of direct measurements of the chemical intensity of sunlight under the equator, made at Pará by Mr. T. E. Thorpe. The curves of daily chemical intensity given in the paper show that the activity of the chemical rays in the tropics is very much greater—on one day fifty-five times as great, as in our latitudes; and these measurements prove that the reported failures of photographers cannot at any rate be ascribed to a diminution in the chemical intensity of sunlight. The following numbers give some of the daily mean chemical intensities at Pará compared with the same days in Kew:—

Daily Mean Chemical Intensity.

1866.	Kew.	Pará.	Ratio.
April 6	28·6	242·0	8·46
„ 7	7·7	301·0	39·09
„ 9	5·9	326·4	55·25
„ 11	25·4	233·2	9·18
„ 20	38·9	385·0	9·90
„ 24	83·6	362·7	4·34

The measurements were made at Pará in the middle of the rainy season, and at very frequent intervals during the day; the curves show the enormous and rapid variation in intensity from hour to hour which the chemically active rays undergo under a tropical sun during the rainy season.

IX. "On the Elimination of Nitrogen during Rest and Exercise on a regulated Diet of Nitrogen." By E. A. Parkes, M.D., F.R.S. Received June 1, 1867.

The experiments recorded in this paper are intended to complete the inquiry into the effect of rest and exercise on the elimination of nitrogen recorded in the Proceedings of the Royal Society (No. 89, 1867).

The experiments were made on two soldiers at the Royal Victoria Hospital at Netley. One of them (S.) was the subject of the former experiments, the other man (B.) was a fresh man. B. is a perfectly healthy temperate man, aged 22½ years, 5 feet 9¼ inches in height, and weighing 140 lbs.

Extreme care was taken to ensure the greatest accuracy both as to food and as to the collection of the excreta. The whole value of such experiments as these, depends on the exactness with which all the conditions are carried out; and without perfect accuracy, the results would only mislead. I have every confidence that the conditions were faithfully observed; there is in fact evidence of this from the experiments themselves.

The course of the experiments was precisely the same as in the observations recorded in the last paper, except that the diet was during sixteen days exactly the same on each day. During four days the men were at their ordinary employment; during two days rested; returned to ordinary work for four days; took very active exercise for two days; and were then for four days more on ordinary occupation.

They took each day the same amount of food, viz.:—

Articles.	Amount, in ounces av.	Total nitrogen in each article, in grains*.
Bread	16	60·99
Meat (cooked)	9 (15 raw)	213
Potatoes (cooked)	12	12
Cabbage (cooked)	3	·1
Milk	6	16·5
Sugar	3	
Butter	1	?
Salt	·25	
Infusion of tea	20	?
Infusion of coffee	20	?
Water	3 to 9	
		302·59 or 19·61 grammes.

The bread was made always in the same way; the meat (steak) was of an uniform quality, and was carefully selected every day. The whole quantity of food was regularly eaten and at the same time. The only variation was that the potatoes weighed sometimes 12 or 12½, and sometimes 13 ounces (which, however, made very little change in the nitrogen), and that the amount of water drunk, usually 5 ounces at dinner and 2 at supper (on eleven days), was on five days taken in less quantity. No alcoholic liquid was taken, and there was no smoking.

This quantity of food was just sufficient to preserve the body at almost precisely the same weight; the men were in perfect health.

During the sixteen days 313·76 grammes (viz. 19·61 × 16) of nitrogen were known to be taken by each man in the food. The following amounts were recovered from the urine in the same time.

S. .... 303·660 grammes, or 18·97 grammes daily.
B. .... 307·257 grammes, or 19·2 grammes daily.

The nitrogen in the stools (as presently noted) quite made up the difference (10 and 6 grammes) between these numbers and the amount of nitrogen passing in, indeed it rather exceeded it if the average of three days can be applied to sixteen. S. passed regularly rather more nitrogen by the bowels than B., and rather less by the urine.

The weight of the body at the beginning and end was nearly the same, and it is therefore certain that during the sixteen days no nitrogen escaped by the skin or lungs, but that all passed by the kidneys and bowels.

The urine was collected from 8 A.M. to 8 A.M., except on the second days of rest and exercise, when it was collected from 8 A.M. to 8 P.M., and from 8 P.M. to 8 A.M. The nitrogen was determined by soda-lime,

* The nitrogen in the crust and crumb of bread and in the meat was determined once; the other amounts were calculated.

the urea by Liebig's mercuric nitrate, the chloride of sodium being got rid of. The stools were weighed every day*.

On the days of rest the men remained in one room, sitting quite still or lying on the bed; they did not leave the room.

On the first day of exercise they walked twenty-four miles on level ground between 8.10 A.M. and 8 P.M. On the second day they walked thirty-five miles between 8.10 A.M. and 9.45 P.M.

The walking was done well, and S., who had been the subject of the previous experiments of exercise on a non-nitrogenous diet, was quite certain that he supported the fatigue much better under the meat diet, than on the former occasion when he was fed on starch and butter.

The amount of work done (the weight of their clothes being taken into account) was calculated by Mr. Haughton's formula, viz., that walking on a level surface is equal to lifting $\frac{1}{20}$ of the weight through the distance walked.

	First day.		Second day.	
	Kilogramme-metres.	Tons lifted a foot.	Kilogramme-metres.	Tons lifted a foot.
S.	129198=	416	194798=	627
B.	125120=	403	188605=	607

The weight of the body.

The following Table gives the daily variations in the weight of the body during the whole period. The weight was taken at the end of the 24 hours.

	S.	B.
	lbs. av.	lbs. av.
Ordinary occupation, 1st day	145	139·5
" " 2nd "	145	140
" " 3rd "	146	139
" " 4th "	145	139·5
Rest .... 5th "	144	138·75
" .... 6th "	143·5	138·5
Ordinary occupation, 7th "	144	138·5
" " 8th "	145	139·25
" " 9th "	145·5	139·5
" " 10th "	145	140
Exercise .... 11th "	142	137
" .... 12th "	139·5	135
Ordinary occupation, 13th "	142	135·75
" " 14th "	143	137·5
" " 15th "	144	138·25
" " 16th "	144·5	139·75

The following Table shows the gain or loss of body-weight in grammes (round numbers).

* I determined also the chloride of sodium and the phosphoric acid (on three occasions), but I have not included these results, in order not to complicate the statement.

	S.	B.
Ordinary occupation, 1st day.........	0	0
" " 2nd "	0	+ 200
" " 3rd "	+ 400	− 500
" " 4th "	− 400	+ 300
Rest .... 5th "	− 500	− 400
" .... 6th "	− 200	− 100
Ordinary occupation, 7th "	+ 200	0
" " 8th "	+ 500	+ 400
" " 9th "	+ 200	+ 100
" " 10th "	− 200	+ 200
Exercise .... 11th "	− 1500	− 1400
" .... 12th "	− 1000	− 1000
Ordinary occupation, 13th "	+ 1000	+ 500
" " 14th "	+ 600	+ 800
" " 15th "	+ 400	+ 400
" " 16th "	+ 300	+ 600

The weight in the first period was fairly constant; but during the rest-period one man lost 1½ lb. and one other 1 lb. in weight; the loss was gradual on the two days, which was different from the alternations which had gone before; the loss was subsequently recovered from at the rate of rather less than ½ lb. a day until the usual weight was regained on the third and fourth days. As the amount of food ingesta was not less, the loss must have been owing to increase in the egesta. This was certainly an unexpected result, but is yet quite certain.

The nature of the increase in the egesta will appear presently. I will merely state here that it was not owing to any condition of external temperature or atmospheric humidity acting on the skin or lungs. In the first four days of ordinary occupation the maximum temperature in the shade was 59°, 61°·2, 64°·8, and 65° F., while the mean of the maximum and minimum temperatures of twenty-four hours was 51°·2, 52°·6, 50°·2, 54°·4. In the rest-period of two days the maximum shade temperature was 64° and 68°, and the mean temperature of the days was 54°·5 and 58°4. In the after-rest period, when the body was regaining weight on the same diet, the temperature rose greatly, the maximum being 74°·8, 81°·6, 75°, and 70°, while the mean of the maximum and minimum was 61°, 66°·3, 62°, and 59°·5.

It is evident therefore that the weight altered independently of the external temperature; for there was scarcely any difference between the first and rest-period, and if any action had been caused it should have gone on in the succeeding hotter days of ordinary exercise during the third period. The air was a little drier during the two days of rest (65·3 per cent. of total humidity) than in the preceding and following periods (72·6 and 72·9); but this slight difference had no effect, because on one of the days following the rest the air was both hotter and drier than on one of the rest-days, and yet the body gained weight.

During the period of exercise both men lost greatly and almost equally in weight, and then during the following period regained it, so that in four days one man had recovered his former weight, and the other man was only ½ lb. short.

arison to give the whole of the results in one Table.

Excretion of Urinary Nitrogen, in grammes. First Period.—Ordinary

S.							
Urea.	Nitrogen in urea.	Non-ureal nitrogen.	Total nitrogen.	Proportion of ureal to non-ureal nitrogen.	Quantity of urine, in cubic centimetres.	Urea.	Nitrogen in urea.
37·668	17·578	·308	17·886		1130	41·245	19·247
35·695	16·657	·153	16·810		810	34·587	16·140
36·300	16·940	2·272	19·212		810	34·425	16·065
37·355	17·432	·088	17·520		870	38·280	17·864
36·754	17·1517	·7052	17·857	1 to ·041	905	37·134	17·329

Second P

Third Period.—Ordinary occupation.

7th day.........	920	34·04	15·885	·035	15·920		750	34·5	16·1	·582	16·682	
8th „	960	37·44	17·372	·236	17·608		800	39·2	18·283	·332	18·615	
9th „	1180	38·94	18·172	1·210	19·382		920	41·4	19·32	1·262	20·582	
10th „	960	34·08	15·914	1·626	17·54		910	35·035	16·349	1·712	18·061	
Mean	1005	36·125	16·836	·7767	17·612	1 to ·046	845	37·534	17·512	·972	18·485	1 to ·055

Fourth Period.—Exercise.

11th day.........	1000	35·5	16·566	1·912	18·478		1110	39·96	18·648	·342	18·99	
12th „ 8 a.m. to 8 p.m.	430	15·05	7·023	·324	7·357		565	19·492	9·096	·957	10·053	
12th day 8 p.m. to 8 a.m.	650	26·741	12·479	·978	13·457		540	21·600	10·080	·795	10·875	
Mean of two days	1040	38·645	18·034	1·607	19·646	1 to ·089	1107·5	40·526	18·912	1·047	19·959	1 to ·055

Fifth Period.—Ordinary occupation.

13th day.........	900	43·65	20·370	·88	21·25		1000	38	17·734	2·517	20·25	
14th „	1000	39·5	18·433	1·509	19·942		1100	40·15	18 736	·537	19·273	
15th „	1430	42·9	20·029	3·459	23·488		1250	35·625	16·625	2·623	19·248	
16th „	1730	37·195	17·357	2·179	19·536		1610	41·86	19·534	2·063	21·597	
Mean	1265	40·811	19·047	2·006	21·054	1 to ·105	1240	38·909	18·157	1·935	20·092	1 to ·106

The elimination of nitrogen by the urine followed precisely the same course in each man; and allowance being made for the difference in food, this course was identical with that determined in the former experiments, when the diet was non-nitrogenous. It is certain that neither during rest nor exercise did nitrogen pass off by the skin or lungs.

It will be convenient to consider the total nitrogen in the first instance.

During the first period of four days the total nitrogen excreted was 71·428 grammes by S. and 74·008 grammes by B. In the period of rest, instead of falling the nitrogen increased in amount, so that in two days 38·274 and 38·943 grammes were excreted. This is not only more than the half of the previous four days, but more than the amount of either the first two or the last two days of the first period. The greatest increase was in the first day of rest, but in the second day the amount was still above the mean of the previous period.

As afterwards shown, this was not owing to lessened elimination by the bowels; for both the weight of the stools and the nitrogen increased in the period of rest. It seems impossible to avoid the conclusion that the condition of rest with an equal entry of nitrogen was accompanied by a daily increase of excretion by the urine of about 1 gramme more nitrogen.

It may, indeed, be said that this is within the limits of error or unavoidable variation, and may be accidental; but if so, it seems most remarkable that the result should run in the same way and be of nearly the same amount in each case, and be confirmed by the independent observation of the urea. In the third period, when the men returned to their ordinary occupation, the nitrogen fell in both on the first day to a lower point than had ever before been noted, and then rose gradually, so that in the four days the amount was almost the same with that of the first period, 70·45 and 73·94 grammes being excreted. In the period of exercise which is to be compared with that of the rest, the results were identical with those of the former experiments when nitrogen was not supplied.

On the first day of exercise the nitrogen in each man fell below the corresponding day of rest by 1·626 and 1·131 gramme. In the next twelve hours, which were almost entirely occupied in exercise, the diminution was still greater, being 2·498 and 1·225 grammes, which would be equivalent to 5 and 2½ grammes for twenty-four hours. In the last twelve hours, of rest after work, the elimination increased greatly, so that 5·142 and 3·331 grammes more were excreted than in the corresponding rest-period; the general result being that on the whole two days' period of exercise, as compared with the whole period of rest, there was an increase of about 1 gramme in the exercise-period in each man, owing entirely to the large excretion in the last twelve hours.

	S.	B.
	grammes.	grammes.
Total nitrogen in urine in two days' rest ...	38·274	38·943
Total nitrogen in two days' exercise	39·292	39·918
	1·018	0·975

The first day following the exercise was a day of almost complete rest; the nitrogen in both men was increased considerably over the average of the first and third periods, and very greatly indeed over the amount of the first day of the third period, the excess being 5·33 and 3·568 grammes over that day. This was the most considerable variation in the period of experiment. The nitrogen continued high all through this period, the result being that in the four days S. excreted 84·216 and B. 80·368 grammes, or 13 and 6 grammes respectively in excess over the first period of four days. It is clear indeed that during this period, the excretion of nitrogen must have been greater than the ingress.

I will not trace the changes in the urea in such detail. They were almost identical with those in the total nitrogen.

In the first period the amount of urea was almost precisely the same in the two men. In the rest-period it increased nearly 2 grammes daily in each man, fell during the third period to the former average, decreased greatly during the first thirty-six hours of the exercise-period as compared with the rest-period, and increased in the last twelve hours; in the last or after-work period it also increased, though in a less proportion than the total nitrogen.

The changes in the non-ureal nitrogen were also very similar in the two men, but will be best followed in the case of B., in whom the excretion of non-ureal nitrogen was more steady from day to day than in S. It was very slightly and immaterially increased in the rest-period, fell as slightly in the after rest-period, remained the same during the exercise-period, and increased to nearly double in the last four days. In S. it increased more in the rest-period and in the exercise-period than in B., and still more in the last four days. This increase in the non-ureal nitrogen after exercise is confirmatory of the results formerly obtained on this point.

If these results are looked at as a whole, it will be seen that though the changes in the amount of nitrogen were for the most part not great, still they were decided and evident changes, and occurred precisely in the same way in the two men. The coincidence in the changes in the urea and in the total nitrogen (determined by such different processes) is a strong argument that the results were real. Throughout the whole time the food was precisely the same, and the modifications were therefore not owing to variation in the ingress of nitrogen.

There was some variation in the amount of urinary water; but the

increased excretion of nitrogen was, I believe, not at all connected with it. Thus in the first and third periods the nitrogen was almost the same, yet in S. the difference in the mean amount of water was 266 cub. centims., and in B. was 60 cub. centims. In S., in the fifth period, the amount of water was the same (within 6 cub. centims.) as in the first period, yet the nitrogen was more than 3 grammes in excess. If individual days are taken, no obvious relation appears between the urinary water and the nitrogen. The largest amount of water in S. (1760 cub. centims.) corresponded to 19·536 nitrogen, while the largest amount of nitrogen (23·488) corresponded to 1430 cub. centims., and the next amount of nitrogen (21·25) was passed in only 900 cub. centims. of urine. In B. the largest amount of nitrogen (21·597) was contained in the largest amount of water (1610 cub. centims.), but almost as great an amount was contained in 1000 and 920 cub. centims. So that differences in the amount of water cannot explain the variations in the exit of nitrogen. If not owing to alteration in food, nor to variable passage of water through the kidneys, it seems tolerably certain that the conditions of rest and exercise were the causes of the variation.

Excretion of Nitrogen by the bowels.

The two men did not have quite the same amount of intestinal excreta. The average daily weight (sixteen days) in the case of S. was 4·798 ounces avoirdupois or 136 grammes; while in the case of B. they amounted only to 3·97 ounces, or 112·8 grammes.

The exact daily weights are given further on, and I will now merely state the amount of nitrogen, which was determined three times.

	Nitrogen in grammes.	
	S.	B.
Last day of first period...............	1·227	0·644
Last day of rest.......................	1·486	1·091
Last day of exercise	2·138	1·504
Mean	1·617	1·079

B. passed (if these three days represent the mean) 0·538 gramme less nitrogen daily by the bowels than S., and during the first twelve days he passed on an average 0·6 gramme more nitrogen in the urine, so that during these twelve days the discharge of nitrogen by the conjoint channels was within 1 gramme the same in the two men; the amounts being in S. 238·848, and in B. 239·757 grammes in twelve days, while the amount of nitrogen passing in (independent of a small amount in the tea, coffee, butter, &c.) was 235·32 grammes. This accordant result proves, I believe, both the estimate of the nitrogen in the food and the collection and analysis of the excreta to have been accurate. I was

quite unprepared for a result so close as that the difference in the excretion of nitrogen of the two men should be only 0·076 gramme, or scarcely more than 1 grain daily. In the last four days S. passed a little more nitrogen by the urine than B., thereby reversing what had gone before. The stools were not analyzed during this period, but I believe that the nitrogen must have been furnished by the body during these four days. As respects the effect of exercise on the intestinal nitrogen, there was a slight increase in rest over the previous period and in exercise over the rest-period.

If the following Table (p. 54) be analyzed, it will be found that the loss of weight in the rest-period was attributable in S. almost entirely to excess in the pulmonary and cutaneous excreta, while in B. it was owing to increase in the urinary and intestinal excreta. It might be presumed to have been chiefly water; but the simultaneous changes in the excretion of nitrogen give it interest. The channel of elimination in B. proves in another way that it was not owing to effect of external temperature in the air.

During the period of exertion the loss of weight was from increase in the skin and lung excretion, and it is interesting to observe how parallel it was in the two men; the loss of weight was subsequently made up by lessening of the skin and lung excreta. The intestinal excreta were not influenced either way by the exercise; and in spite of the great passage of water by the skin, the urinary water was not affected. The antagonism commonly stated to exist between the excretion of water by the skin and kidneys was not perceptible.

Explanation of the preceding facts.

Taking into account the experiments formerly recorded as well as those in this paper, we have to explain the following phenomena.

1. With an unchanged ingress of nitrogen there was a slight excess of nitrogenous excretion during rest as compared with a period of ordinary exercise.

2. There was a decrease of urinary nitrogenous excretion during active exercise as compared with a period of rest, and this was perceptible both when the ingress of nitrogen was stopped, as well as when nitrogen was supplied in regular amount.

3. There was an excess, not great, but long continued in nitrogenous excretion after exercise.

4. There was a retention of nitrogen in the system when it was again supplied after having been cut off, after both rest and exercise, and greatest in the latter case, showing that it is needed in the system, and that an insufficient supply at one time must be subsequently compensated.

In addition we cannot leave out of account the well-known dietetic fact, based on experience, that much muscular work always demands the supply of a larger amount of nitrogen.

wing the daily weights in grammes of the excreta.—The urinary … ; the pulmonary and cutaneous excreta were determined by calc… and the weight of the urine and fæces, furnishing the elements of … lisregarded.

	Food ingesta.	S. Egesta.			Food ingesta.	Urinary.
		Urinary.	Intestinal.	Pulmonary and cutaneous.		
.....	2783	1510	106·5	1166·5	2669	[illegible]
.....	2683·8	1256	220	1207·8	2559	[illegible]
....	2648	1255	106·5	886	2641	[illegible]
....	2733	1251	99·4	1782·6	2733	[illegible]
..	2698	1287	198	1589	2600	
.	2726	1185	106·5	1634·5		

Both the theories of muscular action now being discussed by physiologists seem to me insufficient to account satisfactorily for all the above facts.

The old theory was, that a muscle was more or less destroyed during action and was repaired during rest, and if so, it seemed reasonable to suppose that the action of the muscles would be measured by the amount of nitrogen eliminated. But the decrease in the nitrogenous excretion during exercise and its very moderate increase afterwards (an increase quite out of proportion to the amount of muscle supposed to be destroyed) seem quite inconsistent with this view.

The new theory, springing from the experiments of Professors Fick and Wislicenus, viz., that the nitrogenous framework of a muscle is merely the machinery which allows changes in the non-nitrogenous substances to take place, and that in itself it undergoes during exercise no change, though at first sight consistent with some of the facts, does not appear to be so with all. It does not account for the increase of nitrogenous excretion in rest, for the decrease during exertion, or for the increase afterwards, nor in a satisfactory manner for the great retention of nitrogen in the system which occurs after exercise on a non-nitrogenous diet.

There is something more in the facts than either disintegration *per se*, or stability of nitrogenous composition during muscular action, will account for.

We must find some other explanation; and it appears to me that we can only express the facts by saying that a muscle during action appropriates more nitrogen than it gives off, and during rest gives off more than it appropriates. We have, perhaps, strictly speaking, no right to go beyond this; but it seems clear that as a muscle could hardly be supposed to have two simultaneous actions, we may simplify the above expression by stating that during action a muscle takes nitrogen, and during rest gives it off. To put this in other words, the action of a muscle would seem from these experiments not to be connected with disintegration, but with formation; when it is in exercise the muscle increases, when it is quiescent it lessens in bulk. It may seem a bold innovation to attempt to reverse in this way the old theory of muscular action, especially as the same rule would have to be applied to nutrition generally; but if it explains all the facts, it is at any rate entitled to be fully considered.

In applying this expression in the explanation of the facts, I must premise that the nitrogen discharged by the kidneys and bowels cannot be supposed to be derived solely from the muscles. As it represents all the nitrogen going in, it must be derived from all the nitrogenous tissues, from the nervous substance, the gland cells, the albuminous membranes and fluids, in fact from all nitrogenous structures. That portion of it which is derived from the muscular system comes only in part from those

muscles whose state we can alter. We cannot alter the action of the muscles of respiration, of the heart, the stomach, and intestines, &c. We cannot even reduce the voluntary muscles to a state of complete and prolonged rest. There must be some movement, consequently we must not expect to find large variations in the elimination of nitrogen when a certain number of muscles only are kept in a state of comparative rest or exercise.

The food passing into the body after due preparation in the stomach, liver, and lungs, forms in the blood a reservoir or store of nutriment from which the different parts of the body take their supply as they require it, or according as the special stimulus of each enables it to appropriate it.

In these two men 19·6 grammes, or 302 grains passed daily into, and then out of, the store into the various nitrogenous tissues. This quantity exactly sufficed in the then state of activity of all the organs to preserve perfect action, and to keep the body-weight constant.

A certain number of muscles being brought into a state of rest, the nitrogenous elimination increased; in other words, the muscles appropriated nitrogen in less, and gave it off in greater, amount, owing, if my explanation be correct, to their more rapid disintegration during rest than exercise. This may be understood by supposing that if in the twenty-four hours the voluntary muscles are in a state of rest for twelve, and of exercise for twelve hours, and if the exercise is reduced to six hours, the removal going on at the same rate for eighteen hours instead of twelve hours will increase the exit of nitrogen 50 per cent. Accordingly during the period of rest the elimination of nitrogen increased, and this was necessarily most marked during the first day, when the bulk of the quiescent muscles was greater than on the second day, when it had been reduced by excess of elimination. I do not see how properly to explain the increase during rest except in this way; if the fact be as I state it, no theory of muscular action can be true which does not account for it.

The effect on the reserve or store of nitrogen in the blood would be to leave in it more nitrogen than usual at the end of the two days' rest. The men then commenced ordinary occupation, and immediately the muscles began again to contract and to assume more nitrogen in consequence of the increased exercise. As they had to regain their former composition, the elimination of nitrogen necessarily lessened, and the reserve must have fallen to its normal amount. They would use up the accumulation in the reserve as well as the fresh supply, and the equilibrium would be restored; this was nearly done in fact in twenty-four hours, as may be seen in the Table.

After four days the men took excess of exercise. The elimination of nitrogen at once lessened, because more was used by the contracting muscles, and there were lesser intervals of rest.

The last 10½ hours of the two exercise days formed a period of rest; and during this time the excretion increased, and this increase continued more or less for four subsequent days.

This might be explained by the passing off of excretory products formed during the contraction, according to the old theory; but if so, it seems singular that the increased excretion should have been so moderate, and at the same time should have been spread over so many days, whereas on the hypothesis I have suggested it is easily explicable.

During the exercise-period the extra action of the muscles had added a large amount of nitrogen to their structure; at the end of the time the muscles must have been bulkier, and therefore in the succeeding period of rest furnished a larger elimination of nitrogen than in the after rest-period when they were smaller. Moreover, after the exercise-period there was much more rest than after the rest-period. In the first day after the exercise the men were tired and rested the whole day, and even on the following days did not probably make so much exertion as usual. And the gradual elimination for so many days looks much more like a temporarily enlarged organ returning slowly to its normal size, than like the passage of accumulated excretory products; the chief product being the very soluble urea which is always so rapidly removed from the muscles that scarcely any can be detected in them.

The facts observed in the experiments on a non-nitrogenous diet seem now to be also easily explained. The decrease in the urea during the period of exercise equally occurred, because the muscles used more nitrogen in their action than in the rest-period, taking it from the store, and thereby no doubt robbing other parts.

During the two days of exercise without nitrogen, the muscles may have been just as well fed with nitrogen as during the experiments with 300 grains, only other parts could not have been so; other organs and the muscles not called into play must have acquired nitrogen with more difficulty, and consequently when nitrogen was again given, a large portion was retained to replenish the store and to feed the organs which had been on short allowance.

The quantity retained when nitrogen was again given did not serve (we may suppose) to nourish muscles exhausted by the exercise (which on my theory had even increased in nitrogenous constituents), but other parts.

If this reading of the facts be admitted, it may be asked how it will affect the inference drawn from the experiments of Professors Fick and Wislicenus. They determined the nitrogen discharged, calculated how much muscle it represented, and then argued (and as Dr. Frankland has shown, correctly argued) that this amount of muscle could not have produced the mechanical force which had been exerted. But it is apparent, if I am correct, that the measure of the work must be the amount of nitrogen appropriated by, and not that eliminated from, the muscle, and this was not shown in their celebrated experiments.

But though doubt may be raised as to the basis of their opinion, I

conceive the opinion itself was probably correct. Because even if the work is done during the period when nitrogen is added, and not when it is eliminated, it is difficult to suppose that the changes in the nitrogen are on a scale large enough to account for the result, or that the transformation of a particle of blood-albumen into a particle of muscle-albumen could be attended by any chemical changes which *per se* could equal the mechanical force produced. But we can imagine that such a transformation may be the cause of changes in the non-nitrogenous substances to which the manifestation of force is really owing. There is no reason why disintegration should be more attended with such changes than formation. Indeed it is perhaps more often that the union of chemical substances is attended by signs of transformation of force than their disunion. Or the stimulus which causes the addition of the nitrogen to the muscle may at the same moment originate the changes in the non-nitrogenous substances.

The fact that the substances the presence of which in the muscle suspends the contraction (and therefore, if I am right, the growth of muscle), appear from Ranke's latest observations to be derived from the non-nitrogenous substances, is another argument in favour of the view that great changes go on in these substances during muscular action.

If the opinion of Professors Fick and Wislicenus to this extent, and if the experiments of Ranke and others on the effect of the effete products be adopted, the following would be the theory of muscular action I would propose.

When a voluntary muscle is brought into action by the influence of the will, it appropriates nitrogen and grows; the stimulus or the act of union gives rise to changes in the non-nitrogenous substances surrounding the ultimate elements of the muscular substance which cause the conversion of heat into motion. The contraction continues (the will still acting) until the effete products of these changes arrest it; a state of rest ensues, during which time the effete products are removed, the muscle loses nitrogen, and can again be called into action by its stimulus.

This theory not only explains the experiments now recorded, but simplifies our ideas both of the growth and of the wasting of muscle, and seems likely to explain more easily some processes in disease.

It is also in greater accordance with the rules of diet derived from experience than the theory of Fick and Wislicenus. If correct, it shows why the muscle requires nitrogen for its action, and why increased action requires increased nitrogen. The food must either supply this, or the store of nitrogen in the blood and other organs must be lessened*.

* That an increased supply of fat, and perhaps of starches, is also desirable has long been practically recognized, though the store of fat already in the body renders this less necessary for a time. The observations of Lawes and Gilbert seem to me to render it possible that when a muscle parts with its nitrogen, fat is formed, and if so, a muscle disintegrating during rest may form a store of fat in its texture which may be further transformed at the next addition of nitrogen, *i.e.* at the next contraction.

It enables us to understand why in a well-fed body it may be some time after nitrogen is cut off before the muscles have any difficulty in obtaining what they want, and why in a body ill-supplied with nitrogen exertion lessens, or if kept up produces bad effects.

If exertion is persevered in under such circumstances, a failure somewhere is always observed. Frequently the nervous system or the heart shows signs of weakness, a result which could hardly be explained by the view of the Swiss Professors. It is certainly an argument for the view I have advocated, that it is in harmony with the teachings of experience, and restores to the rules of diet their old significance.

X. "Note on the Lunar-diurnal Variation of Magnetic Declination." By J. A. Broun, F.R.S. Received May 11, 1867.

Lausanne, 7th May, 1867.

I received late last night No. 91 of the Proceedings of the Royal Society, and desire to offer the following remarks on the abstract of a paper by Mr. Neumayer which I find therein (vol. xv. p. 414).

Mr. Neumayer is evidently unacquainted with the Note by me, read to the Royal Society of London in 1861 (Proc. Roy. Soc. vol. x. p. 475), in which I stated as result of the discussions of five years' observations at Trevandrum (near the magnetic equator) that the lunar-diurnal variation of magnetic declination became inverted, like the solar-diurnal variation, when the sun passed from one hemisphere to the other, both the solar- and lunar-diurnal variations depending on the position of the sun.

I also stated the laws of the lunar-diurnal variation, not only for the moon north and south, as Mr. Neumayer has done, but also for the moon on the equator moving northwards, and again on the equator moving southwards, the laws being different for the moon in the same position according as she was moving in one direction or in the other.

I pointed out in the Transactions of the Royal Society of Edinburgh (vol. xviii. p. 354), that the reversal of movement of the declination-needle with the sun north and south of the equator, observed within the tropics, had its equivalent in the different ranges of the solar-diurnal variation for summer and winter in high latitudes. It followed in like manner that, the lunar-diurnal variation being inverted with the solar-diurnal variation near the equator, a similar difference of ranges should be observed in the laws of lunar-diurnal variation for summer and winter in the higher latitudes. Of this fact I satisfied myself by a rediscussion of the Makerstoun observations, after rejecting the large disturbances.

Another consequence of the law of inversion of the lunar-diurnal variation near the equator with the sun's passage from one hemisphere to another, and with the inversion of the solar-diurnal variation, was the opposition or approximate opposition of the mean curves

of lunar-diurnal variation in the higher latitudes of the two hemispheres. This conclusion, however evident at the time my note was written (1861), appeared opposed to the fact, since the law of lunar-diurnal variation at Toronto, according to General Sabine's discussion, was an inversal of that at Prague and Makerstoun, all three places in the same hemisphere; this I pointed out at the time (Proc. Roy. Soc. vol. x. p. 475). This statement seems to have caused a re-examination of the Toronto discussion, as General Sabine afterwards discovered that west had been substituted for east in his original memoir.

It followed from the similarity of the laws for the sun and moon discovered by me, and, *this correction made*, from the observations in the two hemispheres, that the mean law for a north latitude should be the inverse of that for a south latitude; or that a maximum of easterly declination in one hemisphere should be simultaneous, or nearly so, with a minimum in the other.

My chief object now is to draw attention to the fact (published in 1861) of the similarity of the changes of the laws of solar- and lunar-diurnal variations of the magnetic needle, with the sun's change of declination, as this fact appears to have escaped the notice of those men of science who since then have been engaged in proving independently the conclusions which follow from the note now referred to.

Mr. Neumayer remarks "that in some cases the lunar-diurnal variation manifests itself in a very striking manner during the winter months." This fact I had already remarked in the discussion of the Makerstoun observations for 1844 and 1845; but I have shown in a paper forwarded lately to the Royal Society of Edinburgh that the effect of the lunar action is sometimes *greater* than that of the solar action; and this is made evident from the lunar-diurnal variations for *single* days, as well as in the means deduced from a single lunation (Dec. 1858 to Jan. 1859) for each of the four positions of the moon already referred to.

XI. "An Account of Observations on the great Nebula in Orion, made at Birr Castle, with the 3-feet and 6-feet Telescopes, between 1848 and 1867." By Lord OXMANTOWN. Communicated by the Earl of ROSSE, K.P., &c. Received June 17.

(Abstract.)

In this paper an account is given of the observations which have been made with the 3-feet and 6-feet telescopes on the great Nebula in Orion during the last eighteen years. The observations are accompanied by an elaborate drawing.

In the year 1852, Mr. Bindon Stoney made a drawing of the Huy-

genian region; it was repeatedly compared with the nebula by several persons, and we believe therefore that it was quite accurate. It is not now an exact representation of the nebula as it exists, consequently there seems to be strong evidence of change.

The observations were continued by Mr. Hunter from 1860 to 1864, and by me to the present time. A drawing was made by Mr. Hunter while he was assistant, and it has been verified by me in almost all its details, and extended considerably. In one place, where there is a disagreement between Mr. Hunter's drawing and mine, Mr. Hunter had previously been under the impression that some change was going on.

The nebula, when nearly on the meridian, was examined with the 6-feet instrument and with the 3-feet instrument, before and after that time. The appearance of the nebula differs from night to night, as the faint details come out more or less perfectly in the different states of the atmosphere; but the drawing represents it as seen on the best nights.

The present drawing contains many new stars, some laid down by the micrometer, and others by eye estimation. The nebula has been traced to a distance of fully 40′ North, and about the same distance South of the trapezium, on the following side to a distance of about 30′, and to a much greater distance on the preceding side.

As to resolvability, the brighter parts contain a great number of minute stars, generally of a reddish colour. With the spectroscope three bright lines were seen, but there was no certain evidence of a continuous spectrum. The results arrived at by means of the spectroscope do not, however, appear to be at variance with our observations on resolvability, as even if the whole nebula were to consist of minute stars, the continuous spectrum produced by them would still be extremely faint.

XII. "On the apparent relation of the Nerves to the Muscular Structures in the Aquatic Larva of *Tipula crystallina* of De Geer." By Richard L. Maddox, M.D. Communicated by Dr. Sharpey. Received June 18, 1867.

(Abstract.)

To avoid as much as possible errors that might be attributable to a faulty mode of examination, the figures and photographs have all been made from the larvæ alive, and in their natural medium, except two instances in the drawings and one in the photographs. After alluding to the effects of various reagents which were generally found useless in "differentiating" the fine nervous structures, and the ordinary mode of branching in the nerves from the ganglionic chain, two particular methods of termination are selected as illustrative of the relation between the muscular and nervous tissues. One, termed the "flabelliform,"

where the nerve on approaching the muscular sheath expands into a fan shape, and with its fine granular and nucleated contents embraces the muscle in form of the letter A, without any evidence of the granular matter and sarcous elements being in absolute contact; the other, called the "stapiform" or stirrup-shaped. The latter, in its early stage, is knobbed in appearance. This, the early stage, is shown gradually passing into the cellular, looped, or stirrup form, embracing the fine muscular structure somewhat obliquely, or passing entirely round it, and projecting beyond its edge. In this form also there was no evidence of any union of the granular contents with the sarcous elements, though firm union existed between their sheaths or outer membranes. Fine networks, ending apparently in a granular irregular spot with a pale centre and uniting, are pointed out. The relation and union of short muscles passing between others, and nerve-fibres lying alongside them, with flabelliform expansions, are remarked on, and shown in the figures and photographs.

Muscles undergoing degeneration, or the metamorphic change, are noticed, and in no instance could a nerve-fibre be seen attached to them, or a fibre that could with certainty be traced to any nerve or ganglion. No change was observed of a definite character, as regards the mode of union, under muscular contraction. Some of the finest muscular fibres are passed by for special reasons, as constant motion &c. Attention is called to the blood-corpuscles, or to corpuscles which, for convenience, are called creeping corpuscles, and several figures given. The peculiarities of these bodies are regarded as of considerable importance, and, coupled with a remark in Dr. Beale's contribution to the Transactions of the Royal Society, read May 21st, 1863, in reference to the movement of all forms of living matter.

A figure is given of the head of the larva, with the pharyngeal portion of the digestive tract exserted, which was kept alive for many days; also of the beautiful buccal plexus regarded as nervous, though not traced from its source. Attention is directed to the difference in the condition of the larva when this portion is exserted by compression, causing death.

The difficulties attending this double method of delineation arising from muscular contraction, from the movements of the dorsal vessel, and the digestive tube, and from the thickness of the tissues within and beyond the true focus, rendered almost hopeless the efforts to attain exactness between the drawings and the photographs, or the rendering by sunlight alone of the minutest points, especially with high powers; still the photographs are associated to give a truthfulness to the figures by hand.

The terminations of some nerves in the blood-red larva of another gnat, showing the distinct flabelliform arrangement, are also briefly alluded to, with figures to sustain the views advanced.

XIII. "On the Identity of the Body in the Atmosphere which decomposes Iodide of Potassium with Ozone." By Thomas Andrews, M.D., F.R.S. Received June 20, 1867.

It was assumed for many years, chiefly on the authority of Schönbein, that the body in the atmosphere which colours iodide-of-potassium paper is identical with ozone; but this identity has of late been called in question, and as the subject is one of considerable importance, I submitted it lately to a careful investigation, the results of which I beg to lay briefly before the Society. The only property of ozone, hitherto recognized as belonging to the body in the atmosphere, is that of setting free the iodine in iodide of potassium; but as other substances, such as nitric acid and chlorine, which may possibly exist in the atmosphere, have the same property, no certain conclusion could be drawn from this fact alone.

One of the most striking properties of ozone is its power of oxidizing mercury, and few experiments are more striking than that of allowing some bubbles of electrolytic oxygen to play over the surface of one or two pounds of mercury. The metal instantly loses its lustre, its mobility, and its convexity of surface, and when moved about it adheres in thin mirror-like films to the sides of the containing glass vessel. The body in the atmosphere acts in the same way upon pure mercury; but, from the very minute quantity of it which is at any time present, the experiment requires some care in order that the effect may be observed. On passing a stream of atmospheric air, which gave the usual reactions with test-paper, for some hours over the surface of mercury in a U-tube, the metal was distinctly oxidized at the end at which the air first came into contact with it.

This experiment, however, cannot be considered conclusive, as mercury will tarnish and lose its mobility under the influence of many bodies besides ozone.

It is well known that all ozone reactions disappear when ozone is passed through a tube containing pellets of dry peroxide of manganese, or other body of the same class. The same thing occurs with the substance supposed to be ozone in the atmosphere. About 80 litres of atmospheric air were drawn, at a uniform rate, through a tube containing peroxide of manganese, and afterwards made to play upon very delicate test-paper. Not the slightest coloration occurred, although the same paper was distinctly affected when 10 litres of the same air, without the interposition of the manganese tube, were passed over it.

But the action of heat furnishes the most unequivocal proof of the identity of the body in the atmosphere with ozone. In a former communication (Phil. Trans. for 1856, p. 12) I showed that ozone, whether obtained by electrolysis or by the action of the electrical brush upon oxygen, is quickly destroyed at the temperature of 237° C. An apparatus

was fitted up, by means of which a stream of atmospheric air could be heated to 260° C. in a globular glass vessel of the capacity of 5 litres. On leaving this vessel, the air was passed through a U-tube, one metre in length, whose sides were moistened internally with water, while the tube itself was cooled by being immersed in a vessel of cold water. On passing atmospheric air in a favourable state through this apparatus, at the rate of three litres per minute, the test-paper was distinctly tinged in two or three minutes, provided no heat was applied to the glass globe. But when the temperature of the air, as it passed through the globe, was maintained at 260° C., not the slightest action occurred upon the test-paper, however long the current continued to pass. Similar experiments with an artificial atmosphere of ozone, that is, with the air of a large chamber containing a small quantity of electrolytic ozone, gave precisely the same results. On the other hand, when small quantities of chlorine or nitric acid vapour, largely diluted with air, were drawn through the same apparatus, the test-paper was equally affected, whether the glass globe was heated or not.

From these experiments I consider myself justified in concluding that the body in the atmosphere, which decomposes iodide of potassium, is identical with ozone.

XIV. "On the Anatomy of *Balænoptera rostrata*, Fab." By Alexander Carte, M.A., M.D., F.R.C.S.I., F.L.S., M.R.I.A., &c., and Alexander Macalister, M.D., L.R.C.S.I., Demonstrator of Anatomy, Royal College of Surgeons, Ireland, &c. Communicated by W. H. Flower, Esq. Received June 20, 1867.

(Abstract.)

In this paper the authors give an account of the dissection of a young female of the Lesser Fin or Piked Whale, which was captured off Clougher Head, Co. Louth, Ireland, on the 8th of May 1863.

After describing its external form, and giving accurate measurements of its various parts, the authors point out some differences between the relative sizes and positions of the organs of the animal as contrasted with similar parts of those of the same species which have been recorded by previous writers, especially as regards the position of the dorsal fin, which appendage seems to vary in situation in different individuals; and show, that consequently no value, as indicative of species, ought to be attached to its relative position.

This is followed by a description of the osteology of the animal; and attention is drawn to the fact that the body of the axis vertebra is composed, in part, by the displaced body of the atlas, showing that what at present forms the upper half of the centrum of the axis, is in reality the centrum of the atlas.

The myology of the different regions of the animal has been closely investigated, especially the rudimentary muscles of the paddle, which latter the authors have minutely examined.

The anatomy of the mouth, pharynx, and blowholes is described, and the mechanism by which the functions of respiration and deglutition are performed. In connexion with the larynx, a remarkable muscular pouch is mentioned as existing, which appendage is supposed by the authors to be accessory to the act of expiration, serving a somewhat similar office to that of the air-reservoir in a double-action bellows. Directly in front of the glottis there existed a peculiar hood-like fold of mucous membrane arranged in such a way as to allow of its being drawn over the orifice, and so prevent the entrance of all foreign substances into the respiratory tract during the act of deglutition.

The tongue was found fixed, as far as its tip, by a thick frænum. The lateral walls of the submaxillary cavity were thrown into folds, thereby admitting of considerable distention, this arrangement being peculiarly adapted to the feeding requirements of the animal. The number of baleen plates found in the specimen was 280 on each side.

The muscles for acting on the blowholes were arranged in three strata, the superficial and deepest layers being used in opening, and the intermediate one for closing the nasal canals.

The anatomy of the eye and ear is fully described in the original paper, together with that of the digestive, nervous, and vascular systems; in connexion with this last, remarkable vascular retia were found, situated in the axillary, submaxillary and cervical regions.

In the preceding brief abstract the writers have endeavoured to give an outline of their numerous observations on the anatomy of this Cetacean, believing that it presents many features of novelty and interest not hitherto recorded.

XV. "On the Distribution of the Fibres in the Muscular Tunics of the Stomach in Man and other Mammalia." By James Bell Pettigrew, M.D. Communicated by George Busk, Esq. Received June 20, 1867.

(Abstract.)

The author of the present memoir has examined in succession the stomach of the several domestic animals, the Whale, Porpoise, Bear, Puma, Sloth, Cœbus Monkey, Howling Monkey, Orang-Otang, Chimpanzee, and particularly Man, both in the fœtal and adult state.

The plan adopted in the examination was to distend the viscus immediately after its removal from the body with water or air, and view it as a transparent object; to blanch the stomach by maceration, and distend it with plaster of Paris, tinted with blue, or to stain the parietes with carmine and inject with white plaster, the object in either case

being to throw the delicate fibres into strong relief. By adopting those methods, the author has been able to show that the arrangement of the fibres in the stomach remarkably resembles that found in the heart* and bladder†. This is particularly the case in the human stomach, where the fibres are most highly differentiated. In it the fibres pursue complicated, but well-marked directions; the most external and most internal fibres maintaining a more or less longitudinal course, the deeper or more central ones becoming more and more oblique as the centre of the parietes is reached.

The fibres cross each other with great regularity, both from without and from within, the longitudinal intersecting the very oblique at nearly right angles, the slightly oblique and oblique at more acute angles. The slightly oblique, oblique and very oblique fibres are spiral in their nature, and form, or tend to form, figure-of-8 loops. These loops are directed towards the greater and lesser curvatures of the stomach, but are also traceable on the great cul-de-sac or fundus, and on the lesser cul-de-sac or antrum pylori. As a result of the looped distribution of the fibres, the root of the œsophagus and the pylorus are invested with oblique and very oblique spiral fibres, arranged symmetrically in two sets. These fibres pursue opposite directions, and surround the entrance into and exit from the stomach after the manner of sphincters. The crossing and looping of the fibres extends also to the body of the viscus, and shows that the so-called circular layer is in reality composed of very oblique spiral fibres, intersecting at very obtuse angles.

The fibres are arranged in different planes or strata, and may be divided into external and internal sets. These are united to each other by a mutual interchange of fibrous filaments; and the fibres of the several strata interweave to a slight extent, so that the term layer must be used in a restricted sense. The layers are indicated by the prevailing direction of the fibres, and are something like seven in number, three external and three internal, with an intermediate or central layer between.

The fibres having the same direction, are in some instances strongly developed at one part of their course, and feebly at another. They even become gradually attenuated, until they are no longer discernible. The muscular coat of the stomach is thickest towards the pylorus‡ and root of the œsophagus; then along the lesser curvature on either side of the mesial line; then along the greater curvature. It is thinnest on the anterior and posterior surfaces, and towards the cardiac end.

* "On the Arrangement of the Muscular Fibres in the Ventricles of the Vertebrate Heart, with Physiological Remarks," by the author, Phil. Trans. part 3. 1864, p. 445.

† "On the Muscular Arrangements of the Bladder and Prostate, and the manner in which the Ureters and Urethra are closed," by the author, Phil. Trans. part 1. 1867.

‡ In the stomach of the Bear the walls of the antrum pylori are fully a quarter of an inch in thickness.

The gradual diminution in the thickness of the coat of the stomach is occasioned by the fibres of one layer or stratum radiating and becoming more and more delicate, while those of another and opposite layer converge and become stronger and stronger; it usually happening that the stronger fibres supplement the weaker ones, so that the parietes, although not of uniform thickness, are not suddenly strong and weak in parts, but graduated. The only sudden thickening occurs in the shape of two ridges which run along the lesser curvature about an inch apart. The ridges in question are very distinct in the stomach of the Cat. They can also be detected in a modified form in the stomach of the Monkey and of Man.

The dissections on which the above communication is based are preserved in the Museum of the Royal College of Surgeons of England; and the paper is illustrated by numerous original figures showing the distribution of the fibres in the stomachs of the Herbivora, Carnivora; and Omnivora.

XVI. "On a Self-acting Apparatus for multiplying and maintaining Electric Charges, with applications to illustrate the Voltaic Theory." By Sir W. Thomson, F.R.S., Glasgow University. Received June 19, 1867.

In explaining the water-dropping collector for atmospheric electricity, in a lecture in the Royal Institution in 1860, I pointed out how, by disinsulating the water-jar and collecting the drops in an insulated vessel, a self-acting electric condenser is obtained. If, owing to electrified bodies in the neighbourhood, the potential in the air round the place where the stream breaks into drops is positive, the drops fall away negatively electrified; or *vice versâ*, if the air potential is negative, the drops fall away positively electrified. The stream of water descending does not in any way detract from the charges of the electrified bodies to which its electric action is due, provided always these bodies are kept properly insulated; but by the dynamical energy of fluid-motion, and work performed by gravity upon the descending drops, electricity may be unceasingly produced on the same principle as by the electrophorus. But, as in the electrophorus, there was no provision except good insulation for maintaining the charge of the electrified body or bodies from which the induction originates. This want is supplied by the following reciprocal arrangement, in which the body charged by the drops of water is made the inductor for another stream, the drops from which in their turn keep up the charge of the inductor of the first.

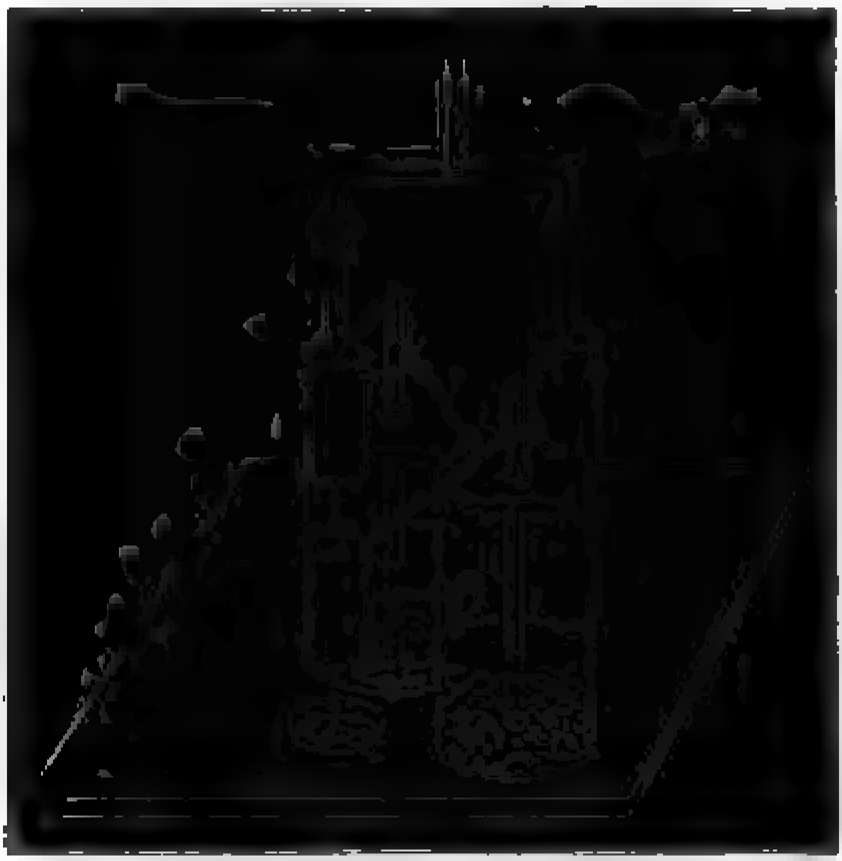
To stems connected with the inside coatings of two Leyden phials are connected metal pieces, which, to avoid circumlocution, I shall call inductors and receivers. Each stem bears an inductor and a receiver

the inductor of the first jar being vertically over the receiver of the second jar, and *vice versâ*. Each inductor consists of a vertical metal cylinder (fig. 1), open at each end. Each receiver consists of a vertical metal cylinder, open at each end, but partially stopped in its middle by a small funnel (fig. 1), with its narrow mouth pointing downwards, and situated a little below the middle of the cylinder. Two fine vertical streams of uninsulated water are arranged to break into drops, one as near as may be to the centre of each inductor. The drops fall along the remainder of the axis of the inductor, and thence downwards, along the upper part of the axis of the receiver of the other jar, until they meet the funnel. The water re-forms into drops at the fine mouth of the funnel, which fall along the lower part of the axis of the receiver and are carried off by a proper drain below the apparatus. Suppose now a small positive charge of electricity be given to the first jar. Its inductor electrifies negatively each drop of water breaking away in its centre from the continuous uninsulated water above; all these drops give up their electricity to the second jar, when they meet the funnel in its receiver. The drops falling away from the lower fine mouth of the funnel carry away excessively little electricity, however highly the jar may be charged; because the place where they break away is, as it were, in the interior of a conductor, and therefore has nearly zero electrification. The negative electrification thus produced in the second jar acts, through its inductor, on the receiver of the first jar, to augment the positive electrification of the first jar, and causes the negative electrification of the second jar to go on more rapidly, and so on. The dynamical value of the electrifications thus produced is drawn from the energy of the descending water, and is very approximately equal to the integral work done by gravity, against electric force on the drops in their path from the point where they break away from the uninsulated water above, to contact with the funnel of the receiver below. In the first part of this course each drop will be assisted downwards by electric repulsion from the inductively electrified water and tube above it; but below a certain point of its course the resultant electric force upon it will be upwards, and, according to the ordinary way of viewing the composition of electric forces, may be regarded as being at first chiefly upward repulsion of the receiver diminished by downward repulsion from the water and tube, and latterly, the sum of upward repulsion of the receiver and upward attraction of the inductor The potential method gives the integral amount, being the excess of work done *against* electric force, above work performed *by* electric force on each drop in its whole path. It is of course equal to m V, if m denote the quantity of elec-

Fig. 1.

tricity carried by each drop, as it breaks from the continuous water above, and V the potential of the inner coating of the lower jar, the potential of the uninsulated water being taken as zero. The practical

Fig. 2.



limit to the charges acquired is either when one or other of them is so strong as to cause sparks to pass across some of the separating air-spaces, or to throw the drops of water out of their proper course and cause them to fall outside the receiver through which they ought to pass. It is curious, after commencing with no electricity except a feeble charge in one of the jars, only discoverable by a delicate electrometer, to see in the course of a few minutes a somewhat rapid succession of sparks pass in some part of the apparatus, or to see the drops of water scattered about over the lips of one or both the receivers.

The Leyden jars represented in the sketch (fig. 2) are open-mouthed jars of ordinary flint glass, which, when very dry, I generally find to insulate electricity with wonderful perfection. The inside coatings consist of strong liquid sulphuric acid, and heavy lead tripods with vertical stems projecting upwards above the level of the acid, which, by arms projecting horizontally above the lip of the jar, bear the inductors and receivers as shown in fig. 2. Lids of gutta percha or sheet metal close the mouth of each jar, except a small air-space of from $\frac{1}{8}$ to $\frac{1}{4}$ of an inch round the projecting stems. If a tube (fig. 3) be added to the lid to prevent currents of air from circulating into the interior of the jar, the insulation may be so good that the loss may be no more than one per cent. of the whole charge in three or four days. Two such jars may be

kept permanently charged from year to year by very slow water-dropping arrangements, a drop from each nozzle once every two or three minutes being quite sufficient.

Fig. 3.

The mathematical theory of the action appended below * is particularly simple, but nevertheless curiously interesting.

The reciprocal electrostatic arrangement now described, presents an interesting analogy to the self-sustaining electromagnetic system recently brought before the Royal Society by Mr. C. W. Siemens and Professor Wheatstone, and mathematically investigated by Professor Clerk Maxwell. Indeed it was from the fundamental principle of this electromagnetic system that the reciprocal part of the electrostatic arrangement occurred to me recently. The particular form of self-acting electrophorus condenser now described, I first constructed many years ago.

* Let c, c' be the capacities of the two jars, l, l' their rates of loss per unit potential of charge, per unit of time, and D, D′ the values of the water-droppers influenced by them. Let $+v$ and $-v'$ be their potentials at time t; v and v' being both of one sign, in the ordinary use of the apparatus described in the text. The action is expressed by the following equations,

$$c\frac{dv}{dt}=D'v'-lv;\qquad c'\frac{dv'}{dt}=Dv-l'v'.$$

If c, D, l, c', D′, l' were all constant, the solution of these equations would be, for the case of commencing with the first jar charged to potential 1, and the second zero,

$$v=\frac{(c'\rho+l')\epsilon^{\rho t}-(c'\sigma+l')\epsilon^{\sigma t}}{c'(\rho-\sigma)},\qquad v'=D\frac{\epsilon^{\rho t}-\epsilon^{\sigma t}}{c'(\rho-\sigma)};$$

with the corresponding symmetrical expression for the case in which the second jar is charged, and the second at zero, in the beginning, the roots of the quadratic

$$(cx+l)(c'x+l')-DD'=0$$

being denoted by ρ and σ. When $ll'>DD'$, both roots are negative; and the electrification comes to zero in time, whatever may be the initial charges. But when $ll'<DD'$, one root is positive and the other negative; and ultimately the charges augment in proportion to $\epsilon^{\rho t}$ if ρ be the positive root.

I may take this opportunity of describing an application of it to illustrate a very important fundamental part of electric theory. I hope soon to communicate to the Royal Society a description of some other experiments which I made seven years ago on the same subject, and which I hope now to be able to prosecute further.

Using only a single inductor and a single receiver, as shown in fig. 1, let the inductor be put in metallic communication with a metal vessel or cistern whence the water flows; and let the receiver be put in communication with a delicate electroscope or electrometer. If the lining of the cistern and the inner metallic surface of the inductor be different metals, an electric effect is generally found to accumulate in the receiver and electrometer. Thus, for instance, if the inner surface of the inductor be dry polished zinc, and the vessel of water above be of copper, the receiver acquires a continually increasing charge of negative electricity. There is little or no effect, either positive or negative, if the inductor present a surface of polished copper to the drops where they break from the continuous water above: but if the copper surface be oxidized by the heat of a lamp, until, instead of a bright metallic surface of copper, it presents a slate-coloured surface of oxide of copper to the drops, these become positively electrified, as is proved by a continually increasing positive charge exhibited by the electrometer. When the inner surface of the inductor is of bright metallic colour, either zinc or copper, there seems to be little difference in the effect whether it be wet with water or quite dry; also I have not found a considerable difference produced by lining the inner surface of the inductor with moist or dry paper. Copper filings falling from a copper funnel and breaking away from contact in the middle of a zinc inductor, in metallic communication with a copper funnel, as shown in fig. 4, produces a rapidly increasing negative charge in a small insulated can catching them below.

Fig. 4.

The quadrant divided-ring electrometer * indicating, by the image of a lamp on a scale, angular motions of a small concave mirror ($\frac{1}{3}$ of a grain in weight) such as I use in galvanometers, is very convenient for exhibiting these results. Its sensibility is such that it gives a deflection of 100 scale-divisions ($\frac{1}{40}$ of an inch each) on either side of zero, as the effect of a single cell of Daniell's; the focusing, by small concave mirrors, supplied to me by Mr. Becker being so good that a deflection can easily be read with accuracy to a quarter of a scale-division By adopting Peltier's method of a small magnetic needle attached to the electric moveable body (or "needle"),

* See Nichol's Encyclopædia, 1860, article "Electricity, Atmospheric:" or Proceedings of the Royal Institution, May 1860; lecture on Atmospheric Electricity.

and by using fixed steel magnets outside the instrument to give directing force (instead of the glass fibre suspension of the divided-ring electrometers described in the articles referred to), and by giving a measurable motion by means of a micrometer screw to one of the quadrants, I have a few weeks ago succeeded in making this instrument into an independent electrometer; instead of a mere electroscope, or an electrometer in virtue of a separate gauge electrometer, as in the Kew recording atmospheric electrometer, described in the Royal Institution lecture.

Reverting to the arrangement described above of a copper vessel of water discharging water in drops from a nozzle through an inductor of zinc, in metallic connexion with the copper, let the receiver be connected with a second inductor, this inductor insulated; and let a second nozzle, from an uninsulated stream of water, discharge drops through it to a second receiver. Let this second receiver be connected with a third inductor used to electrify a third stream of water to be caught in a third receiver, and so on. We thus have an ascending scale of electrophorus action analogous to the beautiful mechanical electric multiplier of Mr. C. F. Varley, with which, by purely electrostatic induction, he obtained a rapid succession of sparks from an ordinary single voltaic element. This result is easily obtained by the self-acting arrangement now described, with the important modification in the voltaic element, according to which no chemical action is called into play, and work done by gravity is substituted for work done by the combination of chemical elements.

XVII. "Note on the Calculus of Chemical Operations." By Professor Williamson. Received June 20, 1867.

XVIII. "Inferences and Suggestions in Cosmical and Geological Philosophy.—Second Series.—On the Luminous Atmosphere of the Sun, exterior to the Photosphere; and on the Probability that the Monochromatic Spectra, from which Mr. Huggins has inferred the Gaseous Constitution of certain Nebulæ, are due in reality to the Luminous Atmospheres of their constituent Stars or Suns." By E. W. Brayley, F.R.S., F.R.A.S., Professor of Physical Geography and Meteorology in the London Institution. Received June 20, 1867.

The Society then adjourned over the Long Vacation to Thursday, November 21.

I. "On the Colouring and Extractive Matters of Urine."—Part I. By EDWARD SCHUNCK, F.R.S. Received June 29, 1865*.

Of all the animal secretions urine is undoubtedly one of the most important. Its varying properties, in health as well as in disease, the frequency with which it is emitted, and the consequent facility with which it may be submitted to examination, render it invaluable to the physiologist and pathologist as a means of throwing light on the processes, either healthy or morbid, going on within the body. Its study has therefore engaged the attention of physicians since the earliest times, and of chemists from the period when chemical analysis was first employed in the examination of natural objects. Notwithstanding the labour bestowed on the subject by many eminent men during the past sixty years, it is still, however, far from being exhausted. There are, indeed, portions of the chemistry of urine concerning which our ignorance is almost complete. It is one of these obscurer parts of the subject that I have endeavoured to clear up, and I hope to succeed in showing that I have added at least a few facts to the sum of our previous knowledge.

Of all the properties of urine none is more obvious, even to the ordinary observer, than its colour. The variations in tint which it exhibits at different times are striking, even to the unpractised eye, and they sometimes serve as important indications to the physician. Nevertheless concerning the chemical nature of the substances to which its colour is due very little is known. Our ignorance on this subject may be ascribed to various causes. In the first place, some of these substances occur in the urine only occasionally, and in very minute quantities, so that the preparation of a quantity sufficient for chemical examination becomes difficult and even impossible, especially when the urine containing them is not abundant. Secondly, it has been found that some of them are very easily decomposed, so much so that the mere heat required for the evaporation of the urine seems to be sufficient to effect a change in their properties and composition. It therefore becomes doubtful, after a long process has been gone through for the purpose of separating any colouring-matter from the other constituents of the urine (a process in which, perhaps, strong chemical reagents have been employed), whether the substance procured was originally contained as such in the urine, or is not rather a product resulting from the decomposition of some other substance or substances. Thirdly, several of the bodies colouring the urine possess very few characteristic properties. They are amorphous and syrup-like, and they retain water with so much pertinacity that on attempting to dry them they undergo decomposition. Neither their compounds nor their products of decomposition exhibit any distinguishing characteristics. They belong to a class on which, for want of a better, the name extractive matter has been

* Read January 11, 1866: see Abstract, vol. xv. p. 1.

conferred. With some chemists, to call a body an extractive matter is to place it among a class which is held to be unworthy of minute examination. To others the name extractive matter is merely a convenient word for a mixture, sometimes occurring in nature, of certain definite, perhaps even crystallized substances, which, by appropriate means, may be resolved into its constituents, and thus be made to disappear entirely from the list of definite chemical bodies. As regards the extractive matter of urine, this view may to some extent be justified, when we recollect that from what was considered to be extractive matter sixty years ago, such well-characterized substances as urea, hippuric acid, and creatine have been successively eliminated; and it is therefore natural to expect that by further research it will be found to contain others of the same nature. I believe this view to be erroneous; and I shall succeed, I hope, in showing that, after having removed from the extractive matter of urine everything which can assume a definite form, there remains a residuum which cannot be further resolved without decomposition. Still, any one holding this view is not likely to undertake the investigation of extractive matters as such, unless it be for the purpose of obtaining something which may be supposed to be contained in them. Lastly, the properties of these colouring and extractive matters, however important they may be to the physiologist and pathologist, present so little that is interesting to the chemist, that the latter would probably not occupy himself with their examination unless for some particular purpose. For myself, I frankly confess that, had I not had a special object in view, this investigation would not have been undertaken. The information for the sake of which it was commenced having been obtained, I should then have abandoned all further inquiry, had I not found reason to suppose, in the course of my experiments, that a more extended investigation would lead to results interesting from a physiological point of view. My endeavours have, I think, been attended with some measure of success; and should physiologists, on becoming acquainted with the results, be of the same opinion, my labour will not have been quite in vain.

The colouring-matters which occurr in, or have been obtained from, urine may be divided into three classes, viz.:—

1st. Those which are only found occasionally in it, in consequence either of disease or of some abnormal state of the system.

2ndly. Those which are produced by spontaneous decomposition, or by the action of reagents on substances, either coloured or colourless, pre-existing in the urine.

3rdly. The colouring-matter or matters occurring in normal urine, and to which its usual colour is due.

A few remarks on the present state of our knowledge on these three classes of pigments, as derived from the labours of my predecessors as well as my own, may not be out of place.

I. The abnormal colouring-matters, which are found ready formed in the urine, may either be peculiar to the secretion, or their presence may be

due to an admixture of blood, bile, or milk, causing the urine to assume various shades of red, green, or white. The latter, as well as those which make their appearance in consequence of the administration of certain drugs, I leave entirely out of consideration. The others, or those peculiar to urine, may be conveniently divided, according to their colour, into three classes, viz., blue, purple or red, and black or brown colouring-matters.

The appearance of a blue colouring-matter in urine has been frequently observed both in ancient and modern times. Cases of its occurrence have been recorded by Janus Plancus *, Delens †, Spangenberg ‡, Prout §, Simon ||, Braconnot ¶, Julia-Fontenelle **, Cantu ††, Reinsch ‡‡, and Du Ménil §§. In all these cases the urine yielded a deposit varying in colour from slate-grey to light blue, or even dark blue, consisting of a blue colouring-matter generally mixed with earthy phosphates. The colouring-matter, after being separated from the impurities with which it was contaminated, was in most of these cases found to have so many properties in common with indigo-blue that several observers, such as Prout and Simon, seemed to have no doubts concerning its identity with the latter. It was, for instance, insoluble in water, but somewhat soluble in alcohol and ether. It was destroyed by nitric acid, but was not affected by other acids, except concentrated sulphuric acid, with which it yielded a blue solution. It was not dissolved by alkalies, except when some reducing agent, such as grape-sugar, was added at the same time. It then dissolved, but was again deposited from the solution on exposure to the air. On being heated, it yielded a violet-coloured vapour. Julia-Fontenelle and Cantu, however, maintain that the colour in the cases examined by them was due to prussian blue; and Angelini |||| suggests that it may possibly be ascribed to phosphate of iron. Lastly, Braconnot has described a blue colouring-matter obtained from urine, which, if his observations are correct, differs entirely from all other pigments derived from the same source. Like indigo-blue it was insoluble in water and alkalies, and only slightly soluble in boiling alcohol; but, on the other hand, it dissolved with ease in dilute acids, forming solutions of a brownish-yellow colour, which, on the addition of an excess of acid, assumed a brilliant red tint. From its solution in acid it was precipitated by alkalies and alkaline earths. To this colouring-matter Braconnot gave the name of *cyanourine*. Since his time, how-

* Commentarii Instituti Bononiensi ad ann. 1767.
† Schweigger's Journal f. Physik u. Chemie, B. xxiii. S. 262.
‡ Ibid. B. xlvii. S. 487.
§ On Stomach and Renal Diseases, 5th ed., p. 567.
|| Simon's Animal Chemistry, translated by Day, vol. ii. pp. 274 & 327.
¶ Annales de Chimie et de Physique, t. xxix. p. 252.
** Archives générales de Médecine, t. ii. p. 104.
†† Mémoires de l'Académie Royal de Turin.
‡‡ Jahrbuch f. Pract. Pharm., B. viii. S. 93.
§§ Archiv d. Pharm., B. xxxix. S. 48.
|||| Giorn. di Fisica, Dec. II. t. viii. (1825).

ever, no one has obtained any substance from urine having exactly the same properties. The urines which deposit the blue colouring-matter are not found to exhibit any peculiarities in other respects, nor does the deposit appear to be characteristic of any peculiar class of diseases. It seems occasionally even to make its appearance during a state of perfect health. Sometimes the deposit seems to contain also another colouring-matter, more easily soluble in alcohol and ether, to which it communicates a fine purple colour.

The deposits of urate of ammonia and urate of soda, which are formed in urine during fever and other diseases, are always found to exhibit different shades of red, varying from pink to carmine. To what this colour is to be attributed has not yet been satisfactorily ascertained. Proust *, who was the first chemist to examine these deposits, thought that he had discovered in them a peculiar coloured acid, which he called *rosacic acid*. It is almost certain, however, that the acid properties of this body were due to an admixture of uric acid. Indeed, Vauquelin, after an examination of this so-called acid, arrived at the conclusion that it was a compound of ordinary uric acid with an intensely red colouring-matter. Vogel †, it is true, professed to have obtained pure rosacic acid by treating the crude deposits with boiling alcohol, but as, according to him, it is converted with great facility into uric acid by the action of sulphuric and nitric acid, it is very probable that his substance still contained some of the latter acid, and that the supposed conversion consisted merely in a destruction of the organic colouring-matter. Fromherz and Gugert ‡ also made some experiments with these red deposits, from which they infer that rosacic acid consists of a neutral, red extractive colouring-matter, mixed with uric acid and urate of soda, which may be separated by treating the mixture first with water and then with warm alcohol, which dissolves the colouring-matter. The latter, after being thus separated from the other constituents, no longer yields uric acid. Prout § suggested that the colour of the red deposits might be due to purpurate of ammonia, the purpuric acid being formed in some unexplained manner by the action of nitric acid on a portion of the uric acid contained in them. To this it was objected by Berzelius ‖ that purpurate of ammonia is insoluble in alcohol. He mixed urate of ammonia with a solution of a purpurate in acetic acid, which does not destroy the colour, and he observed that the precipitated uric acid acquired a pale pink colour closely resembling that of the urinary deposits; but this colour was not removed by boiling alcohol, in which, on the contrary, the colouring-matter of the red deposits is easily soluble. Duvernoy ¶ asserts that he succeeded in preparing a colouring-matter identical with that of the red deposits by evaporating ordinary

* Annales de Chimie, t. xxxvi. p. 265. † Ibid. t. xcvi. p. 306.

‡ Schweigger's Journal, B. l. S. 199. § Annals of Philosophy, vol. xv. p. 155.

‖ Lehrbuch der Chemie, B. ix. S. 421.

¶ Untersuchungen über den menschlichen Urin. Stuttgart, 1835.

healthy urine to one-third or one-fourth of its volume, adding a little nitric acid, allowing it to stand for a day, during which time the colour of the liquid changed from yellowish brown to dark red, and then mixing with a solution of urate of potash. A precipitate was thereby formed of uric acid, having the same red colour as the natural red deposits, from which the red colouring-matter could be extracted by means of alcohol. Recent observers have given names to this colouring-matter, such as *uroerythrine* and *purpurine*, without, however, adding anything of importance to our knowledge of its properties. The method adopted by them for its preparation is essentially the same as that first suggested by the earlier chemists. The deposits containing it are washed with water, and then digested with warm absolute alcohol, which takes up the colouring-matter and, after filtration and evaporation at a temperature not exceeding 50°C., leaves it in the form of a red amorphous residue. It cannot be obtained by evaporating the urine containing it; but on dissolving white and pure urate of ammonia in urine (which by its pink or purple colour indicates the presence of purpurine), the salt is precipitated, on cooling, deeply coloured, and yields the colouring-matter on being treated in the way just described. It is not improbable that this purpurine and the blue colouring-matter just referred to may stand in some relation to one another. An observation made by Angelini* seems to favour this view. This chemist, being desirous of examining the pink deposit which was being formed in his own urine during an attack of fever, had it collected and laid aside; but being unable, from the state of his health, to examine it at once, it remained for some days exposed to the atmosphere, and during this time the pink colour changed in many places into blue. On leaving it to stand for some time longer, the blue tint did not spread further, but the spots became darker in colour.

Instances of black urine are even of rarer occurrence than those of urine coloured blue. Indeed in many cases the black colour seems to have been due to red or purple pigments, which communicated to the urine so deep a tint as to make it appear black. Dulk, for instance, obtained from a black urine a substance of the same colour containing iron, which Berzelius† with some reason suspected to be merely hematine. In the case described by Marcet‡, the urine appears to have been purple, or purplish-brown in the first instance, and to have become black on standing. It contained no red blood-globules and no trace of iron, and yielded no coloured deposit on standing for a length of time, the colouring-matter being kept in solution by the alkali, which was always present in excess. This colouring-matter was examined by Prout, who gave it the name of *melanic acid*. It was precipitated from the urine by means of acids in black flocks, which were found to be nearly insoluble in water and alcohol, but readily soluble in caustic and carbonated alkalies, the solutions being of a very dark colour.

* Giorn. di Fisica, Dec. II. t. viii. (1825).
† Jahresbericht, 20ter Jahrg., S. 576.
‡ Medico-Chirurg. Transactions, 1822.

The solution in ammonia gave copious brown precipitates, with metallic salts. Marcet concludes from the experiments of Prout that this so-called acid bears a close analogy to the products derived from uric acid; but Berzelius remarks that it strongly resembles the black pulverulent substance, insoluble in alcohol, which is formed by the action of concentrated acids on the extractive matters of urine. By heating the urine yielding cyanourine, after separation of the latter by filtration, Braconnot obtained a black sediment which he called *melanourine*. I should at once have assumed that this substance was identical with Prout's melanic acid, if Braconnot had not stated that his black pigment was soluble in weak acids and insoluble in alkalies, whilst the behaviour of melanic acid to acids and alkalies is exactly the reverse. Considering the facility with which the ordinary extractive matters of urine are decomposed, yielding products insoluble in water of a black or brown colour, it is surprising that urines containing these bodies ready formed should not more frequently be met with in cases of disease. It is not improbable, however, that the dark-brown colour of some urinary calculi may be owing to one or the other of these bodies.

II. The second class of urinary colouring-matters comprises those which are formed from urine by artificial means, and consequently do not exist ready formed in the secretion. These may also be classified according to colour, those which have hitherto been observed being either blue, red, or brown.

I believe that Heller * was the first to obtain artificially from urine colouring-matters of a pure blue or red tint. He states, in his first memoir on the subject, that in some diseases the urine contains a notable quantity of a body of a light yellow colour, and easily soluble in water, which he calls *uroxanthine*. When urine containing this body is exposed to oxidizing agencies, such as nitric acid, or even atmospheric air, it deposits a dark-coloured sediment, consisting of a blue and a red colouring-matter, named by him respectively *uroglaucine* and *urorhodine*. The former, after being purified, appears in small groups of crystals of a dark-blue colour, which are insoluble in water, as well as in cold alcohol and ether, but soluble in boiling alcohol. Urorhodine, according to Heller, is formed by a lower degree of oxidation than uroglaucine. It is easily soluble in cold alcohol or ether, to which it communicates a splendid crimson colour, and is always amorphous and apparently of a resinous nature. Uroxanthine, the body from which these colouring-matters are derived, and which, according to Heller, is itself probably derived from urea, is also contained in small quantities in normal urine. Braconnot's cyanourine is, in Heller's opinion, a mixture of uroglaucine and urorhodine. In two subsequent memoirs † Heller communicated some further details on the preparation of these colouring-matters from urine, and on their occurrence in a urinary calculus, without, however, adding any new facts to those previously known regard-

* Heller's Archiv, 1845, S. 161. † Ibid. 1846, S. 19, 536.

ing their chemical or physical properties. The experiments of Alois Martin *, the results of which were made known soon after those of Heller, led to the same conclusion, viz., that in some diseases the urine on being mixed with mineral acids deposits in considerable quantity a dark-coloured sediment, consisting of two colouring-matters, one of which is blue, the other red. Regarding the former, which he calls *urokyanine*, Martin states that it is insoluble in water and caustic alkalies, but soluble in alcohol and ether, that it is dissolved by concentrated sulphuric acid, the solution becoming blue on dilution with water, and that when heated it yields violet-coloured fumes like those of iodine. Although these observations, however incomplete, were no doubt correct, very little importance was attached to them by chemists in general, and their accuracy was even questioned by some. Berzelius characterizes Heller's statements as uncertain and unsatisfactory. Lehmann says, "Heller's experiments were so incomplete that the very existence of such pigments as uroxanthine and urorhodine is still doubtful." Golding Bird was of opinion that Heller had described as crystals of uroglaucine uric acid merely tinted by the changed colouring-matter, and he adds, "This error is an important one, and throws much doubt on many of his conclusions." When a few very simple experiments would have sufficed to prove the accuracy of the observations referred to, or to have shown in what respect they were erroneous, such criticisms as these can hardly be considered fair; and I think that Heller's claims as the discoverer of the artificial formation from urine of a blue and a red colouring-matter of definite character cannot be contested. Golding Bird certainly claims to have been the first to observe the formation of a red or pink colouring-matter, supposed by him to be identical with that of the so-called pink deposits, by the action of hydrochloric acid on healthy urine; but, without deciding the question of priority, I will merely remark that his experiments must have been of a superficial character, or the simultaneous formation of a blue colouring-matter would hardly have escaped his notice. On the other hand, when it is considered that the blue pigment occasionally deposited from urine had, as mentioned above, been proved to be indigo-blue by several of the earlier observers, and that at the time when Heller and Martin gave an account of their experiments the properties and products of decomposition of this colouring-matter were well known, it is surprising that these chemists should not have suspected the identity of uroglaucine, urokyanine, and indigo-blue. A few comparative experiments would have proved their identity, and have thus led to the discovery of one of the most important and interesting facts connected with the chemistry of this subject. How far Heller was from understanding the true nature of his blue colouring-matter will be seen by the following extract from his last memoir. He says, "If a pale yellow urine, rich in uroxanthine, either originally alkaline or alkaline through standing, be kept in a well-corked flask, the violet-coloured sub-

* Heller's Archiv, 1846, S. 191, 287.

stance separates, mostly at the surface, but partly at the bottom. If the flask, while still closed, be shaken, scarcely any change of colour takes place; but if it be shaken after the stopper has been removed and air admitted, the urine becomes, by shaking with the air, more or less green, often very beautifully grass-green. On standing it again becomes pale, and these appearances may be repeated at pleasure with urine that has been kept for months in a flask. This phenomenon, viz., that a strongly alkaline urine containing the mixture of colouring-matters only becomes green by contact with air and not as long as the vessel is closed, is one the cause of which I have not as yet been able to ascertain." Any one acquainted with the properties of indigo-blue would, however, have understood the matter at once. By the combined action of the alkali and the deoxidizing matters contained in the urine, the indigo-blue in Heller's experiment was reduced and dissolved, forming a true indigo-vat, and on admitting air it was reoxidized and precipitated, to be dissolved again when the vessel was closed. Several years later H. v. Sicherer * obtained from a specimen of morbid urine, by the action of strong acids, a blue deposit, the properties of which he found to be those of indigo-blue.

Heller's experiments were followed, after an interval of some years, by those of Hassall †, who observed the formation of a blue colouring-matter on allowing urine from disease to stand for some time exposed to the air. The colouring-matter was mixed with phosphates, mucus, and other impurities; but after the latter had been, as far as possible, removed, it was found to consist of indigo-blue. Hassall inferred from his experiments that the occurrence of this substance in the urine is strictly pathological. "We should be led," he says, "to look for its occurrence in the urine in all those cases of functional derangement of any kind in which any impediment exists to decarbonization, as is the case especially in most diseases of the organs of respiration. . . . . It does not appear that, by any treatment of the urine with reagents, indigo can be developed in healthy urine at will. I have made several attempts with this view, but without obtaining any definite result." This opinion proved, however, to be erroneous.

This subject was next taken up by myself ‡. My experiments on the formation of indigo-blue in plants yielding that colouring-matter led to the conclusion that these plants contain a peculiar substance, belonging to the class of glucosides, which I named *indican.* As this substance is easily soluble in water, alcohol, and ether, and yields, by decomposition with acids, indigo-blue and sugar, I thought it probable that the formation of indigo-blue in urine might be due to the presence of a similar body in the secretion. This supposition was found to be correct. Not being able to procure specimens of morbid urine such as would be likely to yield the colouring-matter, I was compelled to employ healthy urine; but after de-

* Annalen der Chem. und Pharm., B. xc. S. 120.
† Philosophical Transactions, 1854, p. 297.
‡ Memoirs of the Literary and Philosophical Society of Manchester, vol. xiv. p. 239.

priving the latter of the greatest part of the ordinary extractive matter by precipitation with basic acetate of lead, then adding ammonia to the filtered liquid, and acting on the precipitate produced by ammonia with sulphuric or hydrochloric acid, I succeeded in almost every instance in obtaining a small quantity of a colouring-matter, which I had no difficulty in identifying as indigo-blue. The cases in which this did not occur were so few and exceptional that I was led to conclude that indican, or some substance closely resembling it, is a normal constituent of healthy urine, and that it is only the presence of an excess of this, just as of any other of its usual constituents, that can be considered a symptom of disease. The blue colouring-matter was generally accompanied by another, which dissolved in alcohol with a fine purple colour, and which I consider to be identical with Heller's urorhodine. As the indican of plants always yields by decomposition indigo-red as well as indigo-blue, I think it not improbable that this red pigment from urine may consist of indigo-red; but from the difficulty experienced in purifying it, and the very minute quantities which are obtained, this cannot easily be proved. The urine of the horse and the cow yielded the same colouring-matters even in greater abundance than human urine. My experiments have been confirmed by Carter * and others; and it is now, I believe, generally admitted that they afford a means of explaining the formation of the abnormal colouring-matters of the urine, and may even throw some light on the processes of decomposition which the proteine substances undergo in the system. In order to prove the complete identity of Heller's uroglaucine with indigo-blue, Kletzinsky † prepared a large quantity of uroglaucine, and ascertained that its properties and composition are those of indigo-blue, and he accordingly ascribes to Heller the discovery of indigo-blue in urine. I believe, however, that Heller's claims on this field of research cannot be allowed to extend so far. What I think must be conceded to him is, as I stated above, the discovery of a mode of obtaining a blue and a red colouring-matter from urine by artificial means.

The formation of brown colouring-matters by the action of acids on urine was first observed by Proust ‡. Having evaporated fresh urine to a syrup, in order to separate the greatest part of its salts, he added concentrated sulphuric acid to it, and then submitted the liquid to distillation. The distillate contained a large quantity of acetic acid and a little benzoic acid, while the liquid deposited a brown mass of the consistence of pitch, which increased in quantity as the distillation proceeded. This mass consisted chiefly of a resinous body, which he found to be insoluble in water, but easily soluble in alcohol and alkaline liquids. In consistence, colour, and smell it resembled castoreum, and it had a sharp, bitter taste like that of arum-root. Proust believed it to be the substance to which the colour

* Edinburgh Medical Journal, August 1859.

† Schmidt's Jahrbücher d. Medicin, B. civ. S. 36.

‡ Annales de Chimie, t. xxxvi. p. 274; and Annales de Chim. et de Phys. t. xiv. p. 262.

as well as the peculiar odour and taste of urine are due, and he called it the *resin of urine*. The deposit formed in the boiling liquid contained also a black pulverulent body, which he found to be insoluble in water and alcohol, but soluble in alkalies, forming with the latter dark-brown solutions, from which it was precipitated by acids in thick black flocks. When dry, it had a shining appearance resembling that of broken asphalt. Proust called this the *peculiar black substance* from urine; and after some speculations on its nature and origin, he says that probably at some future time the relation, at present quite unknown, in which it stands to other bodies will be discovered. On repeating Proust's experiments, Berzelius obtained nearly the same results; but he was of opinion that these substances are not contained as such in the urine, as Proust had supposed, but are formed by the action of acids on the extractive matters of urine. In this opinion I entirely concur. Scharling's* *oxide of omichmyle* does not seem to me to differ in any of its properties from Proust's resin; but as Scharling, instead of evaporation, employed congelation as a means of concentrating the urine, and then extracted his so-called oxide with ether, there seems some reason for supposing that his substance may have preexisted in the urine. On examining the further details of his process, it will be found, however, that he used boiling caustic lye for the purpose of purifying it; and it need hardly be observed that no conclusion can be drawn regarding the preexistence of any organic compound which has passed through a process of purification involving the use of such an energetic agent as caustic alkali. In the course of his experiments on the constitution of urine, Liebig† also obtained the resinous substance of Proust, and he found it to possess in general the properties previously ascribed to it. The results of this portion of his investigation were summed up in the following words:—"From the preceding it follows that human urine contains, as organic acids, uric acid and hippuric acid, and another nitrogenous substance (most probably the colouring-matter of urine) which, in contact with air (it is only in contact with air that, as already observed by Gay-Lussac, the putrefaction of urine, accompanied by absorption of oxygen, takes place), is decomposed, yielding acetic acid and a resin-like substance."

In my paper on the occurrence of indigo-blue in urine, I gave a short account of some experiments on these brown colouring-matters, and the phenomena attending their formation. I there stated that "when muriatic or sulphuric acid is added to urine, the mixture on being heated becomes brown, and begins to deposit dark-brown flocks, which increase in quantity when the heating is continued. When these flocks are filtered off, washed, and dried, they form a compact dark-brown mass, from which cold alcohol extracts a resinous matter, leaving undissolved a brown powder, which dissolves, however, in a boiling mixture of alcohol and ammonia." These facts were previously known from the researches of Proust. I succeeded,

* Annalen der Chem. und Pharm., B. xlii. S. 265.

† Ibid. B. l. S. 161.

however, in discovering two new facts, to which I attach some importance. The first is, that the composition of the brown pulverulent substance, which is little soluble in alcohol, stands in a definite relation to that of indigo-blue; the second, that the urine, after depositing these flocks and being made alkaline, has acquired the property of reducing oxide of copper, from which it may be inferred that it contains glucose in solution. As the analytical details which led to the discovery of the first fact have not hitherto been published, I think this a fitting occasion for making them known.

The brown pulverulent substance was prepared in the following manner: —Urine was mixed with hydrochloric acid and allowed to stand. The uric acid which was deposited was separated by filtration, and the liquid was boiled for some time. The black powder which separated during the boiling was filtered off, washed with water, dried, and treated with cold alcohol, which extracted the easily soluble resinous portion, thereby acquiring a brown colour. The portion left undissolved by the cold alcohol was dissolved in boiling alcohol to which a little ammonia was added. The brown solution was filtered and mixed with an excess of hydrochloric acid, which produced a brown precipitate, the supernatant liquid remaining coloured. This precipitate was collected on a filter, washed with cold alcohol until the acid and sal-ammoniac were removed, and dried. It had then the appearance of a dull, black, amorphous mass, which yielded a brownish-black powder, strongly resembling some of the products of decomposition of indican. When heated in a crucible it gave off a smell like that of burning horn, and then burned without previously fusing, giving much charcoal, which disappeared without leaving any ash. I need not describe its other properties, as they are in no way characteristic or interesting. Its composition, which is a matter of more importance, was determined by several analyses, the results of which are as follows:—

I. 0·4305 grm., dried at 100° C. and burnt with oxide of copper and oxygen, gave 0·9720 grm. carbonic acid and 0·1985 grm. water.

0·5815 grm., heated with soda-lime, gave 0·4190 grm. platinum.

II. 0·3850 grm., prepared on another occasion, gave 0·8760 grm. carbonic acid and 0·1755 grm. water.

0·5315 grm. gave 0·3685 grm. platinum.

These numbers lead to the formula $C_{14}\,H_7\,NO_4$, which requires—

	Calculation.		Experiment. I.	Experiment. II.
C_{14} ……	84	61·31	61·57	62·05
H_7 ……	7	5·10	5·12	5·06
N ……	14	10·21	10·23	9·85
O ……	32	23·38	23·08	23·04
	137	100·00	100·00	100·00

Now the formula $C_{14}\,H_7\,NO_4$ is also that of anthranilic acid, the acid

formed by the action of alkalies and oxgyen on indigo-blue; and though there is not the least resemblance between the two bodies, still the identity in composition seems to indicate the possibility of a common origin. Is it not possible, it may be asked, that the substance in urine which produces indigo-blue may be in part converted, by a process of oxidation, into some other substance which yields, instead of indigo-blue, a body having the composition of anthranilic acid, *i. e.* of a substance which is formed by the oxidation of indigo-blue? To me it seems very probable that this may be the case. I am, however, far from attaching great importance to the composition of this substance as just given, for on a subsequent occasion I obtained a product having exactly the same appearance as before, but a different composition. On this occasion the method of preparation was somewhat different. The urine was first mixed with acetate of lead as long as a precipitate was produced. To the filtered liquid there was added basic acetate of lead, which gave rise to a second precipitate. This was filtered off, washed with water, and treated with an excess of dilute sulphuric acid, and the filtered liquid, instead of being boiled, was poured into a shallow vessel and left to stand until, by spontaneous evaporation, it had become tolerably concentrated. On now adding cold water, a brown powder was left undissolved, which was filtered off, washed with boiling water, and then treated with boiling alcohol as long as anything was dissolved. The liquid, after being filtered boiling hot, was evaporated, and the residue was treated with a little cold alcohol, which left a brown powder undissolved. This was pressed between folds of blotting-paper and dried, after which it presented the same appearance as the first specimen. Its analysis led to the following results:—

0·3730 grm. gave 0·8295 grm. carbonic acid and 0·1785 grm. water.

0·5750 grm. gave 0·2055 grm. platinum.

The formula $C_{28}\,H_{15}\,NO_{10}$, with which these numbers correspond, requires—

	Calculation.		Experiment.
C_{28}	168	60·64	60·65
H_{15}	15	5·41	5·31
N	14	5·05	5·07
O_{10}	80	28·90	28·97
	277	100·00	100·00

Now the two formulæ, though not identical, stand in a certain relation to one another. If to the first there be added the formulæ of benzoic acid and of water, the sum will represent the second formula, for

$$C_{28}\,H_{15}\,NO_{10}=C_{14}\,H_7\,NO_4+C_{14}\,H_6\,O_4+2\,HO.$$

Benzoic acid is a product of decomposition of hippuric acid and other animal substances, and it need therefore cause no surprise to find its elements among organic groups occurring in animal secretions, though of course its actual presence in this case is doubtful.

These experiments render it probable that the ordinary brown colouring-matters formed by the action of acids on urine are in fact derivatives of indigo-blue, however little their properties may resemble those of the latter. In some experiments, an account of which I presented a short time ago to the Manchester Literary and Philosophical Society [*], I obtained by the direct action of alcohol, acetate of soda, and caustic soda on indigo-blue a number of products, several of which bear a striking resemblance to *uromelanine,* as the brown pulverulent substance obtained by the action of acids on urine has been called, and which differ in their properties from indigo-blue quite as widely as that substance does. But into this part of the subject I cannot enter further at present.

The liquid filtered from the insoluble matter formed by the action of acids on urine I found to possess the property, after being made alkaline, of dissolving oxide of copper and converting it into suboxide on being boiled. This reaction, which had never been previously observed, I attributed to the presence of glucose, which, together with the brown colouring-matters, had been formed at the expense of the extractive matters. The correctness of this inference has been doubted, since the same reaction may be produced by other substances as well as glucose; but whether it be correct or not, the fact remains, that normal urine free from sugar acquires the property of reducing oxide of copper as soon as it has been boiled with the addition of a strong acid. The general conclusion to which I was led by these few experiments was, that there not only exists a great resemblance between indican and the extractive matters of urine, as proved by the similarity of their products of decomposition, but that they are also very probably in some way closely related as regards their composition and general properties. In giving an account of my views on this subject I used these words:—"I think it is probable that the indigo-producing body will be found, as regards its formation and composition, to occupy a place between the substance of the tissues and the ordinary extractive matter of urine." Though this may have appeared at the time when it was pronounced a hasty conclusion, further research has only tended to confirm it.

III. The colouring-matters occurring in normal urine, and to which the usual colour of the secretion is due, have been less frequently submitted to investigation than those which make their appearance only exceptionally, or in consequence of some artificial process of decomposition. This circumstance may easily be accounted for. These substances are all amorphous and possess few characteristic properties; hence their separation from the other constituents of urine is attended with great difficulties, and has even been pronounced impossible. They are also compounds of very little stability, as every one who has worked with them must have observed, so much so that mere evaporation of the urine seems to produce a complete change in their composition, as seen by the marked alteration of colour which takes place during the process. Then it has been observed that

* Memoirs of the Society, 3rd series, vol. iii. p. 66.

normal urine exhibits great diversity of tint without any corresponding difference in its other properties, and hence it has been inferred that these differences are of little physiological or pathological importance, and that an investigation of their cause would not be likely to lead to any useful practical results. Our knowledge of the properties and composition of these substances is therefore extremely defective, and the most discordant views prevail as to their true nature.

Fourcroy and Vauquelin * were of opinion that the smell, colour, taste, and great liability to decomposition of urine, in fact all its characteristic properties, were due to one constituent only, viz. urea. It is evident, however, that their urea must have been impure, since they obtained from it by the action of caustic potash a brown fatty matter and acetic acid, products which could only have been derived from the extractive matter and other impurities with which it was contaminated. It was afterwards shown by Berzelius that urea is colourless, and possesses no remarkable smell nor taste. Proust, as mentioned above, attributed the colour, as well as the bitter taste and peculiar smell of urine, to his fallow resin. Prout thought that the colouring-matter of healthy urine was of two kinds, one of them being capable of combining with urate of ammonia and imparting to it the usual tint of uric acid calculi, the other destitute of this property. To Berzelius †, the great observer who has enriched almost every department of chemical science with his researches, we owe the first, it may almost be said the only, investigation of the extractive matters of urine, the substances to which, as he correctly supposed, the ordinary colour of the secretion is due. This investigation, though now almost forgotten, may still be consulted with advantage, as it contains information not to be found elsewhere. In its main results I have found it remarkably correct, and I shall have occasion to refer to it again. Though Berzelius did not succeed in obtaining his substances in a state of complete purity and free from other constituents of urine, such as urea and chlorides, he nevertheless ascertained the existence of several distinct urinary extractive matters, which were distinguished from one another by their behaviour towards various solvents. One of these he found to be soluble in absolute alcohol, the second was only soluble in alcohol of sp. gr. 0·833, while the third was insoluble in alcohol of all strengths, and only soluble in water. He seems also to have obtained a minute quantity of an extractive matter soluble in ether, the others being insoluble in that menstruum. The extractive matter soluble in absolute alcohol he proposed to name *halophile*, in consequence of its power of combining with various neutral salts. According to Berzelius, these substances bear a great resemblance to the extractive matters of flesh. Duvernoy made some experiments on these extractive matters, and he seems to have been the first to observe the remarkable deepening and change of colour which is seen on adding strong acids to their watery solutions. One of the methods employed by him for separating the colouring or extractive matter

* Annales de Chimie, t. xxxi. p. 68. † Lehrbuch der Chemie, B. ix.

from the other constituents is worth mentioning. He added an excess of acid to urine, and the uric acid which separated on standing he treated with boiling alcohol, which left the acid undissolved, and, after filtration and evaporation, gave a residue consisting of a reddish-brown extractive matter, which had a bitter aromatic taste, and, when warmed, exhaled a urinous odour. The colour of its watery solution was exactly like that of urine.

In his elaborate memoir on urine, Lehmann * makes some remarks on the properties of the extractive matter of urine and the best method of preparing it. For the purpose of obtaining it in a state of purity, he submitted urine to congelation, and evaporated the concentrated liquid *in vacuo*, employing afterwards alcohol and ether for the purpose of extracting it from the residue. No part of the process described by him would induce any extensive decomposition of the substance under examination. On the other hand, it is very doubtful whether it was quite free from impurities, since he attributes to the coloured extractive matter (*färbender Extractivstoff*) of urine the property of inducing decomposition in urea, and consequently in urine also—a property which it certainly does not possess when pure, however liable it may itself be to decomposition. The putrefaction of urine, which manifests itself by the conversion of the urea into carbonate of ammonia, must be caused by some other body. The extractive matter does not act as a ferment, which may indeed be inferred from the very small quantity of nitrogen contained in it. The disagreeable odour which the watery solution of Lehmann's substance began to exhale when exposed to the air also points to some impurity. Its acid reaction he attributes to an admixture of lactic acid, which was generally supposed to be contained in urine, until its entire absence was proved by the experiments of Liebig. Lehmann's observations regarding its other properties, as, for instance, the changes of colour produced in its watery solution by various reagents, are, however, remarkably correct.

Lehmann, as well as Berzelius, found the substance to which healthy urine owes its colour to be completely soluble in water. Subsequently, however, most of the attempts which were made to isolate the colouring-matter of urine ended in the separation of substances quite insoluble in water. These must in all cases have been products of decomposition; for I consider it quite certain that all colouring-matters derived from urine which are insoluble in water are not contained as such in the secretion, provided the latter is in its normally acid state. In the experiments of Scherer † and Harley ‡, various products of decomposition of this kind seem to have been obtained. Scherer, not being satisfied with the methods of preparing and separating the extractive matters given by Berzelius, adopted one of his own, which yielded a brown humus-like substance, insoluble in water, but soluble in alcohol and alkalies. Scherer calls this

* Journal für praktische Chemie, B. xxv. S. 1.

† Annalen der Chemie und Pharmacie, B. lvii. S. 180.

‡ Pharmaceutical Journal, vol. xii. p. 243.

substance the colouring-matter of urine, though it must be evident to any one reading his account that it was a product formed by the action of hydrochloric acid on the extractive matter, and essentially the same as that previously obtained by Proust. Scherer submitted his substance to analysis, and found its composition to vary exceedingly. Hence it may be inferred that it consisted of a mixture of two or more substances. In my experiments on the brown colouring-matters formed by the action of acids, I obtained, as mentioned above, bodies having the same external appearance and general properties, but varying in composition. The latter corresponded on one occasion with the formula $C_{14}H_7NO_4$; on another occasion the analysis led to the formula $C_{28}H_{15}NO_{10}$. Now, on calculating the composition of a mixture of equal parts of the two bodies having respectively these formulæ, it will be found to agree tolerably well with the mean of the two first analyses given by Scherer, as will be seen on comparing his numbers with the calculated composition according to the formula

$$C_{42}H_{22}N_2O_{14}=C_{14}H_7NO_4+C_{28}H_{15}NO_{10}.$$

	Calculation.	Scherer.
C	60·87	61·37
H	5·31	6·10
N	6·76	7·03
O	27·06	25·50
	100·00	100·00

It does not appear that Scherer took the precaution of treating his product with alcohol, in order to separate the easily soluble resinous matter which is always formed together with the pulverulent body when the extractive matters are decomposed by acids. Unless this precaution is taken, the product is sure to contain more than one substance, and its analysis must give very discordant results. On one occasion Scherer obtained by the direct action of hydrochloric acid on urine a dark-blue powder, which when dry assumed a coppery lustre like indigo, and must, indeed, have been indigo-blue itself. The formation of a blue colouring-matter by the action of acids on some constituent of urine had been observed by Heller only a short time previously. Neither of these chemists, however, was aware of its true character, which was not discovered until long afterwards.

Harley * succeeded in separating Scherer's colouring-matter into several substances, to only one of which, in his opinion, the colour of ordinary urine is to be attributed. This, according to his description, is a resinous, amorphous body of a fine red or brownish-red colour, insoluble in water, but easily soluble in alcohol, ether, and caustic alkalies, to which he gave the name of *urohæmatine*. On being incinerated it leaves a little oxide of iron, and hence Harley infers that it is allied to the hæmatine of blood, of which it is perhaps only a modification. By a process similar to that employed by Harley, Marcet † obtained from urine a resinous rose-coloured

* Journal für praktische Chemie, B. lxiv. S. 264.

† Bibl. Univ. de Genève, 1852. p. 144.

substance, having an acid reaction, insoluble in water, but soluble in alcohol, and which he supposed must exist in the secretion in a free state. I have no doubt, however, that this and all similar bodies are products of decomposition derived from the extractive matters or the indigo-producing substance of the urine. They do not preexist in the secretion, but are formed during the process of preparation by the action of the reagents employed. Even a prolonged heating at 100° C. is quite sufficient, as I shall hereafter show, to produce a complete decomposition of the extractive matters, and their conversion into products of an entirely different nature, consisting in great part of brown resinous substances insoluble in water.

The investigations touching this subject which remain to be noticed are few in number. Notwithstanding its importance from a physiological point of view, the difficulties connected with it, and the uncertainty of the conclusions to which most previous researches had led, probably deterred many from entering on its investigation. Tichborne's * account of the normal urinary pigment differs from those of some of his predecessors. According to him the colouring-matter of normal urine is a brown, amorphous substance, which is very hygroscopic, easily soluble in water, less soluble in alcohol, and insoluble in ether. The colour of its watery solution cannot be distinguished from that of ordinary urine, and by making it more or less dilute, the several tints of normal urine may be imitated. Tichborne has given the results of its analysis, which is probably the first ever made of any urinary extractive matter, that is, of the substance as it exists in urine to which the usual colour of the secretion is due. His results, however, differ very widely from those arrived at by myself, and lead to a composition more nearly approaching that of the brown colouring-matters insoluble in water so often obtained in previous experiments. The hypothesis which he has ventured to set up, viz. that this substance is derived in some way from hippuric acid, is, I think, totally without foundation. Indeed there are more reasons in favour of the converse hypothesis, viz. that the urinary extractive matters are the source, or at least one source, of hippuric acid. The existence in urine of more than one kind of extractive matter seems to have escaped the notice of this observer.

By far the most complete investigation of the colouring-matters of normal urine is that of Dr. Thudichum †. The results of this investigation having quite recently been made known, I need not enter at present into any of the details. In giving an account of my own experiments, I shall have occasion to show that my results differ in many respects from those of Dr. Thudichum. I cannot, however, even now refrain from expressing my surprise that, notwithstanding the numerous observations and experiments of chemists on the blue and red colouring-matters from urine, he should have arrived at the conclusion that "from healthy human urine neither indican nor uroxanthine, nor any other substance yielding, by decomposition with

* Chemical News, vol. v. p. 171.

† British Medical Journal, November 5, 1864.

acids, indigo-red and indigo-blue, can be extracted," and that "it yields neither indigo-red nor indigo-blue by boiling with acids." This result, however, may easily be accounted for by any one conversant with the subject who attentively considers the details of his process. The indigo-producing body of urine, if it be not identical with indican, is certainly quite as susceptible of change as the latter; and the small quantity existing in the secretion may easily disappear under the influence of heat, alkalies, or ermentation, and become so changed as no longer to yield indigo-blue or indigo-red with acids. The nature of this change I have explained in my papers on the formation of indigo-blue. Now, Dr. Thudichum's process commences by adding to urine an excess of caustic baryta or lime. At subsequent stages he boils and evaporates his liquids with the assistance of heat. After operations such as he describes it is impossible that any trace of indican, or any body resembling it, can remain undecomposed. Unless certain precautions are adopted in conducting delicate experiments, only negative results can be expected.

The *alkaptone* of Bædeker*, and the colloid acid from urine lately described by Marcet†, probably stand in some relation to the ordinary extractive matters of urine, which they strongly resemble in most of their chemical and physical properties. The nature of the methods employed for the preparation of these bodies renders it, however, extremely doubtful whether they preexisted in the secretion, since in both cases solutions containing, together with the organic substances, strong mineral acids, were heated and even evaporated—a proceeding which must have led to the decomposition of the extractive matters, and the formation of bodies not previously existing.

The preceding account, in which I have endeavoured to present a summary of the results obtained in previous researches, will serve to give an idea of the present state of our knowledge on this subject; and I will now proceed to give an account of my own experiments. Before doing so, I may state that I shall apply the term "colouring-matter" to those bodies only which occur naturally in urine, or are formed by processes of decomposition, and which are insoluble, or not easily soluble, in water. The substances easily soluble in water, to which the colour of normal urine is due, I shall continue to call "extractive matters," until I shall have shown that they are bodies the properties and composition of which are sufficiently definite and unvarying to justify me in bestowing on them peculiar names.

The extractive matters being, as I believe, the source from which most of the colouring-matters of urine are derived, I resolved to commence the investigation by a careful examination of their properties and composition. The first step, indeed, which I thought it necessary to take before proceeding further at all was to ascertain whether these extractive matters are

* Annalen der Chemie und Pharmacie, B. cxvii. S. 98.

† Proceedings of the Royal Society, vol. xiv. p. 1.

bodies of a definite character, or whether they are merely accidental mixtures of various excrementitious substances thrown out by the system, and differing in their nature according to circumstances. In the former case a further exploration of this field would be justified by the probability of arriving at definite results. In the latter case, however, the investigation would certainly have to be abandoned at once, from the want of a secure basis on which to found further research.

In order to arrive at a positive conclusion on this point, the readiest means was, as it seemed to me, to ascertain the exact composition of the extractive matters obtained at different times from various sources; for, being neutral uncrystallizable bodies, it was evident that a mere examination of their chemical and physical properties would lead to no certain result. This portion of the investigation has occupied me for some time, commencing in the year 1856, and will form the subject of the present communication. The successive series of experiments which were made will be distinguished by letters with the respective dates attached.

A (1856).

In this, as well as in the subsequent series of experiments, I made use of neutral and basic acetate of lead for the purpose of separating the extractive matters from the other constituents of urine, the same means having previously been employed with this view by Scherer. Having taken a quantity of ordinary urine, I added to it a solution of acetate of lead, which produced a copious precipitate, consisting of sulphate, phosphate, chloride, and urate of lead, together with lead compounds of extractive matter. This precipitate was filtered off and thrown away. In the filtered liquid, which was lighter in colour than the original urine, basic acetate of lead produced a second precipitate as abundant as the first, and consisting principally of lead compounds of the extractive matters mixed with basic chloride of lead. This precipitate, after being well washed with water, was treated with an excess of cold dilute sulphuric acid, with which it was allowed to remain in contact for some time. The extractive matter set at liberty by the acid communicated to the liquid a brown colour, a peculiar urinous odour being at the same time evolved. The excess of acid was removed by adding carbonate of lead, and stirring the mixture well in a mortar. After all effervescence had ceased, the liquid, which was now of a fine yellow colour like urine itself, was filtered and evaporated; but in order to avoid any decomposition which might have been caused by the application of artificial heat, the evaporation was conducted at the ordinary temperature by means of a current of air in the apparatus formerly employed in the preparation of indican *. After evaporation there was left a thick brown syrup, which was poured into a flask and treated with cold alcohol, with which, after being well shaken, it was left to stand for some time. The alcohol left a portion of this syrup undissolved as a brown glutinous mass (*a*). The liquid, which had a deep yellow colour, was poured off, and there was added to it an alcoholic solution of acetate of lead, which produced a cream-

* See Memoirs of the Manchester Literary and Philosophical Society, vol. xiv. p. 183.

coloured precipitate. Having, as I supposed, added sufficient acetate of lead to precipitate about one half of the matter in solution, I filtered the liquid from the precipitate (*b*), and then added more of the lead solution, which produced a precipitate of a pure cream-colour (*c*). This was filtered off, washed with alcohol, dried *in vacuo*, and submitted to analysis. It contained, like many of the lead compounds subsequently analyzed, chloride of lead, which had to be estimated. By attempting to remove all the hydrochloric acid from the solution of extractive matter before precipitation, I ran the risk of producing decomposition in the organic substance.

I. 1·0895 grm. of this compound, burnt with oxide of copper and oxygen, gave 0·7765 grm. carbonic acid and 0·2175 grm. water.

1·1580 grm., burnt with soda-lime, gave 0·1355 grm. chloride of platinum and ammonium.

0·7780 grm. gave 0·6240 grm. sulphate of lead.

0·9190 grm. dissolved in nitric acid gave with nitrate of silver 0·0795 grm. chloride of silver, equivalent to 0·0772 grm. chloride of lead.

These numbers lead to the following composition :—

C	19·43
H	2·21
N	0·73
O	16·97
PbO	52·26
PbCl	8·40
	100·00

After deducting the oxide and chloride of lead, and calculating the composition in 100 parts of the organic substance combined with them, this composition will be found to correspond with the formula $C_{62}\ H_{39}\ NO_{40}$, which requires

	Calculation.		Experiment.
C_{62}	372	49·93	49·39
H_{39}	39	5·23	5·61
N	14	1·87	1·85
O_{40}	320	42·97	43·15
	745	100·00	100·00

No importance is to be attached to this formula, which is merely an empirical expression for the composition of the substance, or mixture of substances, prepared in the manner described. As a guide to further experiments, the analysis was, however, not without use. The very small proportion of nitrogen obtained showed that the substance was probably free from the urinary constituents containing much of that element, and that the lead compound contained extractive matter only; but whether the latter consisted of only one substance or of several was doubtful, as the method of preparation afforded no guarantee for its purity.

The lead precipitate (*b*), which, it was to be presumed, had the same composition as the one analyzed, was now examined. It was suspended in

water and decomposed with sulphuretted hydrogen, and the filtered liquid was evaporated by means of a current of air in the apparatus above referred to. During evaporation some white crystals were deposited, consisting probably of a product of decomposition formed by the action of the free hydrochloric acid on the extractive matter. These were filtered off, and the liquor was evaporated as before to a syrup, which was treated with cold alcohol. The alcoholic solution, after being filtered from some white crystals which were left undissolved, was evaporated to a syrup, which, after being mixed with a little alcohol for the sake of dilution, was poured into a flask and agitated with successive doses of ether as long as anything was taken up by the latter. The residue left undissolved by the ether (*d*), after the ethereal liquid (*e*) had been poured off, was treated with cold alcohol, which dissolved the whole with the exception of some white crystals *. The filtered liquid was mixed with acetic acid and then with acetate-of-lead solution, and the brown precipitate caused by the latter having been filtered off, there was added to it a small quantity of ammonia, which produced a cream-coloured precipitate. This precipitate was filtered off, washed with alcohol, dried *in vacuo*, and employed for the following analysis:—

II. 1·3735 grm. gave 0·7420 grm. carbonic acid and 0·2345 grm. water.

1·5800 grm. gave 0·1395 grm. chloride of platinum and ammonium.

0·7210 grm. gave 0·6570 grm. sulphate of lead.

1·8070 grm. gave 0·0755 grm. chloride of silver, equivalent to 0·0733 grm. chloride of lead.

These numbers lead to the following composition:—

C	14·73
H	1·89
N	0·55
O	14·99
PbO	63·78
PbCl	4·06
	1·0000

* These crystals consisted probably of the same substance as those deposited during the evaporation of the watery solution filtered from the sulphide of lead. They contained, besides organic matter, a quantity of sulphates of earthy bases. The latter were removed by dissolving the whole in water and adding an excess of caustic baryta. After passing carbonic acid through the filtered liquid, evaporating to dryness, treating the residue with boiling water, filtering, and again evaporating to dryness, a white crystalline mass was obtained, which was free from all inorganic impurities. A few of the properties of this substance may be mentioned, though they are not sufficient to identify it. When heated on platinum-foil it melted and then burned, leaving much charcoal, which, however, disappeared entirely on being further heated. On being heated in a tube, it gave a little crystalline sublimate. It dissolved with difficulty in boiling alcohol, and the solution, on cooling, deposited some transparent, prismatic crystals. The watery solution remained unchanged on the addition of acetate of lead, but on adding ammonia there was an abundant white precipitate. On mixing the watery solution with a salt of copper and an excess of caustic soda it turned blue, but remained unchanged on being boiled.

The simplest formula with which the composition of the substance, combined with the oxide and chloride of lead, agrees is $C_{62}\,H_{46}\,NO_{47}$, which requires

	Calculation.		Experiment.
C_{62}	372	46·03	45·80
H_{46}	46	5·69	5·87
N	14	1·73	1·71
O_{47}	376	46·55	46·62
	808	100·00	100·00

This analysis, though, like the first, of little value in itself, seems to point to the conclusion that the extractive matter has a tendency to undergo a change, which consists in the absorption of water, and which is probably promoted by the action of strong acids. There is, however, another way of explaining its results, which will be given when I come to review the whole of the analytical data.

A portion of the alcoholic solution from which this lead compound was precipitated was evaporated, when it left a brown syrup, some of the properties of which are not without interest. When heated in a crucible it began to boil, evolved acid fumes, consisting partly of hydrochloric acid, and left after combustion much charcoal, which burnt away, leaving a little white ash. The watery solution was strongly acid. After being mixed with a solution of oxide of copper and an excess of caustic alkali it became green, and, on being boiled, the liquid deposited suboxide of copper; but this reaction was probably due to an admixture of some impurity or of some product of decomposition. On adding to the alcoholic solution an alcoholic solution of acetate of lead, a cream-coloured precipitate fell, which, after being filtered off and washed with alcohol, was treated with dilute sulphuric acid. The filtered liquid, after being made alkaline, did not reduce oxide of copper; but, on the other hand, the liquid filtered from the cream-coloured precipitate gave with ammonia a white precipitate, which, on being treated in the same way as the other, was found to contain, in combination with oxide of lead, a substance which, in conjunction with caustic alkali, readily reduced the oxide. The lead compound, the analysis of which has 'ust been given, may indeed, as I shall show further on, have consisted of a mixture of equal parts of two lead compounds, viz. of the compound of an extractive matter and of that of another body having the composition of glucose. The watery solution of the syrup, on being mixed with hydrochloric acid and boiled, became brown, and deposited dark brown flocks. The filtered liquid left on evaporation a residue, which, on being treated with water, dissolved only in part, an additional quantity of brown flocks being left undissolved. These flocks were very little soluble in boiling alcohol, but they dissolved readily in a mixture of alcohol and ammonia. The liquid filtered from the flocks left on evaporation a yellow syrup mixed with a quantity of needle-shaped crystals arranged in star-shaped masses.

On being dried, the syrup became hard, but deliquesced again on exposure to the air.

The ethereal liquid (*e*) containing in solution that portion of the extractive matter of the lead precipitate (*b*) soluble in ether, was agitated with carbonate of lead, by which means the hydrochloric acid contained in it was entirely removed. To the filtered liquid there was added an alcoholic solution of acetate of lead, which produced a cream-coloured precipitate. This was filtered off, washed with cold alcohol, dried *in vacuo*, and analyzed, the results obtained being as follows:—

III. 1·3455 grm. gave 1·0425 grm. carbonic acid and 0·2730 grm. water.
1·5580 grm. gave 0·1385 grm. chloride of platinum and ammonium.
0·7565 grm. gave 0·6025 grm. sulphate of lead.

The compound contained therefore, in 100 parts,

C	21·13
H	2·25
N	0·55
O	17·47
PbO	58·60
	100·00

The composition of the substance, combined in this case with oxide of lead, agrees tolerably well with the formula $C_{86}\ H_{51}\ NO_{52}$, which requires

	Calculation.		Experiment.
C_{86}	516	51·75	51·04
H_{51}	51	5·11	5·43
N	14	1·40	1·34
O_{52}	416	41·74	42·19
	997	100·00	100·00

In the case of a compound like this, having such a high atomic weight, several formulæ may of course be calculated, each of which may give a theoretical composition agreeing as well as the above with that found by experiment. My reasons for adopting the one just given will be stated further on.

A portion of the liquid from which this compound was precipitated with acetate of lead was evaporated, when it left a brown syrup closely resembling that obtained from the liquid from which the lead compound of the second analysis was precipitated. When heated in a crucible it gave off copious fumes, and left off much charcoal, which, however, burnt away, leaving only a trace of ash. Its watery solution had a strongly acid reaction, though it was quite free from hydrochloric acid. When treated with boiling caustic soda lye it evolved ammonia. The watery solution, on the addition of a salt of copper and an excess of caustic alkali, became green, and the filtered liquid, on being boiled, deposited an abundance of

suboxide of copper. This reaction was, however, due to some substance accompanying the extractive matter. On adding to the alcoholic solution of the syrup an alcoholic solution of acetate of lead, a cream-coloured precipitate was produced which contained none of this substance; but on adding ammonia to the filtered liquid, a white precipitate fell in which it was contained in combination with oxide of lead. The lead compound analyzed was therefore free from this impurity, and probably contained merely the urinary extractive matter soluble in alcohol and ether. The watery solution of the syrup, on being mixed with hydrochloric acid and boiled, became darker in colour, and then deposited dark brown resin-like masses, which remained in a state of fusion as long as the liquid was kept boiling. The filtered liquid left on evaporation a syrupy residue, which was only partly soluble in water, a quantity of the resin-like substance being left undissolved. The solution, after being again filtered and evaporated, left a brown syrup filled with crystalline needles. Cold alcohol dissolved the greatest part of this residue, leaving only the crystalline needles undissolved. The resinous substance, after being well washed with water, was treated with cold alcohol, in which it was entirely soluble, forming a brown solution which, on evaporation, left a brown, shining, brittle residue. The extractive matter contained in this compound differs therefore from that of the lead compound previously analyzed, not only by its solubility in ether, but also by its yielding with acids products of decomposition of a different kind.

The alcoholic liquid filtered from the lead precipitate (*b*) was mixed with more acetate of lead and some ammonia, with which it gave a bulky cream-coloured precipitate. This was filtered off, washed with water, then suspended in water, and decomposed with sulphuretted hydrogen. The filtered liquid was evaporated, as before described, by means of a current of air, and the yellow syrup left on evaporation was poured into a flask and agitated with alcohol, which dissolved the whole of it with the exception of a slight residue, consisting of a glutinous substance mixed with some white crystals. The filtered liquid was evaporated, and the residue left was agitated with ether. The ether having been poured off, the insoluble portion was treated with cold alcohol, which dissolved almost the whole of it. The solution was mixed with a little alcoholic solution of acetate of lead, and the precipitate thereby produced having been filtered off, more acetate of lead was added, which gave a precipitate of a pure cream-colour. This was filtered off, washed with alcohol, dried *in vacuo*, and analyzed, the following results being obtained:—

IV. 1·0155 grm. gave 0·6730 grm. carbonic acid and 0·2055 grm. water.

1·1440 grm. gave 0·1425 grm. chloride of platinum and ammonium.

0·4165 grm. gave 0·3180 grm. sulphate of lead.

0·8905 grm. gave 0·0290 grm. chloride of silver, equivalent to 0·0282 chloride of lead.

In 100 parts it contained therefore

C	18·07
H	2·24
N	0·78
O	22·12
PbO	53·63
PbCl	3·16
	100·00

Though the substance combined in this case with the oxide and chloride of lead was without doubt a mixture, still I think it may be of use to devise some formula which shall express its composition, and thus lead to some plausible conjecture as to its constituents. The relatively large quantity of oxygen contained in it makes this rather difficult; but, as I shall presently show, one of the urinary extractive matters is richer in oxygen than the others, and by assuming that the substance in this case was a mixture of equal parts of this extractive matter and glucose, I arrive at the formula $C_{50} H_{39} NO_{41}$, which requires

	Calculation.		Experiment.
C_{50}	300	42·55	41·81
H_{39}	39	5·53	5·18
N	14	1·98	1·80
O_{44}	352	49·94	51·21
	705	100·00	100·00

The liquid filtered from this lead compound did in fact contain a substance having the composition of glucose. On adding to it an excess of ammonia, a bulky precipitate fell, which was filtered off and treated with a mixture of alcohol and acetic acid, in which almost the whole dissolved. To the filtered liquid there was added a small quantity of ammonia, which produced an almost white precipitate. This was filtered off, washed with alcohol, dried *in vacuo*, and analyzed.

1·3730 grm. of this precipitate gave 0·6585 grm. carbonic acid and 0·2265 grm. water.

2·0730 grms. gave 0·0600 grm. chloride of platinum and ammonium.

0·7945 grm. gave 0·7720 grm. sulphate of lead.

In 100 parts the compound contained therefore

C	13·08
H	1·83
N	0·18
O	13·42
PbO	71·49
	100·00

If the small percentage of nitrogen, which was probably due to an admixture of extractive matter be neglected, this composition leads to the formula $C_{24} H_{19} O_{19} + 7PbO$, which requires

C_{24}	144	13·10
H_{19}	19	1·72
O_{19}	152	13·85
7 PbO	784	71·33
	1099	100·00

The brown glutinous mass (*a*) which was left undissolved by cold alcohol was treated with cold water, and the liquid, after being filtered from the insoluble matter (*f*), which consisted for the most part of chloride of lead, was evaporated as usual by means of a current of air. During evaporation some more chloride of lead was deposited, which was separated, and there was left at last a thin syrup, which was poured into a flask and agitated with alcohol. The alcohol converted it into a milky emulsion, which, after standing some time, deposited a brown glutinous substance, the supernatant liquid becoming clear. The latter having been poured off, the deposit was dissolved in cold water, and to the solution there was added acetate of lead and sufficient ammonia to cause a slightly alkaline reaction. The precipitate (*g*) which was thereby produced was filtered off, washed with water, and, after being suspended in water, decomposed with sulphuretted hydrogen. The liquid filtered from the sulphide of lead was evaporated in the air-current, when it left a brown syrup, which was treated with alcohol. The alcoholic liquid, after being filtered from some insoluble matter, consisting of crystals mixed with a little glutinous substance, was evaporated as before to a syrup, and this syrup was poured into a flask and agitated with successive portions of ether until nothing more was dissolved. The ethereal solution (*h*) having been poured off, the insoluble matter was treated with cold alcohol, which dissolved almost the whole of it. To the filtered liquid there was added a little alcoholic solution of acetate of lead; and the precipitate thereby produced having been filtered off, more lead solution was added, which gave a copious precipitate (*i*) of a greyish cream-colour. This, after being filtered off and treated in the same manner as the other lead precipitates, was analyzed, the results obtained being as follows :—

V. 1·3455 grm. gave 0·8455 grm. carbonic acid and 0·2830 grm. water.

1·5710 grm. gave 0·2490 grm. chloride of platinum and ammonium.

0·8830 grm. gave 0·7065 grm. sulphate of lead.

1·4975 grm. gave 0·2580 grm. chloride of silver, equivalent to 0·2508 grm. chloride of lead.

In 100 parts it was therefore composed as follows :—

C	17·13
H	2·33
N	0·99
O	17·38
PbO	45·43
PbCl	16·74
	100·00

If the formula $C_{38}H_{28}NO_{29}$ be adopted as expressing the composition of the organic substance combined with the oxide of lead, the relation in which the latter stands to the other extractive matters will be easily seen, though the calculated composition does not in this case agree very well in all respects with that deduced from the above analysis. This formula requires

	Calculation.		Experiment.
C_{38}	228	45·41	45·30
H_{28}	28	5·57	6·17
N........	14	2·78	2·63
O_{29}	232	46·24	45·90
	502	100·00	100·00

It may excite some surprise that the extractive matter contained in this compound, which was soluble in alcohol, should have been obtained from the brown glutinous mass (*a*), which was insoluble in alcohol, and had quite the appearance of an extractive matter itself. This may, however, be easily explained, since the mass (*a*) contained lead, being, indeed, merely a lead compound of the extractive matter soluble in alcohol, and many of the compounds of the latter with bases are insoluble in alcohol. It is, in fact, still doubtful whether urine does contain an extractive matter insoluble in alcohol, and whether the various substances having this character, obtained in these and previous experiments, are not compounds of extractive matters with bases; but to this point I shall return on a future occasion.

The ethereal solution (*h*) contained some extractive matter, and also hydrochloric acid. The latter having been removed by introducing carbonate of lead and shaking well, there was added after filtration an excess of an alcoholic solution of acetate of lead, which produced a copious precipitate. This was filtered off and then dissolved in a mixture of alcohol and acetic acid. To the filtered solution a small quantity of ammonia was added. The precipitate thereby produced was filtered off and treated as usual before being analyzed.

VI. 1·2650 grm. of this compound gave 0·6860 grm. carbonic acid and 0·2025 grm. water.

1·4445 grm. gave 0·0810 grm. chloride of platinum and ammonium.

0·4610 grm. gave 0·4250 grm. sulphate of lead.

In 100 parts it contained therefore

C.........................	14·79
H	1·77
N	0·35
O.........................	15·26
PbO	67·83
	100·00

[illegible] the
excess of ammonia, which produ
This was filtered off, washed with
decomposed with sulphuretted hydr
into a flask, and there was added t
oxide of mercury, together with son
frequently shaken. By this means t
was completely removed, after which t
The residue was treated with water,
undissolved. Through the filtered liqu
in order to precipitate the mercury
filtered it was evaporated, when it lef
treated with alcohol, which dissolved
filtered alcoholic solution there was ad
which produced an almost white preci
prepared as usual for analysis.

VII. 0·8440 grm. of this compound
and 0·2110 grm. water.

1·1035 grm. gave 0·0950 grm. chloric

0·4535 grm. gave 0·3155 grm. sulpha

In 100 parts it contained therefore

C	....
H	....
N	....
O	....
PbO	....

The

one substance in combination with oxide of lead. Several other compounds similar to this were subsequently analyzed, and it will therefore conduce to clearness to pass them all under review together.

The insoluble matter (*f*), consisting chiefly of chloride of lead, still remained to be examined. After being washed with water, it was treated with cold dilute sulphuric acid. The liquid filtered from the sulphate of lead was mixed with an excess of baryta-water, which produced a brown precipitate consisting of sulphate of baryta mixed with some compound of extractive matter. To the filtered liquid there was added a slight excess of acetic acid, and then acetate of lead and sufficient ammonia to neutralize the acid. This gave a pale yellow precipitate (*i*), which was filtered off, washed, and decomposed with sulphuretted hydrogen. The resulting solution was filtered and evaporated. The residue left on evaporation was treated with alcohol, which left a little white matter undissolved. To the filtered liquid there was added an alcoholic solution of acetate of lead, which gave a yellowish precipitate. This was filtered off, washed with alcohol, and treated as usual previous to its being analyzed.

VIII. 1·2980 grm. of this compound gave 0·9555 grm. carbonic acid and 0·2520 grm. water.

1·5655 grm. gave 0·2575 grm. chloride of platinum and ammonium.

0·7115 grm. gave 0·5320 grm. sulphate of lead.

1·2690 grm. gave 0·1350 grm. chloride of silver, equivalent to 0·1312 grm. chloride of lead.

In 100 parts it contained therefore

C	20·07
H	2·15
N	1·03
O	19·70
PbO	46·71
PbCl	10·34
	100·00

The substance combined with the oxide and chloride of lead contained, in 100 parts,

C	46·74
H	5·02
N	2·40
O	45·84
	100·00

To the liquid filtered from the lead precipitate (*i*), which was employed for the preparation of the last compound, there was added more acetate of lead and an excess of ammonia. The bulky precipitate thereby produced was filtered off, completely washed with water, and then decomposed with sulphuretted hydrogen. The filtered liquid, which had the colour of urine, was mixed with oxide of mercury and left to stand for some time. It was then filtered again and agitated with metallic mercury, by which means the

chloride of mercury contained in it was converted into subchloride, and the liquid was rendered free from chlorine. The filtered liquid, which was almost colourless, was evaporated in the usual manner by means of an air-current, until its volume was considerably diminished. During evaporation a little white matter was deposited which was filtered off. The mercury contained in the solution was removed by means of sulphuretted hydrogen, and after being filtered it was evaporated as before to a syrup. This syrup was found to be insoluble in cold alcohol. It was therefore dissolved in a little water. To the solution there was added a little acetate of lead, which gave a slight precipitate, and this having been filtered off, the liquid was mixed with alcohol until no more precipitate was produced. This precipitate, which was white, was filtered off, washed with alcohol, and dried *in vacuo*. On being analyzed it yielded the following results:—

IX. 1·2825 grm. gave 0·6845 grm. carbonic acid and 0·2145 grm. water.
1·5205 grm. gave 0·0705 grm. chloride of platinum and ammonium.
0·7470 grm. gave 0·6690 grm. sulphate of lead.
In 100 parts it contained therefore

C	14·55
H	1·85
N	0·29
O	17·42
PbO	65·89
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	42·67
H	5·44
N	0·85
O	51·04
	100·00

The two last analyses will suffice to show that the chloride of lead deposited during the evaporation of the liquid obtained from the precipitate, which basic acetate of lead produces in urine after the addition of neutral acetate, takes down with it a portion of the extractive matter, and that the composition of this portion is similar to that of the rest which is retained in solution.

This series of experiments leads to the conclusion that the precipitate produced in urine by basic acetate of lead contains, in combination with oxide and chloride of lead, at least two extractive matters, one of which is soluble in alcohol and ether, the other soluble in alcohol only, but very similar to one another in all other respects; and that it also affords a substance which has the composition and some of the properties of glucose, this being probably a product derived from one or both of the extractive matters, and not preexisting in the urine or even in the lead precipitate itself.

B (1857).

Having by the previous experiments determined in a general way the composition of the precipitate produced in urine by basic acetate of lead, I now resolved to ascertain whether the precipitate with neutral acetate of lead contains any urinary extractive matter in addition to the sulphate, phosphate, chloride, and urate of lead, of which it chiefly consists. For this purpose acetate of lead was added to urine, and the precipitate thereby produced was washed with water and then treated with an excess of dilute sulphuric acid, with which it was left to stand for some time. To the filtered liquid, which had a deep yellow colour, there was added sufficient baryta-water to remove the sulphuric acid, and then an excess of milk of lime, which gave a gelatinous precipitate consisting chiefly of phosphate of lime. The liquid, which had now lost much of its colour, was filtered and made acid with acetic acid. Acetate of lead now produced no precipitate but on the addition of ammonia a cream-coloured precipitate fell, which after being washed was treated with dilute sulphuric acid. The excess of acid was removed by means of carbonate of lead, and the filtered liquid was evaporated in the usual manner in a current of air to a syrup. This syrup was dissolved in cold alcohol, and to the solution there was added an alcoholic solution of acetate of lead. The precipitate thereby produced was filtered off, washed with alcohol, and then treated with dilute sulphuric acid. The excess of the latter having been removed as before by means of carbonate of lead, the filtered liquid was evaporated. The residue which was left was treated with cold alcohol, and the alcoholic solution, after being filtered, was mixed with twice its volume of ether, which caused it to become milky. After some time a glutinous deposit settled at the bottom of the vessel, leaving a supernatant liquid, which was bright yellow and clear. To this liquid there was added an alcoholic solution of acetate of lead, which produced a cream-coloured precipitate. This was filtered off, washed with alcohol, dried *in vacuo*, and analyzed as usual, the results being as follows:—

I. 0·9805 grm. gave 0·8425 grm. carbonic acid and 0·2320 grm. water.

1·1795 grm. gave 0·1850 grm. chloride of platinum and ammonium.

0·6775 grm. gave 0·5000 grm. sulphate of lead.

0·7280 grm. gave 0·0120 grm. chloride of silver, equivalent to 0·0166 grm. chloride of lead.

Hence the compound contained, in 100 parts,

C	23·43
H	2·62
N	0·98
O	18·34
PbO	53·03
PbCl	1·60
	100·00

The composition of the substance, combined in this case with oxide and chloride of lead, corresponds, if the great excess of hydrogen be disregarded, with the formula $C_{62}H_{36}NO_{37}$, which requires

	Calculation.		Experiment.
C_{62}	372	51·81	51·66
H_{36}	36	5·01	5·79
N	14	1·95	2·17
O_{37}	296	41·23	40·38
	718	100·00	100·00

This formula differs, as will be seen, only by three atoms of water from that to which the first analysis of the preceding series led, though, like the latter, it does not represent a pure unmixed substance. It may therefore be inferred that the precipitate produced in urine by neutral acetate of lead contains the same extractive matters as the precipitate which basic acetate of lead gives in the filtrate. This conclusion was confirmed in a very satisfactory manner by subsequent experiments.

C (1857).

The facility with which the hydrochloric acid derived from the chloride of sodium of the urine is removed from solutions of urinary extractive matters containing the acid, by means of oxide of mercury and metallic mercury, led me to try this method of purification on a somewhat larger scale than before. For this purpose a quantity of urine was mixed with acetate of lead, and to the liquid filtered from the precipitate basic acetate of lead was added, which produced, as usual, a second precipitate. This precipitate was filtered off, washed, and treated with dilute sulphuric acid. The excess of acid having been removed by means of carbonate of lead, the filtered liquid was evaporated in the air-current. The residue left on evaporation was treated with cold water, which left undissolved a mixture or compound of chloride of lead and extractive matter. This was filtered off, and the lead still contained in solution was precipitated by a current of sulphuretted hydrogen. The filtered liquid containing hydrochloric acid was now agitated with freshly precipitated oxide of mercury, to which some metallic mercury was added. As soon as it had become free from chlorine, it was again filtered and evaporated in the air-current. The glutinous residue left on evaporation was treated with cold alcohol, in which only a trace dissolved. That which was left undissolved by the alcohol was now dissolved in water, and through the solution, which contained an abundance of mercury, a current of sulphuretted hydrogen was passed, and the filtered liquid was evaporated. The residue was treated with cold alcohol. A pale yellow glutinous substance was left undissolved, which was dissolved in water. The watery solution was mixed with ammonia, with which it gave a flocculent precipitate. To the filtered liquid there was added acetate of lead, and the precipitate thereby produced was filtered off, washed, and

dissolved in acetic acid. The filtered solution was mixed with a large quantity of alcohol, which produced a pale cream-coloured precipitate. This was filtered off and prepared for analysis in the usual manner.

I. 1·3755 grm. of this compound gave 0·8125 grm. carbonic acid and 0·2770 grm. water.

2·0605 grms. gave 0·1175 grm. chloride of platinum and ammonium.

0·6830 grm. gave 0·5785 grm. sulphate of lead.

These numbers lead to the following composition:—

C	16·11
H	2·23
N	0·35
O	18·99
PbO	62·32
	100·00

The composition of the organic substance, combined with the oxide of lead, may be expressed by the formula $C_{86}\,H_{71}\,NO_{76}$, which requires

	Calculation.		Experiment.
C_{86}	516	42·68	42·75
H_{71}	71	5·87	5·93
N	14	1·15	0·95
O_{76}	608	50·30	50·37
	1209	100·00	100·00

It will be seen that the Analyses VII. and IX. of Series A, which were made with compounds prepared in the same manner as this, led to almost the same composition. It must not be inferred from this that these were all compounds of a pure substance with oxide of lead. It is more probable that the oxide of mercury employed in their preparation had simply the effect of removing certain substances from the solution with which it was brought into contact, leaving a mixture of others, the quantities of which stood in a certain unvarying ratio to one another. That the above formula represents a mixture containing glucose is very probable, as I shall afterwards show; and, on the other hand, it is certain that a great part of the extractive matters entered into combination with the oxide of mercury, as the solutions, after being shaken up with the oxide, became several shades lighter in colour. It is also not improbable that oxide of mercury causes the extractive matters to undergo a certain degree of oxidation—a conclusion to which the large percentage of oxygen yielded by the analysis just given, as well as the two others, seems to point. In my subsequent experiments I therefore ceased to employ oxide of mercury except as a means of purifying the extractive matter insoluble in ether and alcohol.

The mixture or compound of chloride of lead and extractive matter obtained as usual in this experiment was employed for the preparation of a compound containing less chloride of lead, the process being the same as

that adopted in preparing for Analysis VIII. of Series A. It was also analyzed, but the results present nothing of interest.

D (1857).

The lead compounds next analyzed were procured from the same source as those of Series A. Urine was mixed with a solution of acetate of lead as long as a precipitate was produced, and to the filtered liquid basic acetate of lead was added, which gave, as usual, an abundant precipitate. This was allowed to settle, washed, filtered off, and then treated with cold dilute sulphuric acid. The excess of the latter having been removed by means of carbonate of lead, the filtered liquid was evaporated in the usual manner by means of a current of air. The syrup which was left behind was dissolved in a little water, and the solution was mixed with a large quantity of alcohol. To the filtered liquid there was added an alcoholic solution of acetate of lead and some ammonia. The precipitate thereby produced was filtered off, washed with alcohol, and then treated with dilute sulphuric acid. The excess of acid having been removed with carbonate of lead, the filtered liquid was evaporated in the usual manner to a syrup. This syrup was poured into a flask together with a little alcohol, and a large quantity of ether was then added, which caused the separation of a glutinous substance that was slowly deposited at the bottom of the vessel. After the ethereal solution had become clear, it was poured off from the glutinous deposit (*a*) and evaporated, and the residue was treated with water, which left a little fatty matter undissolved. The filtered liquid was evaporated in the usual manner to a syrup, which was dissolved in alcohol. On the addition of acetate of lead and a little ammonia to this solution, a cream-coloured precipitate fell, which was filtered off, washed with alcohol, dried *in vacuo*, and analyzed. It contained no trace of chloride of lead.

I. 0·9905 grm. gave 0·7145 grm. carbonic acid and 0·2030 grm. water.

1·2625 grm. gave 0·1285 grm. chloride of platinum and ammonium.

0·6400 grm. gave 0·5265 grm. sulphate of lead.

These numbers correspond, in 100 parts, to

C	19·67
H	2·27
N	0·63
O	16·90
PbO	60·53
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	49·83
H	5·75
N	1·62
O	42·80
	100·00

It will be seen that the composition of the substance combined with oxide

of lead is nearly the same as that to which Analysis I. Series A. led, and which was expressed by the formula $C_{62}\,H_{39}\,N\,O_{40}$. It may also be remarked as a singular circumstance, that the quantity of oxide of lead found in the present analysis is equal to the sum of the oxide of lead and chloride of lead (52·26+8·40) of the former. Hence it may be inferred that in these compounds the chloride of lead and oxide of lead replace one another weight for weight, and not according to their respective equivalents. The same circumstance occurred on several other occasions*, otherwise it might have been attributed to mere accident.

The glutinous substance (*a*), insoluble in the mixture of alcohol and ether, was treated with water. The liquid, after being filtered from some undissolved matter, consisting chiefly of chloride of lead, was mixed with a solution of acetate of lead and a large quantity of alcohol, which produced a cream-coloured precipitate. This was filtered off, and after being treated in the usual manner, submitted to analysis, the results being as follows:—

II. 1·5515 grm. gave 0·9650 grm. carbonic acid and 0·3295 grm. water.

2·0575 grms. gave 0·3430 grm. chloride of platinum and ammonium.

0·9105 grm. gave 0·6875 grm. sulphate of lead.

1·1730 grm. gave 0·2340 grm. chloride of silver, equivalent to 0·2274 grm. chloride of lead.

These numbers lead to the following composition:—

C	16·96
H	2·35
N	1·04
O	20·24
PbO	40·02
PbCl	19·39
	100·00

The composition of the organic portion of the compound may in this case be represented by the formula $C_{38}\,H_{29}\,NO_{34}$, which requires

	Calculation.		Experiment.
C_{38}	228	41·99	41·79
H_{29}	29	5·34	5·81
N	14	2·57	2·57
O_{34}	272	50·10	49·83
	543	100·00	100·00

The mixture of extractive matter and chloride of lead obtained in this case was treated in the manner before described, and a compound was obtained which was also analyzed; but the details need not be given as

* For example, the percentage of oxide of lead of the sixth analysis of Series A (67·83) is equal to the sum of the oxide of lead and chloride of lead of the second analysis of the same series (67·84), the amount of other constituents yielded by the two analyses being nearly the same.

they possess no interest. I may state, however, that it contained 40 per cent. of oxide of lead and 35 per cent. of chloride of lead, and that the composition of the remaining 25 per cent. of organic substance did not differ very widely from that to which the preceding analysis conducted.

E (1858).

After having added to urine an excess, first of acetate and then of basic acetate of lead, the liquid is found to have lost the greatest part of its colour. Nevertheless it produces with ammonia a bulky precipitate, which is similar in appearance to the two other lead precipitates, and also contains some extractive matter. The object of this series of experiments was to ascertain whether the composition of this portion of extractive matter is the same as that of the portion contained in the two other precipitates. A quantity of urine was accordingly mixed with acetate of lead, and then with basic acetate of lead, until the latter gave no more precipitate. After the liquid had become clear, it was decanted and mixed with an excess of ammonia. The precipitate thereby produced was allowed to settle, filtered off, completely washed, and then treated with dilute sulphuric acid in the cold. The excess of the latter was removed by means of carbonate of lead, and the filtered liquid was evaporated as usual by a current of air. The residue left on evaporation was treated with cold water, and the liquid, after being filtered from the mixture of chloride of lead and extractive matter (*a*) left undissolved, was evaporated as before. The syrupy residue now left was well shaken with cold alcohol, which left a portion (*b*) undissolved. The filtered liquid was evaporated, and the residue having been dissolved in a little alcohol, the solution was mixed with a large quantity of ether, which made it milky and produced a copious syrup-like deposit (*c*). This was allowed to settle, and the ethereal liquid was poured off and evaporated. The residue left on evaporation was dissolved in cold alcohol, and to the solution there was added an alcoholic solution of acetate of lead, which produced a precipitate containing much chloride of lead. The addition of a little ammonia to the filtered liquid gave rise to a second precipitate, which was filtered off, washed, and prepared in the usual manner for analysis.

I. 0·8405 grm. gave 0·5000 grm. carbonic acid and 0·1240 grm. water.

1·1070 grm. gave 0·1595 grm. chloride of platinum and ammonium.

0·5575 grm. gave 0·5285 grm. sulphate of lead.

In 100 parts it contained therefore

C....................	16·22
H....................	1·63
N....................	0·90
O....................	11·50
PbO..................	69·75
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C....................	53·63
H....................	5·41
N....................	2·99
O....................	37·97
	100·00

This analysis yielded so large an amount of nitrogen and so little hydrogen and oxygen as compared with the preceding analyses, that it becomes difficult to bring it into harmony with the latter without having recourse to very improbable hypotheses. This discordance would, however, not be sufficient to justify the conclusion that the extractive matter soluble in ether contained in the precipitate with ammonia has a different composition from that prepared in the same way from the two other lead precipitates. By subsequent experiments it was indeed rendered very probable that they are identical in composition. It may therefore be inferred that in this case the discrepancy was due to some error, perhaps analytical. I should have hesitated in giving the details of this analysis had I not been desirous of presenting, without making any selection, the whole of the evidence on which my final conclusions are based.

The syrup-like deposit (*c*), insoluble in ether, was dissolved in alcohol. The solution was mixed with an alcoholic solution of acetate of lead, and the precipitate thereby produced was filtered off, washed with alcohol, and treated with dilute sulphuric acid. The excess of acid was removed by means of carbonate of lead, and the filtered liquid was evaporated in the air-current. The residue left on evaporation was treated with cold water, which left a quantity of chloride of lead undissolved; and to the filtered liquid there was added acetate of lead, and then a large quantity of alcohol. This gave a precipitate which, after being treated in the usual manner, was analyzed, the results obtained being as follows:—

II. 1·1540 grm. gave 0·6320 grm. carbonic acid and 0·2050 grm. water.

1·7405 grm. gave 0·2700 grm. chloride of platinum and ammonium.

0·3005 grm. gave 0·2570 grm. sulphate of lead.

0·7200 grm. gave 0·1015 grm. chloride of silver, equivalent to 0·0986 grm. chloride of lead.

In 100 parts it contained therefore

C....................	14·93
H....................	1·97
N....................	0·97
O....................	16·51
PbO..................	51·92
PbCl	13·70
	100·00

The substance combined with oxide and chloride of lead contained, in 100 parts,

C	43·43
H	5·73
N	2·82
O	48·02
	100·00

Several subsequent analyses led to the same composition as this. I shall therefore defer for the present giving the corresponding formula.

The syrupy matter, insoluble in cold alcohol (*b*), was treated with cold water, which left a quantity of gelatinous matter undissolved. Through the filtered liquid sulphuretted hydrogen was passed in order to precipitate the lead in solution, and after being again filtered it was evaporated in the air-current to a syrup, which was treated with cold alcohol as long as anything was dissolved. A portion (*d*) was left undissolved. To the filtered liquid there was added an alcoholic solution of acetate of lead, which produced a cream-coloured precipitate. This was filtered off and treated as usual before being analyzed.

III. 1·1430 grm. gave 0·5310 grm. carbonic acid and 0·1720 grm. water.

1·6965 grm. gave 0·1100 grm. chloride of platinum and ammonium.

0·6470 grm. gave 0·6245 grm. sulphate of lead.

1·0900 grm. gave 0·0485 grm. chloride of silver, equivalent to 0·0471 grm. chloride of lead.

In 100 parts it contained therefore

C	12·67
H	1·67
N	0·40
O	13·39
PbO	67·55
PbCl	4·32
	100·00

The substance combined with oxide and chloride of lead contained, in 100 parts,

C	45·04
H	5·94
N	1·44
O	47·58
	100·00

The portion of the syrupy residue (*d*) which was left undissolved by cold alcohol in the preparation of the preceding compound was dissolved in cold water, and to the solution acetate of lead and alcohol were added, which produced a dirty-white precipitate. This was filtered off, washed with alcohol, suspended in water, and decomposed with sulphuretted hydrogen. The filtered liquid was evaporated in the air-current, and the syrupy residue was treated with alcohol, which left a portion of it undissolved. The latter,

after the liquid had been poured off, was dissolved in water, and the solutiomixed with acetate of lead and alcohol. The dirty-white precipitate thereby produced was filtered off, washed, dried, and analyzed.

IV. 0·5750 grm. gave 0·5355 grm. carbonic acid and 0·2010 grm. water.

0·7260 grm. gave 0·2595 grm. chloride of platinum and ammonium.

0·3585 grm. gave 0·1930 grm. sulphate of lead.

In 100 parts it contained therefore

C	25·39
H	3·88
N	2·24
O	28·91
PbO	39·58
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	42·02
H	6·42
N	3·70
O	47·86
	100·00

The results obtained in this analysis approximate to those yielded by Analysis II., Series D.

The mixture of chloride of lead and extractive matter (*a*) which was obtained in this series of experiments was also examined, and a lead compound was prepared from it which was submitted to analysis. The results, however, do not possess sufficient interest to make them worthy of communication. They served to show that the organic portion of the mixture did not differ in composition very widely from the extractive matters, the compounds of which had been previously analyzed.

It was about this time that I made a discovery of some interest connected with the chemistry of urine. I was occupied with the examination of the lead precipitate produced by ammonia in the liquid filtered from the precipitate with basic acetate of lead, and having treated it in the manner above described for the purpose of separating the extractive matters contained in it, I obtained an alcoholic solution of the latter, to which I added, as usual, a quantity of ether, and I observed after some time, mingled with the glutinous deposit produced by the ether, a quantity of crystalline matter. On pouring off the liquid and adding a little cold water to the deposit, the crystalline portion was left undissolved, and after filtration and washing had the appearance of a silky mass of a brownish hue. By dissolving it in boiling water and adding animal charcoal, the colour was removed; and on evaporating the filtered liquid I obtained a substance, crystallizing in white silky needles, which had the properties of tyrosine. It was very little soluble in cold water, and crystallized from the solution in

boiling water in snow-white masses consisting of star-shaped groups of needles. It gave, on being tried by Piria's method for discovering tyrosine, a very decided reaction. Its watery solution also gave, with nitrate of mercury, the reaction peculiar to tyrosine.

Among my collection of products from urine, there is one also belonging to this period; but by what means it was procured I cannot state, as I can find no memoranda relating to it among my notes. All that I can say regarding its preparation is, that it was obtained, like the other, from the lead precipitate with ammonia, and I think by a similar process. It is, however, totally different in its properties. It consists of regular, colourless crystals, and has a sweet taste. It dissolves in boiling water, but is not easily soluble in cold water. Its watery solution, on the addition of a salt of copper and an excess of caustic soda, turns blue; but no suboxide of copper is deposited on boiling the liquid. From its giving, when treated in the manner described by Scherer, with nitric acid, then with chloride of calcium and ammonia, the pink colour characteristic of inosite, I conclude that it consists of that peculiar species of sugar.

It is well known that both tyrosine and inosite are found in the urine in disease; and as the urine employed in my experiments was secreted by a great number of individuals, it seemed not improbable that among those individuals there might be some whose urine contained those bodies as the result of some morbid condition. In this case it would be the precipitates with basic acetate of lead and with ammonia in which these substances would be found, as they are neither of them precipitated by neutral acetate of lead. It is possible, however, that their occurrence may be due to some peculiar decomposition undergone by the extractive matters. I shall return to this point when I come to describe the properties of the latter.

F (1860).

The investigation had now reached a point at which, in my opinion, no advantage was to be derived from attempting to devise new methods of preparing the bodies under examination for analysis. I preferred going over the ground again, employing the same methods as before, and obtaining, if possible, some confirmation of the previous results.

The series of experiments now to be described consists of a renewed examination of the three lead precipitates in which the urinary extractive matters are contained—that produced in urine with neutral acetate of lead, that with basic acetate of lead in the liquid filtered from the first, and that with ammonia in the liquid filtered from the second precipitate.

The precipitate with acetate of lead was treated in the manner described in giving an account of the experiments of Series B. After being washed it was decomposed with sulphuric acid; the excess of the latter was removed from the filtered liquid by means of caustic baryta, and the phosphoric acid was precipitated by adding milk of lime. The filtered liquid having been mixed with an excess of acetic acid, acetate of lead and ammonia were

added to it, producing a cream-coloured precipitate. This was filtered off, washed with water, and treated with dilute sulphuric acid. The excess of the latter having been removed by means of carbonate of lead, the filtered liquid was evaporated in the air-current. The chloride of lead which was deposited during evaporation was filtered off, and the syrupy residue which was left at last was treated with cold alcohol. The liquid was poured off from the undissolved portion and evaporated, and the syrupy residue having been again dissolved in a little alcohol, the solution was mixed with a large quantity of ether, which threw down a portion of the matter in solution. The liquid, which was of a golden-yellow colour, was poured off from the insoluble deposit and evaporated to a syrup. This syrup was poured into a flask and agitated with a quantity of ether. After standing for some time, the ether, which had dissolved a portion of the syrup, was poured off and evaporated. The residue, which was free from compounds of chlorine, was dissolved in alcohol, and to the solution there was added an alcoholic solution of acetate of lead, which produced a precipitate of the usual colour. This was filtered off, washed with alcohol, dried *in vacuo*, and analyzed, the following results being obtained:—

I. 1·1080 grm. gave 0·9095 grm. carbonic acid and 0·2500 grm. water.
1·5635 grm. gave 0·1360 grm. chloride of platinum and ammonium.
0·7160 grm. gave 0·5430 grm. sulphate of lead.

These numbers lead to the following composition:—

C	22·38
H	2·50
N	0·54
O	18·78
PbO	55·80
	100·00

The composition of the substance combined with oxide of lead corresponds in this case with the formula $C_{86}\,H_{53}\,NO_{54}$, which requires

	Calculation.		Experiment.
C_{86}	516	50·83	50·65
H_{53}	53	5·22	5·67
N	14	1·37	1·22
O_{54}	432	42·58	42·46
	1015	100·00	100·00

On a previous occasion the analysis of the lead compound of the extractive matter soluble in ether led to the formula $C_{86}\,H_{51}\,NO_{52}$, which differs from the above by two equivalents of water. This difference might be ascribed to a more or less perfect desiccation of the lead compound; but I think it is more probably due to an absorption in one case of the elements of water, a process which often takes place with bodies of this class. From the extractive matter insoluble in ether but soluble in alcohol I also prepared a

lead compound, but the quantity obtained was not sufficient for a complete analysis.

The precipitate with basic acetate of lead was submitted to a process which did not differ from that just described, except in being rather simpler on account of the absence of phosphoric acid. It yielded by this treatment four substances; viz. one soluble in ether (*a*), a second soluble in alcohol but insoluble in ether (*b*), a third insoluble in a mixture of alcohol and ether (*c*), and a fourth insoluble in alcohol as well as in ether (*d*). The substance soluble in ether (*a*) was dissolved in absolute alcohol, and to the solution there was added an alcoholic solution of acetate of lead, which gave a precipitate of the usual colour. This was filtered off and prepared in the same manner as before for analysis.

II. 0·7215 grm. of this compound gave 0·6155 grm. carbonic acid and 0·1695 grm. water.

1·1295 grm. gave 0·1215 grm. chloride of platinum and ammonium.

0·4820 grm. gave 0·3680 grm. sulphate of lead.

In 100 parts it contained therefore

C	23·26
H	2·61
N	0·67
O	17·29
PbO	56·17
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	53·06
H	5·95
N	1·52
O	39·47
	100·00

The composition of the substance, combined in this case with oxide of lead, differs, as will be seen, very widely from that of the extractive matter soluble in ether as determined by previous experiments. This want of accordance is surprising, and I can only attribute it to some error in the analysis. The ratio between the nitrogen, the carbon, and the hydrogen is about the same as usual, and the discrepancy may therefore have arisen from an error in the estimation of the oxide of lead. There was not sufficient material left for another determination.

The substances (*b*) and (*c*), which I supposed to be essentially the same, were dissolved together in absolute alcohol. The solution was filtered from a small quantity of insoluble matter, and there was added to it an alcoholic solution of acetate of lead, which produced a precipitate of the usual colour. This was filtered off, washed, dried, and analyzed with the following results:—

III. 0·5855 grm. gave 0·4100 grm. carbonic acid and 0·1230 grm. water.

0·7835 grm. gave 0·1625 grm. chloride of platinum and ammonium.

0·4300 grm. gave 0·3265 grm. sulphate of lead.

0·5085 grm. gave 0·0800 grm. chloride of silver, equivalent to 0·0777 grm. chloride of lead.

These numbers lead to the following composition :—

C	19·09
H	2·33
N	1·30
O	18·39
PbO	43·60
PbCl	15·29
	100·00

The composition of the extractive matter contained in this lead compound corresponds with the formula $C_{38}H_{27}NO_{28}$, which requires

	Calculation.		Experiment.
C_{38}	228	46·24	46·44
H_{27}	27	5·47	5·66
N	14	2·83	3·16
O_{28}	224	45·46	44·74
	493	100·00	100·00

The substance (*d*) was treated with cold water, which dissolved the whole of it, with the exception of some chloride of lead which was filtered off. Sulphuretted hydrogen was passed through the liquid in order to precipitate the lead in solution; and having been again filtered, it was stirred in a mortar with sulphate of silver, by which means the hydrochloric acid as well as the excess of sulphuretted hydrogen contained in it were removed. Through the filtered liquid sulphuretted hydrogen was passed in order to precipitate the silver in solution. It was then filtered again, agitated with carbonate of lead in order to remove the sulphuric acid, filtered, freed from excess of lead by sulphuretted hydrogen, filtered again, and then evaporated in the air-current to a syrup. This syrup having been dissolved in water, acetate of lead was added to the solution, which after filtration was mixed with a large quantity of alcohol. This produced a precipitate, which was filtered off, washed with alcohol, suspended in water, and decomposed with sulphuretted hydrogen. The filtered liquid was evaporated in the air-current to a syrup, which was treated with cold alcohol until all the soluble matter was removed. The portion insoluble in alcohol* was dissolved in

* A part only of this was employed in the preparation of the lead compound. The remainder was kept in a flask for a considerable time. It was then found to have deposited a quantity of crystalline matter, which remained undissolved on the addition of cold water, but was soluble in boiling water. The boiling solution, after being decolorized with animal charcoal and filtered, deposited, on cooling, a quantity of tyrosine in white crystalline needles.

water, and to the solution acetate of lead was added, which gave a slight precipitate. The filtered liquid was mixed with a large quantity of alcohol, which produced a cream-coloured precipitate. This was filtered off and prepared in the usual manner for analysis.

IV. 1·0485 grm. gave 0·6810 grm. carbonic acid and 0·2195 grm. water.
1·5440 grm. gave 0·2755 grm. chloride of platinum and ammonium.
0·6120 grm. gave 0·4820 grm. sulphate of lead.
In 100 parts it contained therefore

C	17·71
H	2·32
N	1·12
O	20·90
PbO	57·95
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	42·11
H	5·51
N	2·66
O	49·72
	100·00

It will be seen that this analysis yielded numbers not differing very widely from those of Analysis II. Series D. The composition of the extractive matter combined with oxide of lead corresponds in both cases with the formula $C_{35} H_{29} NO_{34}$. A comparison of the two analyses affords also a further corroboration of what has been stated above, viz. that in the lead compounds containing chloride of lead, the latter replaces the same weight, not an equivalent quantity of oxide of lead. The difference between the amount of oxide of lead of the one compound and that of the chloride and oxide taken together of the other, is in this case greater than in those before referred to.

The precipitate produced by ammonia in the liquid filtered from that with basic acetate of lead still remained to be examined. In consequence, however, of a change of residence and other unforeseen circumstances the investigation suffered a lengthened interruption at this stage, and was only recommenced after an interval of two years. During this time the precipitate was kept in well-stoppered bottles covered with water, so as to preserve it moist and out of immediate contact with the atmosphere. It had undergone no perceptible change. It was therefore filtered off and treated with dilute sulphuric acid. The excess of acid was removed by means of carbonate of lead, and the filtered liquid was evaporated at a moderate temperature in a hot-air stove instead of, as hitherto, by means of a current of air, which I now no longer had the means of producing of the requisite strength. The residue left after evaporation was treated with cold water, which left a quantity of chloride of lead of a dirty-yellow colour undis-

solved. The liquid was filtered, sulphuretted hydrogen was passed through it, and after being filtered from the precipitated sulphide of lead, a boiling solution of sulphate of silver was added to it as long as any precipitate of sulphide or chloride of silver was produced. The excess of silver was removed from the filtered liquid by sulphuretted hydrogen, and the liquid having been again filtered was agitated with carbonate of lead, which took away the sulphuretted hydrogen and the sulphuric acid contained in it. It was then filtered, sulphuretted hydrogen was passed through it, and after being filtered from the sulphide of lead, it was evaporated in the hot-air stove to a syrup. This syrup was then poured into a flask, a little alcohol and a large quantity of ether were added, and the whole was well shaken. After standing for some time the ethereal liquid was poured off from the undissolved portion of the syrup (*e*) and evaporated. The residue left after evaporation was treated with water, and the resulting solution was filtered from a little fatty matter which was left undissolved and evaporated. The residue was poured into a flask and treated with ether. After standing some time the liquid was poured off from the undissolved syrup-like matter (*f*) and evaporated, and the residue was treated again with ether, which left a little more syrup-like matter undissolved. The residue now left was dissolved in alcohol, and to the solution acetate of lead and ammonia were added. The precipitate thereby produced was filtered off, washed with alcohol, and treated with a little dilute acetic acid, in which it was for the most part soluble. Some brown flocks which were left undissolved were filtered off, and the liquid was mixed with a large quantity of alcohol which produced a pale cream-coloured precipitate. This was filtered off, washed, dried, and analyzed, the following results being obtained:—

V. 1·2335 grm. gave 0·8460 grm. carbonic acid and 0·2375 grm. water.
1·5440 grm. gave 0·1175 grm. chloride of platinum and ammonium.
0·7570 grm. gave 0·6265 grm. sulphate of lead.

These numbers lead to the following composition:—

C	18·70
H	2·13
N	0·47
O	17·81
PbO	60·89
	100·00

The composition of the substance combined with oxide of lead agrees tolerably well with the formula $C_{86} H_{59} NO_{60}$, which requires

	Calculation.		Experiment.
C_{86}	516	48·27	47·81
H_{59}	59	5·52	5·44
N	14	1·30	1·20
O_{60}	480	44·91	45·55
	1069	100·00	100·00

This formula represents a substance containing several atoms more water than the extractive matter soluble in ether, the composition of which was arrived at by Analysis III. Series A. The tendency to absorption of water by this body was already indicated by Analysis I. of the present series. Here it has taken place to a still greater extent, and has probably attained the extreme possible limit. The absorption of water in this instance I was at the time inclined to attribute to the evaporation of the solutions containing the extractive matters having been conducted at a higher temperature than before. I now think it more probable that it was due to the length of time during which the lead precipitate with ammonia was kept in contact with water.

The syrup-like matter insoluble in alcohol and ether (*e*) and that insoluble in ether alone (*f*) were mixed together and treated with alcohol. After standing for some time the liquid was poured off from some glutinous substance (*g*), which was left undissolved, and evaporated. The residue left on evaporation was treated with warm absolute alcohol, which dissolved the greatest part of it. To the solution there was added a little acetate of lead, and after being filtered from the precipitate it gave, with an excess of alcoholic lead solution, an abundant precipitate, which after being treated in the usual manner was analyzed.

VI. 0·8800 grm. of this precipitate gave 0·7235 grm. carbonic acid and 0·2335 grm. water.

1·4885 grm. gave 0·3300 grm. chloride of platinum and ammonium.

0·2160 grm. gave 0·1415 grm. sulphate of lead.

In 100 parts it contained therefore

C	22·42
H	2·94
N	1·39
O	25·05
PbO	48·20
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	43·28
H	5·69
N	2·68
O	48·35
	100·00

The glutinous substance (*g*) left undissolved by alcohol was treated with water, and the resulting dark-brown solution was filtered from some undissolved gelatinous matter and agitated with oxide of mercury, which took up a great deal of the colour, leaving the solution of a pale yellow tint. After filtration sulphuretted hydrogen was passed through it in order to precipitate the mercury in solution, and after being again filtered it was evaporated, when it left a pale yellow amorphous residue. This was treated

with alcohol, and the portion left undissolved by the alcohol was dissolved in water. To the watery solution acetate of lead was added, and the filtered liquid was mixed with a large quantity of alcohol, which produced a pale cream-coloured precipitate. This was filtered off and prepared in the usual manner for analysis.

VII. 1·0625 grm. of this precipitate gave 0·7060 grm. carbonic acid and 0·2400 grm. water.

2·0080 grms. gave 0·2505 grm. chloride of platinum and ammonium.

0·8545 grm. gave 0·6210 grm. sulphate of lead.

In 100 parts it contained therefore

C	18·12
H	2·50
N	0·78
O	25·13
PbO	53·47
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	38·94
H	5·40
N	1·68
O	53·98
	100·00

These numbers seem to point to the conclusion that the extractive matter insoluble in alcohol had also absorbed a considerable quantity of water, in consequence of the long-continued contact of the mixed lead compounds with water. As there seemed, however, some reason to suspect the presence of glucose in the compound of this analysis, I thought it hardly worth while to devise any formula to represent its composition.

G (1862).

The next series of experiments was made with urine obtained from the surgical wards of the Manchester Infirmary. By employing material derived from an entirely different source, I hoped to obtain further confirmation of the results previously arrived at. I refrained, however, from using any truly morbid urine for fear of introducing an element of uncertainty into the experiments. The urine supplied to me, through the kindness of the medical officers of the Institution, did not differ perceptibly from that of healthy individuals, the appearance, colour, and reaction being quite normal.

The urine was first mixed with a solution of acetate of lead, and the precipitate thereby produced was separated and treated in the manner before described, for the purpose of getting rid of the phosphoric and uric acid contained in it; and by means of acetate of lead and ammonia a precipitate was then obtained containing, besides chloride of lead, only compounds of

the extractive matters. This was then added to the lead precipitate produced by ammonia in the urine after being filtered from the precipitate with acetate of lead. The mixture contained, therefore, the extractive matters of the three lead precipitates, which had before been separately examined. It was treated in the manner described near the conclusion of the preceding section, that is to say, it was acted on by dilute sulphuric acid; the hydrochloric acid set at liberty was removed by means of sulphate of silver, the excess of silver was precipitated as sulphide, the sulphuric acid was got rid of with carbonate of lead, and the lead in solution having been precipitated with sulphuretted hydrogen, the liquid was evaporated, leaving a residue containing the extractive matters, which were separated from one another by means of alcohol and ether. In this way I obtained a substance soluble in alcohol and ether (*a*), a second soluble in alcohol but insoluble in ether (*b*), and a third insoluble in alcohol and ether (*c*). From the first a lead compound was prepared in the usual manner, the analysis of which led to the following results:—

I. 0·9530 grm. gave 0·6695 grm. carbonic acid and 0·1665 grm. water.
1·3260 grm. gave 0·0995 grm. chloride of platinum and ammonium.
0·5250 grm. gave 0·4500 grm. sulphate of lead.

In 100 parts it contained therefore

C	19·15
H	1·94
N	0·47
O	15·37
PbO	63·07
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	51·88
H	5·25
N	1·27
O	41·60
	100·00

It will be seen that the composition of the extractive matter of this compound may be expressed by the formula $C_{36} H_{31} NO_{52}$, the same to which Analysis III. Series A led.

The substance *b* was treated in the cold with absolute alcohol, which left a portion of it undissolved. The solution was evaporated, and the residue was treated as before with absolute alcohol. The residue left after the second evaporation was dissolved in water. Acetate of lead was added to the solution, and the filtered liquid was mixed with alcohol, which produced a precipitate. This was treated as usual and then analyzed.

II. 1·0980 grm. gave 0·7935 grm. carbonic acid and 0·2410 grm. water.
1·6135 grm. gave 0·2740 grm. chloride of platinum and ammonium.
0·4765 grm. gave 0·3550 grm. sulphate of lead.

These numbers lead to the following composition:—

C	19·70
H	2·43
N	1·06
O	22·00
PbO	54·81
	100·00

The composition of the substance combined with oxide of lead agrees very well with the formula $C_{38}H_{27}NO_{32}$, which requires

	Calculation.		Experiment.
C_{38}	228	43·42	43·59
H_{27}	27	5·12	5·37
N	14	2·66	2·34
O_{32}	256	48·80	48·70
	525	100·00	100·00

Several of the preceding analyses gave a composition corresponding more or less closely with the same formula, as I shall show when I come to give a summary of the whole of the results obtained.

The substance *c*, which was insoluble in alcohol, was treated with cold water. The resulting solution was filtered from some gelatinous matter (consisting of silica), which was left undissolved, and then agitated with oxide of mercury. After filtration, sulphuretted hydrogen was passed through it in order to precipitate the mercury in solution, and after being filtered it was evaporated. The glutinous residue left on evaporation was dissolved in a very small quantity of water, and the solution was mixed with a large quantity of alcohol, which precipitated a substance of a glutinous nature. After standing for some time the liquid was poured off, and the precipitated matter was washed with alcohol and then dissolved in a little water. To the solution there was added acetate of lead, which produced a dirty-yellow precipitate; and the addition of a considerable quantity of alcohol to the filtered liquid gave rise to a second precipitate, which was filtered off, washed, dried, and analyzed as usual.

III. 1·1780 grm. of this precipitate gave 0·8090 grm. carbonic acid and 0·2660 grm. water.

2·0705 grms. gave 0·3585 grm. chloride of platinum and ammonium.

0·7485 grm. gave 0·5595 grm. sulphate of lead.

In 100 parts it contained therefore

C	18·72
H	2·50
N	1·08
O	22·70
PbO	55·00
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	41·60
H	5·55
N	2·40
O	50·45
	100·00

Two of the preceding determinations, viz. Analysis II. Series D and Analysis IV. Series F, led to nearly the same results, the composition of the substance contained in the lead compound corresponding in all three cases with the formula $C_{38}H_{29}NO_{34}$.

Being curious to ascertain what had been taken up by the oxide of mercury which was employed in the purification of the substance used in the preparation of the last lead compound, it was, after being washed, suspended in water and decomposed by a current of sulphuretted hydrogen. The filtered liquid was evaporated to a syrup. This syrup, which was very brown, was dissolved again in a very little water, and the solution was mixed with a large quantity of alcohol, which produced a glutinous deposit. The latter, after the liquid had been poured off, was dissolved in a little water, and to the solution there was added acetate of lead, which gave a dirty-yellow precipitate. The filtered liquid was mixed with a large quantity of alcohol, and the resulting precipitate was filtered off, washed, dried, and analyzed.

0·7170 grm. of this precipitate gave 0·5365 grm. carbonic acid and 0·1650 grm. water.

1·0915 grm. gave 0·6240 grm. chloride of platinum and ammonium.

0·3615 grm. gave 0·2595 grm. sulphate of lead.

In 100 parts it contained therefore

C	20·40
H	2·55
N	3·59
O	20·65
PbO	52·81
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	43·24
H	5·41
N	7·60
O	43·75
	100·00

From the unusually large amount of nitrogen yielded by this analysis, it must be concluded that the oxide of mercury took up some substance differing in composition from the extractive matters, probably a product of decomposition of the latter.

This series of experiments confirms in a remarkable manner the results

previously obtained, and serves to show that the composition of the extractive matters does not vary with the source whence they are derived.

H (1863).

In order to remove any doubt that might still have remained in regard to the composition of the urinary extractive matters, I made another series of experiments, employing for this purpose ordinary healthy urine. The urine was mixed as usual with acetate of lead, and then with basic acetate, but the precipitate with the latter was alone made use of. This, after being washed with water, was treated in the manner before described, and yielded as usual a substance soluble in ether (*a*), a second soluble in alcohol but insoluble in ether (*b*), a third insoluble in a mixture of alcohol and ether (*c*), and a fourth insoluble both in alcohol and in ether (*d*). The first of these was treated with water, which left a quantity of fatty matter undissolved. To the filtered liquid there was added acetate of lead; and having been again filtered, it was mixed with a large quantity of alcohol, which produced a precipitate of the usual appearance. The analysis of this precipitate yielded the following results:—

I. 1·0945 grm. gave 0·8540 grm. carbonic acid and 0·2115 grm. water.
1·5585 grm. gave 0·1265 grm. chloride of platinum and ammonium.
0·6600 grm. gave 0·5255 grm. sulphate of lead.

In 100 parts it contained therefore

C	21·28
H	2·14
N	0·50
O	17·50
PbO	58·58
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	51·37
H	5·16
N	1·20
O	42·27
	100·00

If this composition be compared with that yielded by Analysis III. Series A, it will be seen that the difference between them is not greater, even as regards the amount of oxide of lead, than is usually found in two analyses of the same substance. This near approximationi n the results of the first and the concluding series of experiments, as far as regards one of the extractive matters, is remarkable.

The substances *b* and *c*, though probably one and the same, were examined separately, in order to remove all doubt as to the identity of their composition. The former was treated with absolute alcohol, in which it was entirely soluble. To the solution there was added a little acetate of lead, and the dark-brown precipitate produced by the latter having been

filtered off, an excess of alcoholic-lead solution gave an abundant precipitate, which was filtered off and then treated with acetic acid, less acid being taken than would have sufficed to dissolve it entirely. The filtered liquid was mixed with a large quantity of alcohol, which gave a precipitate of a pure cream-colour. This was analyzed in the usual manner.

II. 1·1365 grm. gave 0·9295 grm. carbonic acid and 0·2695 grm. water.

1·5655 grm. gave 0·2595 grm. chloride of platinum and ammonium.

0·6855 grm. gave 0·4585 grm. sulphate of lead.

In 100 parts it contained therefore

C	22·30
H	2·63
N	1·04
O	24·82
PbO	49·21
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	43·90
H	5·15
N	2·04
O	48·91
	100·00

The substance *c* was treated with alcohol. The liquid was poured off from the portion left undissolved and evaporated. The residue was dissolved in a little alcohol, and the solution was mixed with a large quantity of ether, which precipitated a syrupy mass. After the latter had settled, the liquid was poured off, and the syrup was dissolved in water. Acetate of lead was added to the solution, and the filtered liquid was mixed with a quantity of alcohol. The precipitate thereby produced was filtered off, washed, dried, and analyzed, the results being as follows:—

III. 1·1475 grm. gave 0·8800 grm. carbonic acid and 0·2610 grm. water.

1·5390 grm. gave 0·2995 grm. chloride of platinum and ammonium.

0·7300 grm. gave 0·5135 grm. sulphate of lead.

In 100 parts it contained therefore

C	20·91
H	2·52
N	1·22
O	23·60
PbO	51·75
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	43·33
H	5·22
N	2·52
O	48·93
	100·00

Hence it follows that the substances *b* and *c* have the same composition, a composition corresponding with the formula $C_{38}\,H_{27}\,NO_{32}$, the same to which Analysis II. of the preceding series conducted.

The substance *d* was treated with cold water. The resulting dark-brown solution was filtered from some gelatinous matter, which remained undissolved and was found to consist chiefly of silica, and then mixed with an excess of acetate of lead. This produced a dark-brown precipitate, which was filtered off. A little ammonia and a large quantity of alcohol were then added to the liquid, and the bulky cream-coloured precipitate produced was filtered off, washed with alcohol, suspended in water, and decomposed with sulphuretted hydrogen. The filtered liquid was evaporated, and the brown glutinous residue which was left was dissolved in water. The addition of acetate of lead to the solution produced a brown precipitate. The filtered liquid gave, on being mixed with alcohol, a cream-coloured precipitate, which was filtered off, washed, dried, and analyzed as usual.

IV. 1·1760 grm. gave 0·9445 grm. carbonic acid and 0·3165 grm. water.

1·6280 grm. gave 0·5300 grm. chloride of platinum and ammonium.

0·6715 grm. gave 0·4330 grm. sulphate of lead.

In 100 parts it contained therefore

C	21·90
H	2·99
N	2·04
O	25·63
PbO	47·44
	100·00

The substance combined with oxide of lead contained, in 100 parts,

C	41·66
H	5·68
N	3·88
O	47·78
	100·00

The unusually large amount of nitrogen yielded by this analysis was probably due to an admixture of the impurity which in the former experiments was removed by means of oxide of mercury, and which contains, as I have shown, more than seven per cent. of that element. Had I employed the same method of purification as before, the composition would probably have corresponded more closely with that to which Analysis III. of the preceding series and several previous determinations led, and which may be represented by the formula $C_{38}\,H_{29}\,NO_{31}$.

The numerical results obtained by these determinations are, I think, sufficiently numerous and concordant to allow of definite conclusions being drawn regarding the composition of the urinary extractive matters.

[redacted]

II. "On the Colouring and Extractive Matters of Urine."—Part II. By Edward Schunck, F.R.S. Received June 20, 1866*.

Before entering on a consideration of the analytical details contained in the first part of this memoir, it will be necessary to determine the exact number of urinary extractive matters, the existence of which I am justified in assuming after having brought the examination of their composition to a close. Berzelius, as I have before stated, inferred from his experiments that human urine contained three distinct extractive matters, one soluble in absolute alcohol, another soluble only in alcohol of sp. gr. 0·833, and a third insoluble in alcohol of all strengths, and only soluble in water. A similar conclusion would probably be arrived at by any one perusing the account which I have given of my own experiments without at the same time possessing any information regarding the chemical nature of the compounds analyzed. Nevertheless this conclusion would be incorrect, since a few simple experiments suffice to prove that the extractive matter insoluble in alcohol is not a distinct substance, but invariably contains a quantity of alkaline or earthy bases, on the removal of which the organic matter with which they were combined becomes soluble in alcohol, and that the extractive matters peculiar to urine are only two in number, the first being soluble in ether and alcohol, the second soluble in alcohol only. This inference is quite consistent with the results derived from the analyses of the lead compounds obtained on various occasions from the extractive matter insoluble in alcohol, the details of which have been given in the first part of this memoir.

A specimen of the extractive matter insoluble in alcohol was prepared in the following manner :—Ordinary urine was mixed with acetate of lead, and the precipitate thereby produced having been separated, basic acetate of lead was added to the liquid, and the second lead precipitate was filtered off, washed, and treated with dilute sulphuric acid. The excess of the latter was removed by means of carbonate of lead, and the filtered liquid was evaporated to a syrup, which was treated with alcohol. The latter left a portion of the syrup undissolved; and this portion, after being washed with alcohol, was again dissolved in water and sulphuretted hydrogen was passed through the solution, which, after being filtered from the precipitated sulphide of lead, was treated with peroxide of mercury and metallic mercury, for the purpose of removing the hydrochloric acid contained in it. After standing some time and being frequently agitated the liquid was filtered, and sulphuretted hydrogen was passed through it in order to precipitate the mercury in solution, and after being again filtered it was evaporated. The residue left on evaporation was dissolved in a little water, and the solution was mixed with alcohol as long as any precipitate was produced; the precipitate collected at the bottom of the vessel forming a brown glutinous

* Read June 21, 1866: see Abstract, vol. xv. p. 1.

deposit, which, after pouring off the supernatant liquid, was dissolved in water. The solution on being evaporated over sulphuric acid left a residue, consisting of what would be called the urinary extractive matter insoluble in alcohol. It had the appearance of a dark brown, amorphous, brittle, gum-like mass, opaque in thick layers, but translucent at the edges. It had a slightly acid and nauseous taste. It was easily reduced by pounding to a light brown powder, and when exposed to the air it did not appear to deliquesce; but on being afterwards heated in the water-bath it swelled up considerably and became filled with small cavities or vesicles, caused doubtless by the escape of the water which it had absorbed from the atmosphere. Its watery solution, which was of a dark yellowish-brown colour, had an acid reaction. Its external properties did not differ materially from those ascribed to this substance by Berzelius, who says, "It has a yellowish-brown colour, is opaque in the mass; it has a slightly bitter taste, remains dry in the air, and dissolves in water with a dark yellow colour."

Now the substance as thus prepared was found to contain a considerable quantity of bases, by combination with which the organic constituents were rendered insoluble in alcohol. On being heated on platinum foil it swelled up considerably, and gave off gaseous products having a smell like that of burning bread, leaving at last a porous charcoal, which was with great difficulty reduced to ash. The ash was greyish-brown and alkaline, but for the most part insoluble in water; it consisted of oxide of iron, alumina, carbonate of lime, magnesia, carbonate of soda, and a trace of potash. A quantitative determination of the ash yielded the following results:—

0·7665 grm. of the substance heated for some time in the water-bath gave 0·1025 grm. ash = 13·37 per cent.

The finely powdered substance communicated no colour whatever to absolute alcohol; but on adding a few drops of concentrated sulphuric acid and allowing to stand for some time, the liquid acquired a deep yellow colour, exactly like that of solutions of the extractive matter soluble in alcohol. The matter left undissolved by the acid mixture was, after pouring off the liquid and washing with alcohol, treated with water, in which the greatest part dissolved, but without communicating much colour to the liquid; it consisted of the sulphates of lime, magnesia, and other bases. This experiment alone would suffice to prove that the substance contained the extractive matter soluble in alcohol in combination with various bases. A further proof was afforded by another experiment. The watery solutions of the urinary extractive matters become considerably darker on the addition either of strong mineral acids, such as hydrochloric acid, or of alkalies. The deepening of colour in the former case is due to decomposition, in the latter to mere combination. Now on taking five equal measures of a watery solution of the substance under examination, adding to the first merely water, to the second acetic acid, to the third dilute hydrochloric acid, to the fourth water and sufficient caustic potash to made it just alkaline, and to the fifth strong hydrochloric acid, taking care that the bulk of

the five liquids should be exactly the same, and then, after pouring them into test-tubes of the same bore, comparing together the tints which they exhibited, the following differences were observed:—The second was a little lighter than the first; the third, however, was much lighter; whilst the fourth was as dark as, but not darker than the first; and the fifth appeared hardly darker than the third; but on being boiled a few moments and allowed to cool, its tint was as dark as that of the first. Hence it follows that the colour of the substance owed a part of its intensity to the presence of an alkaline or other base; for had this not been the case, had the extractive matter been in an uncombined state, the addition of dilute acid would have caused a deepening of the tint, or at least have left it unchanged instead of making it lighter, whereas the alkali would have produced a darkening of the colour. The addition of a large excess of acid lowered the intensity of the colour in the first instance, in consequence of the acid combining with the bases; but on heating the solution it had the effect of deepening the colour, from the decomposing action of the acid on the now free extractive matter.

In order to isolate the organic body, or bodies, contained in the substance, some of the latter was dissolved in water, and to the solution basic acetate of lead was added. This deprived the solution of its colour, giving an abundant cream-coloured precipitate, which was filtered off, washed, suspended in water, and decomposed with sulphuretted hydrogen. The filtered liquid was evaporated at a gentle heat, when it left a brown glutinous residue. This, when burnt, still left a considerable quantity of ash, which was yellow and non-alkaline, and consisted chiefly of alumina with a little oxide of iron and a trace of lime. When treated with absolute alcohol, a great part of this glutinous matter dissolved, yielding a deep yellow solution, while a quantity of light-brown flocks was left undissolved. The filtered liquid left on evaporation a brownish-yellow deliquescent substance, which had the appearance and properties of the extractive matter soluble in alcohol. When burned, it left only a slight trace of ash. When treated with boiling caustic alkaline lye it evolved ammonia. It was perfectly insoluble in ether, and was therefore free from the extractive matter soluble in that menstruum. Its watery solution, when mixed with sulphuric acid and heated for some time, deposited a quantity of dark brown flocks, which, after being filtered off, washed, and dried, had the appearance of a dark brown, almost black powder (uromelanine), which was almost insoluble in alcohol, but dissolved readily in aqueous ammonia, giving a brown solution, from which it was reprecipitated by acids in brown flocks. This is the most characteristic property of the extractive matter soluble in alcohol and insoluble in ether, and serves to distinguish it from the one soluble in ether, which yields, by decomposition with strong acids, a brown resinous substance, easily soluble in alcohol (uroretine). That portion of the substance derived from the precipitate with basic acetate of lead, which was left undissolved by absolute alcohol in the shape of light brown flocks, was easily soluble in water.

The solution yielded on evaporation a yellow, brittle, transparent, gum-like substance, which, when burnt, left a considerable quantity of reddish-yellow ash. Hence it appears that the precipitate with basic acetate of lead contained, besides oxide of lead, other bases in combination with the organic bodies, a portion of it consisting probably of double compounds of extractive matter with lead, alumina, lime, &c. Similar compounds, without doubt, were present in the original precipitate thrown down from urine by basic acetate of lead. By the second precipitation a great portion of the alkaline or earthy bases was removed, the portion which remained forming, by combination with a part of the extractive matter, a compound insoluble in alcohol.

The substance under examination contained, however, in addition to extractive matter, a body having, like glucose, the property of reducing an alkaline solution of copper, a property which does not belong to either of the extractive matters in a state of purity. On mixing the watery solution with sulphate of copper and an excess of caustic soda, it assumed a bright green colour, and on being boiled deposited an abundance of suboxide of copper. The body producing this reaction cannot be separated from the extractive matter insoluble in ether by the use of solvents, since they behave in the same manner towards ordinary menstrua, nor by crystallization, as both are, as usually obtained, amorphous. Both substances are also precipitated from the watery solution by basic acetate of lead, and from the alcoholic solution by neutral acetate. If, however, a solution of the two substances in water contains also earthy and alkaline bases, the glucose-like body tends to combine with these to the exclusion of the extractive matter, and on now evaporating and treating the residue with alcohol, provided a sufficient quantity of bases is present, the alcohol takes up only extractive matter; but if they are deficient, the solution in alcohol will contain both. On the other hand, the compounds of the extractive matter with bases are less soluble in alcohol than those of the glucose; so that, on adding alcohol to a watery solution containing compounds of both bodies, the precipitate frequently consists of a compound of extractive matter only, without glucose. With a knowledge of these facts, it is easy to explain why in my experiments I obtained sometimes pure extractive matter, sometimes glucose only, and occasionally a mixture of both, without any great difference in the physical properties of the products being observed. That the glucose-like body so often accompanying the extractive matters has a composition similar to that of grape-sugar, and other substances of the same class, was proved by the analysis of its lead compound, the results of which were given in the first part of this memoir.

A specimen of extractive matter, insoluble in alcohol, obtained by evaporating a portion of the liquid from which the lead compound of Analysis IV. Series F, was precipitated with acetate of lead and alcohol, and which had been kept for several years in my collection of products from urine, gave on examination similar results. It was a dark brown, shining,

amorphous substance, transparent in thin layers, brittle when quite dry, but becoming soft and viscid on exposure to the air. Its taste was acid, followed by a bitter after-taste. When heated it swelled up, gave off much gas, and left a bulky charcoal, which was easily reduced to a white ash. A determination of the quantity of ash yielded the following results:—

0·5005 grm. dried in the water-bath left 0·0120 grm. ash=2·39 per cent.

The ash was non-alkaline, entirely insoluble in water, but soluble in hydrochloric acid, and consisted of alumina with traces of oxide of iron and lime. The substance was completely insoluble in boiling absolute alcohol; but on adding a little concentrated sulphuric acid, the liquid acquired a deep yellow colour, and nearly the whole of the substance dissolved gradually, though not so easily as in the preceding case. The alcoholic solution, on being mixed with several times its volume of ether, separated into two layers, the lower one being of a deep yellow colour, and containing apparently the whole of the extractive matter, while the supernatant ethereal liquid was almost colourless. The watery solution, when tried in the usual manner, was found to contain no trace of glucose. In this case, therefore, the substance consisted of extractive matter only, in combination with alumina. A very small amount of the latter is sufficient, as it appears, to produce with the extractive matter a compound insoluble in alcohol.

Some of the cream-coloured lead compound, which had served for Analysis III. Series G, and which had been prepared from a substance insoluble in alcohol, was treated with a mixture of alcohol and sulphuric acid. The liquid, on being left to stand in contact with the compound, gradually acquired a bright yellow colour like that of a solution of extractive matter, while the resulting sulphate of lead appeared almost white.

These experiments lead to the conclusion that there exists no urinary extractive matter insoluble in alcohol, and that what has hitherto been so called consists generally of a compound of the extractive matter soluble in alcohol, with some base or with several bases mixed occasionally with similar compounds of the glucose, which so often accompanies this extractive matter, and is probably one of its products of decomposition. Though the extractive matter soluble in ether forms, with bases, compounds insoluble in alcohol, I have never found this substance to be a constituent of the product insoluble in alcohol when prepared in the manner above described.

I shall now proceed to give my views of the composition of the uncombined urinary extractive matters, as deduced from the analytical determinations contained in the first part of this paper.

For the extractive matter soluble in alcohol and ether when in its highest state of purity I have adopted the formula $C_{38}H_{51}NO_{32}$. The Analyses III. Series A, I. Series F, I. Series G, and I. Series H gave numbers agreeing tolerably well with this formula, as the following Table, in which the results are placed together for the sake of a more

easy comparison with one another, and the theoretical composition, will show* :—

	Calculation.		A. III. 2.	F. I. 1.	G. I. 1, 2, 3.	H. I. 2.	Mean.
C_{86} ..	516	51·75	51·04	50·65	51·88	51·37	51·23
H_{51} ..	51	5·11	5·43	5·67	5·25	5·16	5·38
N ..	14	1·40	1·34	1·22	1·27	1·20	1·26
O_{52} ..	416	41·74	42·19	42·46	41·60	42·27	42·13
	997	100·00	100·00	100·00	100·00	100·00	100·00

I do not insist very strongly on the correctness of this formula; since, for a body the atomic weight of which is so high, and was determined solely by the amount of nitrogen contained in it, other formulæ might be calculated agreeing equally well with the results of analysis. I have adopted this one in preference, because a simple relation is thereby established between the composition of this extractive matter and that of the other, as I shall presently show. It will, however, hardly be doubted that the composition of this substance is, under all circumstances, the same, seeing that the material employed for its preparation in these experiments was derived from various sources, at wide intervals of time, and that it was obtained on one occasion from the precipitate with neutral acetate of lead, on other occasions from that with basic acetate of lead, or from all the three lead precipitates combined.

The second analysis corresponds more closely with the formula $C_{86}H_{53}NO_{54}$ than with the one just given, as I have before remarked. This proves that the substance has a tendency to take up the elements of water, a tendency still further developed in the case of the specimen employed for the Analysis V. Series F, which led to the formula $C_{86}H_{59}NO_{60}$.

In order to avoid circumlocution I shall for the future call this substance *urian*.

The extractive matter soluble in alcohol but insoluble in ether was in my opinion obtained only on one occasion free from all admixture, and in the state in which I suppose it to exist originally in the urine. On this occasion its composition corresponded with the formula $C_{38}H_{27}NO_{28}$, as proved by the results of Analysis III. Series F, which were as follows :—

	Calculation.		F. III. 2.
C_{38}	228	46·24	46·44
H_{27}	27	5·47	5·66
N	14	2·83	3·16
O_{28}	224	45·46	44·74
	493	100·00	100·00

* In this and the following Tables the numerals 1, 2, and 3 denote that the determination was made with substance obtained either from the precipitate produced in urine with neutral acetate of lead (1), or from that with basic acetate of lead in the liquid filtered from the first precipitate (2), or from the precipitate with ammonia in the filtrate from the other two precipitates (3). When the three numerals occur together, it indicates that all three precipitates were employed in the manner described in Part I.

In my earlier experiments, before I had commenced to employ ether without alcohol for the separation of the two extractive matters, I occasionally obtained mixtures of equal quantities of the two bodies, as proved by several analyses of lead compounds, the results of which have been given. For instance, Analyses I. Series A, and I. Series D, conducted to the formula $C_{62} H_{39} NO_{40}$. By doubling this formula, its relation to the two other formulæ will be seen at once, since

$$2(C_{62} H_{39} NO_{40}) = C_{86} H_{51} NO_{52} + C_{38} H_{27} NO_{28}.$$

The Analysis I. Series B, which was made with a similar mixture, led to the formula $C_{62} H_{36} NO_{37}$, which only differs from the preceding by three equivalents of water, from which it is to be inferred that in this case also both extractive matters were present, and that neither preponderated over the other. These analyses, therefore, though valueless in themselves, serve to confirm the two formulæ given for the extractive matters.

The extractive matter soluble in alcohol but insoluble in ether I propose to name *urianine*. The relation in which it may possibly stand to urian is shown by the equation

$$C_{86} H_{51} NO_{52} + 24\,HO = C_{38} H_{27} NO_{28} + 4\,(C_{12} H_{12} O_{12}),$$

which proves that urian after absorbing water may split up into urianine and glucose; and though this is a process which I have not hitherto actually observed, still it is one which may be assumed to take place within the body.

The later analyses of the lead compounds of this substance corresponded with the formula $C_{38} H_{27} NO_{32}$, as will be seen from the following Table, in which the results are placed in juxtaposition with one another and with the theoretical composition:—

	Calculation.		F. VI. 3.	G. II. 1, 2, 3.	H. II. 2.	H. III. 2.	Mean.
C_{38} ..	228	43·42	43·28	43·59	43·90	43·33	43·52
H_{27} ..	27	5·12	5·69	5·37	5·15	5·22	5·37
N ..	14	2·66	2·68	2·34	2·04	2·52	2·39
O_{32} ..	256	48·80	48·35	48·70	48·91	48·93	48·72
	525	100·00	100·00	100·00	100·00	100·00	100·00

The difference in composition to which the two formulæ point is, in my opinion, to be attributed not to any errors of analysis, nor to any variation in the quality of the urine employed on different occasions, but rather to a difference in the mode of preparation. In the later series of experiments artificial heat was employed in the evaporation of the solutions instead of the current of cold air made use of for the same purpose at the commencement of the investigation. I am inclined to think that, in consequence of the elevation of temperature, slight as it was, the substance took up four equivalents of oxygen; and though there was no apparent difference in the physical properties of the original and the oxidized substance, still they can-

not be considered as identical. If a distinct name is to be bestowed on the latter, I would suggest *oxurianine* as the most appropriate. There are indications of the presence of this substance in the earlier experiments also. For instance, Analysis II. Series E gave a composition not differing very widely from that to which the later determinations conducted.

The Analyses IX. Series A and I. Series C gave numbers corresponding with the formula $C_{86}\,H_{71}\,NO_{76}$, which seems to indicate that the substance analyzed was a mixture of oxurianine and glucose, since

$$C_{86}\,H_{71}\,NO_{76}=C_{38}\,H_{27}\,NO_{32}+4\,(C_{12}\,H_{11}\,O_{11}).$$

This supposition was confirmed by an examination of the lead compound of the analysis last named, a portion of which still remained. On treating some of this compound, which was of a pale cream-colour, with dilute sulphuric acid, I obtained a yellow solution which was filtered from the sulphate of lead. A portion of this solution, on being mixed with more acid and heated, became darker, and deposited a quantity of brown flocks, which is an indication of the presence of urianine or of oxurianine. Another portion became, on the addition of sulphate of copper and an excess of caustic alkali, of a deep blue colour, and on being boiled deposited an abundance of suboxide of copper.

The Analysis IV. Series A, which led to the formula $C_{50}\,H_{39}\,NO_{44}$, likewise represents a mixture of oxurianine and glucose, 1 equivalent of each having been present in this case.

The analyses of the lead compounds prepared from the so-called extractive matter, insoluble in alcohol, support the view which I take of its nature, viz. that it consists in most cases of a compound of the extractive matter soluble in alcohol with bases, since these analyses gave for the substance, combined with oxide of lead, numbers corresponding with the formula $C_{38}\,H_{29}\,NO_{34}$, as will be seen on glancing over the results, which are here subjoined :—

	Calculation.		D. II. 2.	F. IV. 2.	G. III. 1, 2, 3.	H. IV. 2.	Mean.
C_{38} ..	228	41·99	41·79	42·11	41·60	41·66	41·79
H_{29} ..	29	5·34	5·81	5·51	5·55	5·68	5·64
N ..	14	2·57	2·57	2·66	2·40	3·88	2·87
O_{34} ..	272	50·10	49·83	49·72	50·45	48·78	49·70
	543	100·00	100·00	100·00	100·00	100·00	100·00

The tendency to absorb water on the part of these substances, which I have pointed out in the case of urian, shows itself here also. Seeing that the body having a composition corresponding to the formula $C_{38}\,H_{29}\,NO_{34}$ was always obtained from compounds of extractive matter with bases, it is probable that its formation from the one having the formula $C_{38}\,H_{27}\,NO_{32}$ is due to the action of bases, an action which so often leads to the absorption of water by organic bodies.

In the preceding review several of the determinations given in Part I. have not been referred to. They are as follows:—

Series A, Analyses II., V., VI., VII., VIII.
E, „ I., III., IV.
F, „ II., VII.

Of these A. II. and A. VI. gave numbers corresponding very well with the formula $C_{62}H_{46}NO_{47}$, which may be considered to represent a mixture of what may be called the hydrates of urian and urianine, since

$$2(C_{62}H_{46}NO_{47})=C_{86}H_{59}NO_{60}+C_{38}H_{33}NO_{34}.$$

A hydrate having the formula $C_{86}H_{59}NO_{60}$ was once isolated, and its lead compound was submitted to analysis (F. V.); but the other was not obtained in my experiments, though Analysis V. Series A gave results agreeing with the formula $C_{38}H_{28}NO_{29}$, which represents a compound of urianine and water. The substance with which Analysis VII. Series A was made was probably a mixture of oxurianine and glucose, examples of which occurred frequently in the course of my experiments, as I have before explained. The Analyses I. Series E and II. Series F are the only determinations of importance which admit of no explanation, unless it be assumed that considerable errors were committed in making them.

I maintain, then, that, with the exceptions just named, the numerical results obtained in my examination of the composition of these bodies may be explained by adopting the views which I have set forth, views which, I venture to say, have at least the merit of simplicity to recommend them. Whether they are correct or not I should, however, despair of arriving any nearer the truth by still further multiplying experiments of the kind I have described, and I therefore have brought this portion of the investigation to a close.

The striking analogy subsisting between the extractive matters of urine and the series of bodies of which indican forms the first member is a point of some interest, to which I shall have again occasion to refer. The relation in which urian and urianine stand to one another is, in my opinion, similar to the one which has been found to exist between indican and indicanine. Then, again, both urianine and indicanine take up oxygen, and are converted into oxurianine and oxindicanine, bodies which, in their physical properties, resemble those from which they are derived. It is indeed not impossible that oxindicanine may be actually converted by a very simple process into oxurianine, as will be evident from the following equation:—

Oxindicanine. Oxurianine.

$$C_{40}H_{23}NO_{32}+4HO=C_{38}H_{27}NO_{32}+2CO_2.$$

Processes such as the one represented by this equation are constantly going on in the body, and it is therefore quite possible that the indican originally existing in the blood or tissues may be decomposed, and appear in the urine as ordinary extractive matter. Indeed the same process may take place in the urine itself as a result of fermentation and oxidation,

which may serve to explain the fact of the existence of indican in urine having so frequently been overlooked.

From the experiments hitherto described, I am justified, I think, in drawing the following conclusions:—

1. Human urine contains, under all circumstances, two distinct and peculiar extractive matters, one of which is soluble in alcohol and ether, while the other is soluble in alcohol but insoluble in ether.

2. The composition of these bodies is almost always the same, the slight variations which are found to occur being due, not to any difference in the quality or source of the urine employed at various times, but rather to the decomposition which takes place during the process of preparation, and which cannot be entirely avoided.

3. Both substances contain nitrogen as an essential constituent, but in so small a proportion as to show that their atomic weight must be very high.

4. Both substances have a tendency to take up water, especially when their aqueous solutions are heated or mixed with strong acids.

5. The extractive matter insoluble in ether takes up a certain proportion of oxygen, and is converted into a product, which does not differ in its appearance or its most obvious physical properties from the original substance.

6. There exists no urinary extractive matter insoluble in alcohol, the substance hitherto so called consisting in most cases of compounds of one of the true extractive matters with various bases.

III. "On a Crystalline Fatty Acid from Human Urine." By Edward Schunck, F.R.S. Received September 21, 1866*.

The occurrence of fatty matter in urine is a somewhat rare phenomenon, and is generally considered as a symptom of disease, or at least of an abnormal state of the system. In most cases it is found associated with albumen, forming the so-called "chylous urine," in which the fatty matter is suspended in such extremely minute particles as to give the liquid the appearance of milk. In a few instances it has occurred in the shape of fluid oil-globules floating about in the urine; but it is more frequently found enclosed in cells, which sink and form a deposit at the bottom of the vessel. Fatty matter is a constituent of *kiesteine*, the pellicle which is sometimes formed on the surface of the urine of pregnant women; and a fat resembling butter was obtained from it by Lehmann, though by some authors the very existence of kiesteine as a peculiar deposit is doubted. Lastly, a few cases are described in which a fat-like substance was passed with the urine in the form of small concretions, which, when fresh, were soft and elastic, but dried into hard, yellow, wax-like masses (Heller's

* Read November 15, 1866: see Abstract, vol. xv. p. 258.

urostealith). In no recorded instance was the fatty matter contained in the secretion in a state of true solution.

The accounts which are given of the physical and chemical properties of the fatty matters of urine are extremely vague, and quite insufficient to enable us to identify them, so that it may be concluded that in most cases the quantity obtained was extremely small. Dr. Beale has, indeed, shown that the fatty matter which accumulates in the epithelial cells, passed with the urine in some cases of fatty degeneration of the kidney, contains cholesterine; and Berzelius and Lehmann state that urine, when distilled with the addition of sulphuric acid, yields butyric acid; but in other respects our ignorance is almost complete. None of the works devoted to the subject of urine contain a hint which would lead one to suppose that fatty matter in any form is a constituent of the ordinary healthy secretion.

These few words will probably suffice to give an idea of the present state of our knowledge on this subject from a chemical point of view.

The discovery of which I am about to give an account was a result of the examination of the colouring and extractive matters of urine with which I have been occupied for some time, and which forms the subject of several Papers already communicated to the Royal Society. In the course of my experiments, I observed on several occasions, mixed with the urinary extractive matters, drops of a brown or yellow oil, the appearance of which I could not account for, since it was difficult to conceive how fat of any kind could be deposited from watery solutions of these extractive matters, which generally have an acid reaction; unless, indeed, it was assumed either that it was a product of decomposition, or that the extractive matters possess the property of effecting the solution or suspension of fatty matter in water. On one occasion there was deposited during the evaporation of a watery solution of urian (the extractive matter soluble in ether) a quantity of fatty acid, from which I prepared a baryta-salt soluble in boiling alcohol, and crystallizing from this solution in small scales. Traces of a fat-like substance were almost always obtained on treating watery solutions of the extractive matters with animal charcoal, filtering, treating the charcoal with boiling alcohol, and evaporating the alcoholic liquid. Animal charcoal also effected the separation of a small quantity of fatty matter from urine itself, and this circumstance led me to devise a plan for procuring a quantity sufficiently large to enable me to determine its chief properties. This method I shall now proceed to describe.

Ordinary healthy urine, having been filtered so as to separate all insoluble matter, is passed in successive portions through purified animal charcoal contained in a common percolating apparatus. The percolating liquid appears quite colourless, and devoid of the usual odour of urine. A large quantity of urine may thus be passed with the same effect through a small quantity of charcoal; but at last there arrives a point at which the charcoal, though apparently retaining its decolorizing and deodorizing power undiminished, suffers the liquid to pass through with extreme slowness

only, and the latter, after having percolated, appears rather milky, from a small quantity of white matter suspended in it. At this point it is advisable to discontinue the percolation of urine and to commence washing the charcoal with water. This is continued until every trace of chlorides and phosphates is removed, and the charcoal is then laid to dry, either in the air or at a moderate temperature in a stove. When dry the charcoal is treated with boiling alcohol, to which it communicates a bright yellow colour like that of urine itself, the liquid is filtered, and the process is repeated until the alcohol acquires only a faint yellow colour. To arrive at a point at which it would appear quite colourless seemed to me almost impossible. The whole of the alcoholic liquid, which in any case is considerable in quantity, is now evaporated either spontaneously or at a moderate temperature. The brown syrupy residue which is left on evaporation is mixed with water, which leaves undissolved a quantity of dark-brown semifluid fatty matter to be separated by filtration. The liquid, which has a yellow colour, contains in solution a crystallized organic substance, the occurrence of which in urine has not hitherto been observed. It also contains, provided the evaporation of the alcoholic liquid was conducted spontaneously, a quantity of indican; for on the addition of sulphuric or hydrochloric acid, it deposits flocks of indigo-blue—a reaction which, however, ceases to be produced after the solution has stood for some time in a warm place. Its colour is mainly due to the ordinary extractive matters of urine which it contains.

The fatty matter which is left undissolved by the water has a dark-brown colour and a strongly urinous odour. In order to purify it, it is dissolved in alcohol, and the filtered liquid is evaporated. The residual fatty mass is pressed between blotting-paper, in order to absorb as much as possible the more fluid portion, and it is then redissolved in alcohol. The alcoholic solution is agitated with a little animal charcoal, which deprives it of some of its colour, then filtered and evaporated, when it leaves a brownish-yellow residue, which still retains some of the odour just referred to. By treating it with very dilute spirits this odour, as well as the yellow colour, which seem to belong to the same body, are removed, and an almost white solid fat is left undissolved*. This may be still further purified by dissolving it in a boiling solution of carbonate of potash. The soap, which separates on cooling, is filtered off, washed with a solution of carbonate of potash, and decomposed with acid. The fatty acid which separates is now quite

* The filtered alcoholic liquid leaves on evaporation a semifluid, yellow, amorphous fatty matter, having a peculiar urinous odour. When heated on platinum-foil this substance becomes more fluid, gives off a strong smell like that of burning fat, and then burns with a bright flame. It dissolves easily in caustic soda-lye, and the solution froths on being boiled. Its alcoholic solution is yellow, reddens litmus-paper more strongly than the solution of the crystalline acid, and gives with acetate of lead a dirty-yellow flocculent precipitate, which is somewhat soluble in alcohol, since the liquid, when boiled and filtered boiling hot, deposits a quantity of white crystalline grains accompanied by a few thin prismatic crystals.

colourless. After being washed it is dissolved in alcohol. On spontaneous evaporation the solution leaves a perfectly white crystalline residue consisting of the acid in a state of purity.

As thus prepared, the substance has all the properties characteristic of the group of fatty acids to which palmitic and stearic acid belong. It is white, has a pearly lustre and a crystalline appearance, and when viewed under the microscope is seen to consist of small star-shaped masses. From a solution in boiling dilute spirits it is deposited, on the solution cooling, in shining scales. The alcoholic solution reddens litmus-paper slightly; it floats on the surface of water, which it repels like all other fats. When the water is heated, it melts into oily drops, which on cooling become solid and crystalline. Its melting-point, as determined with an apparently pure specimen, is 54°·3 C. When impure, *i. e.* contaminated with the body which imparts the brownish-yellow colour to the crude product, it fuses at a lower temperature. A specimen only slightly coloured melted at 52°·8 C., another at 49°·5 C. When heated between two watch-glasses, the acid fuses and is then volatilized, leaving only a trace of residue, while there is formed on the upper glass an oily sublimate, which on cooling becomes solid and glassy. This sublimate dissolves easily in alcohol, and the solution leaves on spontaneous evaporation a white crystalline residue consisting of needles arranged in star-shaped or feather-like masses. The substance dissolves as easily in ether as in alcohol, and the solution leaves on evaporation a white crystalline mass. It is easily soluble in boiling dilute caustic potash and soda-lye, as well as in aqueous ammonia; these solutions froth on being boiled like those of ordinary soap. The solution in potash deposits on cooling a quantity of white pearly scales, which settle slowly to the bottom of the vessel. The soda compound separates in the form of a thick, white, amorphous soap, a very small quantity of which is sufficient to cause the liquid to gelatinize on cooling. The ammoniacal solution deposits on cooling a quantity of scales, which resemble the potash compound, together with a few crystalline needles. Boiling solutions of carbonate of potash and carbonate of soda also dissolve the acid readily. When the residue left by evaporating the solution in carbonate of potash to dryness is treated with boiling absolute alcohol, an alcoholic solution of the potash-soap is obtained, which, after being filtered from the excess of carbonate of potash and spontaneously evaporated, leaves a residue consisting partly of isolated prismatic crystals, partly of star-shaped masses. The soda compound may in the same manner be obtained in a crystalline state. The alcoholic solution of either of these compounds gives with acetate of baryta a white crystalline deposit. A watery solution gives with nitrate of silver a white, curd-like precipitate, which blackens slowly on exposure to the light. The ammoniacal solution of the acid produces, with the chlorides of barium and calcium, white, flocculent precipitates, which do not become crystalline on standing. The alcoholic solution yields with acetate of lead, an abundant white amorphous precipitate.

These experiments lead to the conclusion that human urine contains in a state of solution a crystalline fatty acid, having the general properties of the members of this class, which are solid at the ordinary temperature. The quantity of this substance which I obtained was too inconsiderable to enable me to determine its composition, and the melting-point therefore afforded the only means of ascertaining whether it is identical with any of the known fatty acids or not. Were it not for the low melting-point there would be nothing to oppose the conclusion that it is palmitic acid, one of the constituents of human fat. It is, however, a well-known fact that mixtures of two solid fatty acids in certain proportions melt at a lower temperature than the most fusible even of the constituents. For instance, according to Heintz, a mixture of 30 parts of stearic acid with 70 of palmitic acid fuses at 55°·1 C., though the melting-point of the former when pure is 70° and of the latter 60°. This urinary acid may therefore be a mixture of this kind and not a peculiar substance—in fact a mixture of the two acids just named, which, according to recent investigations, constitute together what was formerly called margaric acid, the solid acid of human fat.

Considering how many of the organs and secretions of the human body contain fat, it need not excite surprise that a minute quantity of fatty acid should be found in urine also, in consequence of deficient oxidation or from other causes. That it forms a normal constituent of the secretion I do not venture to assert, though the urine employed in my experiments in no case exhibited anything peculiar, and when submitted to the process above described, never failed to yield a little of the fatty acid. The quantity obtained was always extremely small. In one experiment, for instance, 45 litres of urine yielded 0·14 grm. of tolerably pure acid, which, assuming the urine to have been of average composition, would be equal to the 22000th part of its solid constituents. It is far from certain, however, that this was the total quantity contained in it. The simple method of separating the substance from the urine which I have described will enable pathologists to determine whether in cases of disease its quantity is sensibly increased.

The question how this fatty acid, which belongs to a class of bodies almost insoluble in water, comes to be dissolved in urine will naturally suggest itself, but it is one to which it is difficult to find a satisfactory reply. Whether urine is capable of dissolving a small quantity of the acid itself, whether the latter is contained in it in combination with some base, the compound being soluble in water but not decomposable by the weak acids of the urine, or whether, as there seems reason to suspect, the extractive matters promote the solubility of the fatty acid in water, are points on which I express no opinion. That the animal charcoal, when used in the manner above described, effects not a mere filtration, but an actual separation of some of the constituents of urine, may be considered as quite certain.

IV. "On Oxalurate of Ammonia as a Constituent of Human Urine." By Edward Schunck, F.R.S. Received November 15, 1866*.

When urine is allowed to percolate through animal charcoal in the manner described in the preceding Paper, several organic substances are absorbed and separated by the charcoal in addition to the fatty acid there referred to. The liquid obtained by treating the charcoal with boiling alcohol yields on evaporation a syrupy residue, of which a great part dissolves in water, the fatty acid being left undissolved. The filtered liquid on being again evaporated leaves a brown syrup, among which a quantity of yellowish crystals is formed on standing. On treating the mass with cold alcohol, the syrupy portion, consisting of urinary extractive matter, is removed, the crystals being left undissolved. The latter are filtered off, washed with alcohol, and then dissolved in boiling water. The solution, which has a slightly yellow colour, is evaporated to a small volume, and the crystals, which separate on standing, are pressed between blotting-paper and then dissolved in a little boiling water to which a small quantity of animal charcoal is added. The filtered solution, if tolerably concentrated, becomes on cooling almost solid, from the formation of a quantity of white crystalline needles, which, after the liquid has been drained off, only require drying. The substance as thus prepared consists of pure oxalurate of ammonia, since it is found to possess both the properties and the composition of that salt, as I shall now proceed to show.

The crystals of which it consists are mostly small, and exhibit, even when magnified, few well-defined forms. When a few drops of the watery solution are allowed to evaporate spontaneously on a slip of glass, the residue, when viewed under the microscope, is found to consist mainly of groups of crystals arranged round centres in various irregular forms, the larger ones being composed of prisms, which are acuminated, jagged at the edges, and transversely striated, the smaller ones of needles arranged in star-shaped, double fan-shaped, or circular masses. Occasionally isolated crystals are seen, having the form of rhombic plates, some of which have two of their opposite angles truncated. I have not yet had an opportunity of comparing these forms with those exhibited by the oxalurate of ammonia obtained directly from uric acid. The substance is tolerably soluble in boiling water, but very slightly soluble in boiling alcohol, the little which dissolves in the latter being deposited, on the solution cooling, in fine needles arranged in stars. The watery solution is neutral to test-paper; but on allowing a drop to fall on blue litmus-paper, and exposing the latter to the air for some hours, the spot will appear quite red. The watery solution, on being mixed with hydrochloric or nitric acid, yields a white crystalline deposit (oxaluric acid), which, on being left in contact with the acid liquid, gradually disappears. If nitric acid has been employed and the solution, after

* Read November 15, 1866: see Abstract, vol. xv. p. 259.

the deposit has dissolved, be spontaneously evaporated, a mass of crystals is left, some of which have the well-known form of nitrate of urea, while the others are prismatic, and consist doubtless of oxalic acid. If the solution, after the addition of any strong acid, be boiled, oxalic acid may after a few moments be detected in it. The watery solution gives no precipitate with chloride of calcium, not even on the addition of ammonia; but on boiling, an abundant precipitation of oxalate of lime takes place. If a tolerably concentrated solution be mixed with chloride of calcium and left to stand, it deposits after some time a quantity of prismatic lustrous crystals, consisting doubtless of oxalurate of lime. The watery solution gives no immediate precipitate with nitrate of silver; but after a few moments it begins to deposit white crystalline needles, which, if the solution was concentrated, increase to such an extent as to fill the whole liquid. These needles are silky in appearance, and do not blacken on exposure to the light, but only become slightly yellow; they dissolve easily in ammonia, but no reduction takes place on boiling the solution. The watery solution of the substance gives with acetate of lead a copious crystalline deposit, and if this be filtered off, the solution yields on standing a crop of small lustrous crystals. These crystals, when examined under the microscope, are found to have very regular forms, consisting of elongated four-sided prisms, with six terminal faces. Whether this form is the same as that of the oxalurate of lead, prepared with acid obtained from the usual source, I cannot say, as I have been unable to find any description of the salt in the books. The watery solution gives no precipitate with perchloride of mercury; but on the addition of chloride of zinc it deposits after some time a quantity of white, hard crystalline grains, which, after being filtered off and washed, are found to contain no chlorine, and on being heated, melt and burn, leaving a white residue of oxide of zinc. If the substance is dissolved in dilute hydrochloric acid, and the solution, after the addition of bichloride of platinum, is evaporated to dryness, the residue on being treated with cold alcohol dissolves partly, a quantity of shining yellow crystals, consisting of chloride of platinum and ammonium, being left undissolved.

Such are the principal reactions of this substance. Its analysis yielded the following results:—

0·6230 grm. lost, on being heated for several hours in the water-bath, 0·0030 grm., or 0·48 per cent., a loss too trifling to be attributed to anything but hygroscopic moisture.

0·3565 grm. of the dry substance gave 0·3150 grm. carbonic acid and 0·1615 grm. water.

0·2605 grm., burnt with soda-lime, gave 1·1425 grm. chloride of platinum and ammonium.

These numbers lead to the formula $C_6 H_7 N_3 O_8$, which is that of oxalurate of ammonia, and requires

	Calculation.		Experiment.
C_6	36	24·16	24·09
H_7	7	4·70	5·03
N_3	42	28·19	27·54
O_3	64	42·95	43·34
	149	100·00	100·00

It is therefore certain that the substance obtained by this process is oxalurate of ammonia. These experiments, however, by no means decide the question whether the oxaluric acid exists originally in a free or combined state, since most chemists deny the presence of ready-formed ammonia in urine; and it is quite possible that in my experiments a sufficient quantity of ammonia was formed by the decomposition of urea to saturate the oxaluric acid present. Still I incline to the opinion that the ammonia-salt preexisted in the urine examined, since there were no perceptible indications of decomposition during the percolation of the urine through animal charcoal, a process, indeed, which would rather tend to prevent decomposition than to promote it. The acid reaction of the urine might be urged as an objection to this view; but, on the other hand, it may safely be asserted that we are still in the dark as to the cause of the acid reaction of urine, which may be due to an acid or acids much weaker than oxaluric.

Whether oxaluric acid, either free or combined, is a normal constituent of human urine or not, is a question which may also be raised; but it is one to which I am unable to give a decided reply, as my experiments are not sufficiently numerous for the purpose. I may venture, however, to express my opinion that this acid will be found to be a constituent of the healthy secretion *. The presence of oxaluric acid in urine had been previously suspected, since the dumb-bell crystals occasionally found among the deposits of oxalate of lime are supposed, by Golding Bird and others, to consist of oxalurate of lime, though the evidence on which this opinion is founded is unsatisfactory, and has been refuted by other observers.

On the other hand, there can be no doubt that the presence of oxaluric acid or its compounds in urine, whether it be an exceptional phenomenon or not, serves to explain, in an easy and satisfactory manner, the formation

* In a mixture, or any impure product supposed to contain oxaluric acid, I would recommend its detection in the following manner:—The matter, if soluble, should be dissolved in water; but if it is insoluble, in consequence of the presence of some base, a little sulphuric acid should be added to set at liberty the oxaluric acid, after which the solution should be mixed with acetate of lead; and if any precipitate is thereby produced, this must be filtered off and the liquid left to stand, when it deposits small shining crystals if oxaluric acid is present. The residue obtained by evaporation of the mother-liquid of creatine, obtained from urine in the usual manner by means of chloride of zinc, gave, when treated in this way, crystals which could not be distinguished by their form from oxalurate of lead. Oxalurate of silver, distinctly crystallized, can only be obtained from perfectly pure oxaluric acid.

of oxalate of lime so often taking place in the secretion. The appearance of oxalate of lime as a deposit from urine long after its emission has hitherto been a puzzling phenomenon, and the most improbable hypotheses have been resorted to in order to explain it. It has, for instance, been assumed that there exists in the animal economy a tendency to the formation of a soluble triple compound of oxalic acid, lime, and albumen, which, by its decomposition, allows oxalate of lime to crystallize. Then it has been maintained by Rees that uric acid and the urates furnish oxalic acid when the urine containing them is simply heated or boiled; though this statement is questioned by other observers, and it is certain that under ordinary circumstances the conversion of one into the other can only be effected by means of very powerful oxidizing agents such as nitric acid. The attempts which have been made to prove that oxalate of lime may exist ready formed and in a state of solution in the urine are also unsatisfactory, the only known solvent likely to occur naturally being acid phosphate of soda. Were this salt really the means of keeping the oxalate dissolved, the latter would only be deposited when the acid reaction of the urine had disappeared, or had at least somewhat diminished, which is not the case. Now, however, the whole process may be easily explained. Oxaluric acid, as all chemists know, may be considered as a compound of oxalic acid and urea minus water, its composition corresponding to that of oxamic acid. By the action of acids, alkalies, or even water at a high temperature, it is decomposed, yielding oxalic acid and urea. How easily this process of decomposition may be set up in urine when allowed to stand, or even boiled, need not be pointed out. The oxalic acid as soon as formed combines, of course, with the lime which is always present in urine, producing the well-known deposit of oxalate. Those who maintain, with Rees, that oxalate of lime may be produced in the urine after excretion are therefore quite correct, though the phenomenon has hitherto been wrongly interpreted. The conversion of oxaluric into oxalic acid may, however, commence already in the bladder, or even more remote parts of the system, and thus lead to the formation of concretions and calculi. Regarding the origin of the oxaluric acid of urine there can be little doubt. In the animal frame, just as in the laboratory, it must be formed by the oxidation of uric acid, which is its only known source; it may be considered as the vehicle appointed by nature for getting rid of oxalic acid in the least injurious form. Were this acid excreted as such, it would, by combining with lime, produce serious results, which are prevented by the simple expedient of causing it to pass off in a state of intimate union with urea.

Communications received since the end of the Session.

I. "On a New Class of Bodies Homologous to Hydrocyanic Acid."—I. By A. W. Hofmann, LL.D., F.R.S. Received August 20, 1867.

The typical transformation which hydrocyanic acid undergoes when submitted, under appropriate circumstances, to the action of water, is capable of assuming two different forms when accomplished in its homologues.

If the hydrocyanic molecule be found to fix the elements of two molecules of water, yielding ultimately formic acid and ammonia, it is obvious that the atom group which in the homologues of hydrocyanic acid we assume in the place of hydrogen may be eliminated when these homologues are decomposed by water in conjunction either with formic acid or with ammonia. To take an example:—When acting with water upon the simplest homologue of hydrocyanic acid (upon cyanide of methyl), we may expect to see the methyl-group separating either in the form of methyl-formic, *i. e.* acetic acid, or in the form of methyl-ammonia, *i. e.* of methylamine. The difference of the two reactions and their relation to the metamorphosis of hydrocyanic acid itself are exhibited by the following equations:—

$$\underbrace{CHN}_{\text{Prussic acid.}} + 2H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}} + H_3N.$$

$$1.\ \underbrace{C_2H_3N}_{\text{Cyanide of methyl } \alpha.} + 2H_2O = \underbrace{C_2H_4O_2}_{\text{Methylformic (acetic) acid.}} + H_3N.$$

$$2.\ \underbrace{C_2H_3N}_{\text{Cyanide of methyl } \beta.} + 2H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}} + \underbrace{CH_5N.}_{\text{Methylamine.}}$$

The former one of these processes of transformation is familiar to chemists from the study of the hydrocyanic ethers or nitriles. The first member of this remarkable group of bodies (cyanide of ethyl) was discovered by Pelouze; the general character of their transformation was subsequently established by the beautiful investigations of Kolbe and Frankland on the one hand, and by those of Dumas, Malaguti, and Le Blanc on the other.

Researches in which I have been engaged during the last few weeks have proved that the second process of transformation does not less frequently occur. Indeed I have found that there corresponds to each of the hydrocyanic ethers or nitriles known hitherto, a second body of precisely the same composition but of absolutely different properties. These substances, when changed by water, undergo the transformation which is exhibited by the last one of the three above equations.

A happy experiment has led me to the discovery of this new class of bodies. In a lecture I wanted to exhibit the interesting transformation of

ammonia into prussic acid by means of chloroform, which was first observed by M. Cloez, and which illustrates so well our present views on quantivalence. When the two substances alone are allowed to act upon one another, this reaction can be rapidly accomplished only at a high temperature and consequently under pressure. In order to shorten the process (in one word, in order to exhibit this important reaction in a lecture-experiment), I had added potash to the mixture for the purpose of fixing the newly formed prussic acid, and was delighted to find that a few seconds' ebullition was sufficient to yield a considerable amount of cyanide of potassium, so as to furnish, after the addition of the two salts of iron, a large quantity of Prussian blue. On subsequently repeating the experiment with some of the derivatives of ammonia, more especially with several primary monamines, I was astonished to observe in each case a powerful reaction giving rise to the evolution of vapours of an almost overwhelming odour, strongly recalling that of prussic acid. But few experiments were necessary for the purpose of isolating the odoriferous bodies. The compounds thus formed are the substances isomeric with the hydrocyanic ethers or nitriles hitherto examined.

From the host of bodies which were thus suddenly thrown into view, it was necessary to single out the compound of a particular series in order to determine by accurate experiments the nature of the new reaction. The facility of procuring the necessary material, as well as old predilections, suggested the phenyl-series as the one to be examined in the first place. I beg leave to submit to the Royal Society a brief account of the mode of preparation, and of the principal properties, of the new derivative of aniline.

Cyanide of Phenyl.

A mixture of aniline, chloroform, and alcoholic potash yields on distillation a liquid of a powerfully aromatic but, at the same time, hydrocyanic-acid-like odour. The vapour of the liquid gives rise to a peculiar bitter taste, and causes, moreover, in the throat the suffocating sensation so characteristic of hydrocyanic acid. On redistilling the liquid, alcohol and water pass first, and ultimately an oily body is procured, which, in addition to the smelling substance, still contains a large amount of aniline. The latter is separated by oxalic acid, when the powerfully smelling compound remains in the form of a brownish oil. Freed from water by hydrate of potassium and purified by distillation, the new body presents itself as a mobile liquid, exhibiting a greenish colour in transmitted, and a beautifully blue colour in reflected light. This colour does not disappear by distillation even in a current of hydrogen.

The analysis of the blue oil has established the formula

$$C_7 H_5 N.$$

The compound is thus seen to be isomeric with benzonitrile, discovered by Fehling, from which it differs, however, in all its properties. In order to distinguish the new compound from benzonitrile I will call it *cyanide of*

phenyl, without intending, however, by selecting this name, to express any particular view as to its constitution. The formation of cyanide of phenyl is represented by the following equation:—

$$\underbrace{C_6H_7N}_{\text{Aniline.}} + \underbrace{CHCl_3}_{\text{Chloroform.}} = \underbrace{C_7H_5N}_{\text{Cyanide of phenyl.}} + 3HCl.$$

Cyanide of phenyl cannot be volatilized without undergoing decomposition. During distillation the thermometer marks for some time the constant temperature of 167°, which may be taken as the boiling-point of cyanide of phenyl. Then the temperature rises rapidly to from 220° to 230°. The brown liquid which now distils is destitute of odour, and solidifies on cooling to a crystalline mass, easily purified by solution in alcohol, but not yet more minutely examined. Cyanide of phenyl is remarkable for the facility with which it combines with other cyanides. The compound with cyanide of silver is particularly beautiful. The behaviour of cyanide of phenyl with acids is more especially characteristic. Scarcely changed by the action of alkalies, it cannot be left in contact even with moderately dilute acids without undergoing alteration. When submitted to the action of concentrated acids, the liquid bursts into ebullition, and the solution, after cooling, contains only formic acid and aniline.

$$\underbrace{C_7H_5N}_{\text{Cyanide of phenyl.}} + 2H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}} + \underbrace{C_6H_7N}_{\text{Aniline.}}$$

Benzonitrile, isomeric with cyanide of phenyl, is known to be slowly attacked by acids, but to be rapidly transformed by alkalies into benzoic acid and ammonia.

$$\underbrace{C_7H_5N}_{\text{Benzonitrile.}} + 2H_2O = \underbrace{C_7H_6O_2}_{\text{Benzoic acid.}} + H_3N.$$

The transformation of benzonitrile into benzoate of ammonium, as, indeed, the transformation of the nitriles into the ammonium-salts of the respective acids generally, is not accomplished in one single bound. By fixing only one molecule of water, benzonitrile is first converted into benzamide,

$$\underbrace{C_7H_5N}_{\text{Benzonitrile.}} + H_2O = \underbrace{C_7H_7NO}_{\text{Benzamide.}}$$

Nor is the corresponding term of the isomeric series wanting. This substance has long been known as phenyl-formamide or formanilide,

$$\underbrace{C_7H_5N}_{\text{Cyanide of phenyl.}} + H_2O = \underbrace{C_7H_7NO}_{\text{Phenyl-formamide.}}$$

But, in addition to phenyl-formamide, there figures in this series a second intermediate compound, the analogue of which among the derivatives of

benzonitrile is not yet perfectly known*. This compound is the well-defined base which some time ago I described as methenyldiphenyldiamine, and which may be looked upon as formed by the association of a molecule of cyanide of phenyl with a molecule of aniline. The successive changes which cyanide of phenyl undergoes when submitted to the influence of water are thus exhibited by the following series of equations:—

$$\underbrace{C_{14}H_{10}N_2}_{\substack{\text{2 mols. cyanide}\\ \text{of phenyl.}}} + 2H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}} + \underbrace{C_{13}H_{12}N_2}_{\substack{\text{Methenyldi-}\\ \text{phenyldiamine.}}}$$

$$\underbrace{C_{13}H_{12}N_2}_{\substack{\text{Methenyldi-}\\ \text{phenyldiamine.}}} + H_2O = \underbrace{C_7H_7NO}_{\substack{\text{Phenyl-}\\ \text{formamide.}}} + \underbrace{C_6H_7N}_{\text{Aniline.}}$$

$$\underbrace{C_7H_7NO}_{\substack{\text{Phenyl-}\\ \text{formamide.}}} + H_2O = \underbrace{CH_2O_2}_{\substack{\text{Formic}\\ \text{acid.}}} + \underbrace{C_6H_7N}_{\text{Aniline.}}$$

A glance at these formulæ shows that the metamorphosis of phenylic cyanide is perfectly analogous to that of phenylic cyanate, which I have studied at an earlier date.

$$\underbrace{C_{14}H_{10}N_2O_2}_{\substack{\text{2 mols. cyanate}\\ \text{of phenyl.}}} + 2H_2O = \underbrace{CH_2O_3}_{\substack{\text{Carbonic}\\ \text{hydrate.}}} + \underbrace{C_{13}H_{12}N_2O}_{\text{Diphenyl-urea.}}$$

$$\underbrace{C_{13}H_{12}N_2O}_{\text{Diphenyl-urea.}} + H_2O = \underbrace{C_7H_7NO_2}_{\substack{\text{Anhydrous car-}\\ \text{bonate of aniline.}}} + \underbrace{C_6H_7N}_{\text{Aniline.}}$$

$$\underbrace{C_7H_7NO_2}_{\substack{\text{Anhydrous car-}\\ \text{bonate of aniline.}}} + H_2O = \underbrace{CH_2O_3}_{\substack{\text{Carbonic}\\ \text{hydrate.}}} + \underbrace{C_6H_7N}_{\text{Aniline.}}$$

In conclusion, I may state that I have submitted ethylamine, amylamine, and toluidine to the action of chloroform. The phenomena are, as might have been expected, perfectly analogous. Indeed the application of the new reaction to the different classes of the ammonia derivatives, to the amides, to the diamines and triamines, and perhaps even to some of the natural alkaloids, promises a rich harvest of results. Should I have the good fortune of gathering some of these, I shall not fail to present them to the Society, which has so generously encouraged and assisted my earlier researches in the field of organic chemistry.

* Shortly before his death, Gerhardt was engaged in experiments on the action of pentachloride of phosphorus on the amides, a brief account of which was subsequently published by M. Cahours. Among other substances, I find that, by acting with pentachloride of phosphorus upon benzanilide, Gerhardt obtained a chloride, $C_{13}H_{10}NCl$, which yields with ammonia a crystalline substance. It can scarcely be doubted that this compound is the derivative of benzonitrile corresponding to methenyldiphenyldiamine,

$$C_{13}H_{10}NCl + H_3N = C_{13}H_{12}N_2 + HCl.$$

II. "On a New Series of Bodies Homologous to Hydrocyanic Acid."— II. By A. W. Hofmann, LL.D., F.R.S. Received August 31, 1867.

In a letter submitted to the Royal Society some weeks ago I directed attention to a new series of homologues and analogues of hydrocyanic acid, generated by the action of chloroform on the primary monamines. As a representative of this group of bodies, I described the cyanide of phenyl, the formation and the properties of which had been almost exclusively the subject of my researches.

I have followed up the study of these new bodies, which have become more and more attractive to me in proportion as I investigated their nature. Being formed in a well-defined reaction, endowed with properties quite unexpected, stable in certain cases, and of extreme alterability in others, capable of the most varied reactions that can be imagined, these bodies possess all the characters which invite a detailed examination. Thus I find myself at the threshhold of a long investigation, the results of which I beg permission to submit to the Royal Society in the order in which they present themselves.

Cyanide of Ethyl.

After having fixed in the phenylic group the general characters of the reaction, my attention was very naturally directed to the ethylic series. For this purpose, it was first necessary to procure ethylamine in rather considerable quantities. Happily in this case the liberal cooperation, so often experienced, of my friend Mr. E. C. Nicholson, was again at hand. Interesting himself with a cordiality, for which I cannot sufficiently thank him, in the continuation of my researches on the ethylic bases, Mr. Nicholson had placed at my disposal the product of the action of ammonia on iodide of ethyl produced in a single operation performed in one of his great autoclaves on 20 kilogs. of iodide of ethyl.

Thanks to the happy alliance between science and industry, which characterizes our times, I was thus enabled to study the transformation of ethylamine under the influence of chloroform on a rather large scale.

On gradually introducing a mixture of an alcoholic solution of ethylamine and chloroform into a retort containing powdered potassic hydrate, a most powerful reaction takes place; the mixture enters into ebullition, and a liquid distils over, the penetrating odour of which surpasses anything that it is possible to conceive. Besides the odoriferous body the product of the distillation contains ethylamine, chloroform, alcohol, and water, and a considerable number of rectifications are required in order to isolate the cyanide of ethyl from this mixture.

As the substance is rather volatile, the frequently repeated fractional distillations become a most painful operation, and more than once, while I have been engaged in these experiments, my laboratory has been almost inaccessible. Thus with a temperature of 30° I have found it desirable to

interrupt for the time the preparation in the pure state of the cyanide of ethyl, and to resume it at a more favourable season.

I was nevertheless curious to study, even now, a true homologue of cyanide of ethyl in order to compare its properties with those of cyanide of phenyl. The boiling-points of the amylic compounds being within convenient limits, I was induced to select the amyl-series as presenting the greatest chance of success.

On submitting amylamine to the action of chloroform, the same phenomena are observed as in the analogous reaction between chloroform and aniline. One molecule of amylamine and one molecule of chloroform contain the elements of one molecule of cyanide of amyl and three of hydrochloric acid:—

$$\underbrace{C_5H_{13}N}_{\text{Amylamine.}} + \underbrace{CHCl_3}_{\text{Chloroform.}} = \underbrace{C_6H_{11}N}_{\text{Cyanide of amyl.}} + 3HCl.$$

The cyanide of amyl is a transparent colourless liquid lighter than water, insoluble in water, but dissolved by alcohol and ether, of an oppressive odour, resembling at the same time that of amylic alcohol and of hydrocyanic acid. Its vapour possesses, in a still higher degree than that of the cyanide of phenyl, the property of producing on the tongue an insupportably bitter taste, and of giving rise in the throat to the sensation of suffocation, so characteristic of hydrocyanic acid.

The cyanide of amyl may be distilled without decomposition. It boils at 137° C., that is, at a temperature 8° lower than the boiling-point of its isomer, capronitrile. It will be remembered that the boiling-point of cyanide of phenyl is lower than that of benzonitrile.

Under the influence of alkalies and acids, the cyanide of amyl behaves in the same manner as the phenylic cyanide. Though only slightly attacked by alkalies, it is decomposed by acids with a violence which is almost explosive; a short ebullition with water is sufficient to transform it into formic acid and amylamine:—

$$\underbrace{C_6H_{11}N}_{\text{Cyanide of amyl.}} + 2H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}} + \underbrace{C_5H_{13}N.}_{\text{Amylamine.}}$$

In order to fix this equation by numbers, I have carried out the reaction by means of dilute sulphuric acid. The formic acid was then distilled off and transformed into a sodium-salt, and analyzed as formate of silver; the residue in the retort furnished, on addition of an alkali, amylamine in considerable quantities. It was identified with that obtained from cyanate of amyl, both by the determination of its boiling-point and by the analysis of the platinum-salt.

The transformation of the cyanide of amyl, like that of the cyanide of phenyl, does not take place at a single step; intermediate combinations corresponding to methenyldiphenyldiamine and to phenylformamide are produced, but I have not yet obtained them in a state of purity.

I have designated the body described in this note by the name of *cyanide of amyl*; I am of course aware that the same name has been given to the substance produced by the action of cyanide of potassium on the sulphamylates; but as the latter compound, in consequence of its transformation into caproic acid and ammonia, has a right to the name *capronitrile*, I have thought it desirable to distinguish, provisionally at least, the new product by the name of cyanide of amyl.

The examination of the cyanides of amyl and phenyl establishes in a positive manner the existence of a group of bodies isomeric with the nitriles derived from the ordinary alcohols and phenols.

I have not as yet pursued more minutely the study of the other terms of these groups; in fact the field opened by these new observations presents questions much more attractive. The existence of the new homologues of hydrocyanic acid allow us to foresee the formation of quite another series of homologues of cyanogen. These bodies will be produced by the action of chloroform on the diamines. Ethylene-diamine, for example, will thus be transformed into the dicyanide of ethylene:—

$$\underbrace{C_2H_8N_2}_{\text{Ethylene-diamine.}} + \underbrace{2CHCl_3}_{\text{Chloroform.}} = \underbrace{C_4H_4N_2}_{\text{Dicyanide of ethylene.}} + 6HCl.$$

I am now occupied with the study of the action of chloroform on ethylene-diamine, and I propose shortly to inform the Royal Society whether experiment confirms the predictions of theory.

III. "On a New Series of Bodies Homologous to Hydrocyanic Acid."—III. By A. W. Hofmann, LL.D., F.R.S. Received September 7, 1867.

The new cyanides isomeric with the nitriles, which I have described in two previous communications, are not formed exclusively by the action of chloroform on the primary monamines. On perusing the papers describing the examination of the organic cyanides, we see at a glance that the chemists who investigated them have had in their hands at the same time the isomeric cyanides with which I am engaged.

In fact everyone who has distilled mixtures of sulphomethylate, sulphethylate, or sulphamylate of potassium with the cyanide of the same metal, will remember the repulsive odour possessed by the products so obtained. This odour only disappears in proportion as the product is purified, and especially after its treatment with acid, in order to remove the ammonia, and with oxide of mercury to separate the hydrocyanic acid.

Dumas, Malaguti, and Le Blanc, in their researches on the nitriles, mention the insupportable odour possessed by the cyanides obtained by the cyanide-of-potassium process; while the products obtained by the dehydration of the ammoniacal salts by means of phosphoric anhydride have a very agreeable aromatic odour.

In a research made by Mr. Buckton and myself on the transformations of the amides and nitriles under the influence of sulphuric acid, we repeatedly had occasion to prepare acetonitrile (cyanide of methyl) and propionitrile (cyanide of ethyl) by the distillation of a sulphomethylate or sulphethylate with cyanide of potassium. In our paper we mention substances of a formidable odour which appeared in these reactions, and we describe the efforts we made in order to isolate them. But as they are only formed in small quantity, we had to give up the attempt.

Mr. E. Meyer*, who has also been occupied with cyanide of ethyl, but who employed another method of preparation, encountered the same bodies. By acting on cyanide of silver with iodide of ethyl in sealed tubes, he obtained, together with iodide of silver, an unstable compound of cyanide of silver and cyanide of ethyl; and there was formed in the same reaction a liquid of an overwhelming odour. This latter, on distillation, presented the characters of a mixture from which it was impossible to isolate a product with a constant boiling-point. When treated with an acid the odour disappeared, and the solution contained ethylamine which was identified by the analysis of the platinum-salt. These are certainly the characters of the cyanides formed by the action of chloroform on the primary monamines; and it cannot be doubted that Mr. Meyer has had in his hands the ethyl-term of the series of cyanides which I am studying, both in the combination with cyanide of silver and in the complex liquid which accompanied it.

If such results did not particularly attract the attention of chemists, it was owing to the fact that the author failed in ascertaining the complementary product of ethylamine, namely, formic acid. Mr. Meyer, besides, states that his research remained unfinished; and thus it will be understood how experiments otherwise so carefully carried out should have fallen into an oblivion from which neither the author nor any other chemist has endeavoured to recal them during the many years which have elapsed since their publication.

In consequence of the examination of the bodies produced by the action of chloroform on the primary monamines, these old experiments have acquired a new interest; and it appeared to me, for more than one reason, that it would be desirable to repeat them, making use of the experience gained by my late researches.

For this purpose I have submitted cyanide of silver to the action of several organic iodides.

The iodides of methyl and ethyl act very slowly on cyanide of silver at the ordinary temperature; but the reaction takes place at the temperature of boiling water.

After a digestion of about ten hours, the transformation is complete; a brown solid matter is formed, having the appearance of paracyanogen, together with a yellowish oily layer possessing in a marked manner the odour of the isomers of the nitriles.

* Journal für praktische Chemie, vol. lxvii. p. 147.

As several preliminary experiments gave indications of a rather complicated reaction, and as it would have been difficult for me readily to obtain sufficient substance by operating in sealed tubes, I performed the experiment in the amylic series, supposing that the higher boiling-point of the iodide of amyl would render it more easy of attack. My expectation was indeed fulfilled: two molecules of cyanide of silver and one molecule of iodide of amyl act on one another with extreme violence at the boiling-point of the latter. It is convenient to operate on a moderate scale, so as to be carefully protected from the escaping gases, which consist of equal volumes of amylene and hydrocyanic acid, mixed with a small quantity of the cyanide of amyl.

The experiment was made in a retort adapted to the lower end of a condenser, the upper end of which was connected with a series of washing-bottles. In the first a small quantity of cyanide of amyl was condensed; the second contained water intended to absorb the hydrocyanic acid; the third one water and bromine in order to transform the amylene into bromide, of which I was thus enabled to collect a considerable quantity during my researches.

After an hour's digestion, the reaction is finished, and the residue in the retort consists of a dark viscous mass, becoming almost solid on cooling; this is a mixture of iodide of silver and a combination of cyanide of silver and cyanide of amyl. The reaction then takes place according to the equation:—

$$\underbrace{C_5H_{11}I}_{\text{Iodide of amyl.}} + \underbrace{2AgCN}_{\text{Cyanide of silver.}} = AgI + \underbrace{AgCN, C_5H_{11}CN.}_{\substack{\text{Compound of cyanide of} \\ \text{silver and cyanide of amyl.}}}$$

But simultaneously a certain quantity of cyanide of amyl splits into amylene and hydrocyanic acid:—

$$\underbrace{C_5H_{11}CN}_{\text{Cyanide of amyl.}} = \underbrace{C_5H_{10}}_{\text{Amylene.}} + \underbrace{CHN.}_{\substack{\text{Hydrocyanic} \\ \text{acid.}}}$$

This secondary transformation depends principally on the manner in which the operation is conducted; it may give rise to very great loss if the reaction be rather tumultuous.

It was now necessary to separate the cyanide of amyl from the residue in the retort. Up to the present time I have found no other means of effecting this than by submitting the residue to dry distillation; in this operation a further quantity of hydrocyanic acid and amylene is disengaged, and a liquid distils over, which on rectification boils between 50° and 200°. By submitting it to fractional distillation it was found that the first part still contained a quantity of amylene, whilst the latter products had become inodorous. The intermediate portion, rectified several times, finally exhibited a constant boiling-point between 135° and 137°.

The liquid which distils at this temperature is perfectly pure cyanide of amyl. It possesses all the properties which I have described in my previous communication, and is characterized especially by its odour and by the facility with which, under the influence of hydrochloric acid, it splits into formic acid and amylamine. I have not yet completely examined the products boiling at a higher temperature, but everything seems to show that they consist, partly at least, of capronitrile.

The experiments which I have just described show, in a positive manner, that the same bodies can be obtained by the action of chloroform on the primary monamines, and by the treatment of cyanide of silver with the alcoholic iodides. In the latter process many secondary products are obtained; but by a more complete study perhaps it may be modified so as to diminish their quantity.

However this may be, the study of the action of the alcoholic iodides upon silver-salts deserves to be resumed; and it is very probable that in many cases it will be found that the bodies so produced will be but isomeric with those obtained by the ordinary processes.

For the special researches in which I am engaged at the present time, the observations just described have a particular interest; they permit us, in fact, to produce the isomeric cyanides without first preparing the primary monamines; they are especially important with reference to the generation of the polycyanides. The polyamines, in fact, are little, if at all, known up to the present, whilst the iodides of methylene and ethylene and iodoform are easy to procure.

If I have not yet succeeded in preparing a dicyanide of ethylene, $C_4 H_4 N_2$, isomeric with Mr. Maxwell Simpson's cyanide, it is because I have not had at my disposal a sufficient quantity of ethylene-diamine. I now hope to obtain this body by submitting cyanide of silver to the action of iodide of ethylene.

In conclusion, I may be permitted to announce as very probable the existence of a series of bodies isomeric with the sulphocyanides. Already Mr. Cloez has shown that the action of chloride of cyanogen on ethylate of potassium gives rise to the formation of an ethylic cyanate possessing properties absolutely different from those belonging to the cyanate discovered by Mr. Wurtz. On comparing, on the other hand, the properties of the methylic and ethylic sulphocyanides with those of the sulphocyanides of allyl and phenyl, we can scarcely doubt that we have here the representatives of two groups entirely different, and that the terms of the methylic and ethylic series, which correspond to oil of mustard and to the sulphocyanide of phenyl, still remain to be discovered. Experiments with which I am now engaged will show whether these bodies can be obtained by the action of the iodides of methyl and ethyl on sulphocyanide of silver.

I must not conclude this note without expressing my thanks to Messrs. Sell and Pinner for the hearty cooperation that they are giving me in these researches.

IV. "Second Supplementary Paper on the Calculation of the Numerical Value of Euler's Constant." By William Shanks, Houghton-le-Spring, Durham. Communicated by the Rev. Professor Price, F.R.S. Received August 29, 1867.

When $n=2000$, we have

$$1+\tfrac{1}{2}+\tfrac{1}{3}+\ldots\tfrac{1}{2000}$$
$$=8{\cdot}17836\ 81036\ 10282\ 40957\ 76565\ 71641\ 69368\ 79354$$
$$66740\ 91251\ 77402\ 20409\ 26320\ 14205\ 58039\ 78429$$
$$87946\ 27554\ 87631\ 13645+$$

$$E={\cdot}57721\ 56649\ 01532\ 86060\ 65120\ 90082\ 40243\ 10421\ 59335$$
$$93995\ 35988\ 05772\ 51046\ 48794\ 94723\ 80546$$ (last term is $-\frac{B_{12}}{24\ .\ 2000^{24}}$).

Here the 60th decimal place in the value of E is the same when n is 2000 as it is when n is 1000.

When $n=500$, we have in the value of E, 60th decimal and onwards........	1 53865 48677 &c.
„ 1000, „ „ „	2 02455 61942 &c.
„ 2000, „ „ „	2 51046 48794 &c.
By subtracting the first of these three from the second, we have	48590 13265
By subtracting the second from the third, we have	48590 86852

It is somewhat remarkable that these differences are the same to five places of decimals; and it may be observed that the value of E will probably be changed and extended very slowly indeed by employing higher values of n. The remark in the previous Supplementary Paper*, as to n being 50000 or even 100000 in order to obtain probably about 100 places of decimals in E, seems, the author now thinks, to be not well founded; and he hesitates even to conjecture what number of terms of the Harmonic Progression should be "summed" to ensure accuracy in the value of E to 100 decimals.

* Proceedings, vol. xv. p. 429.

November 21, 1867.

Lieut.-General SABINE, President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows:—

President.—Lieut.-General Edward Sabine, R.A., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., LL.D.

Secretaries.— { William Sharpey, M.D., LL.D.
George Gabriel Stokes, Esq., M.A., D.C.L., LL.D.

Foreign Secretary.—Prof. William Hallows Miller, M.A., LL.D.

Other Members of the Council.—Frederick Augustus Abel, Esq.; William Benjamin Carpenter, M.D.; Prof. A. Cayley, LL.D.; Jacob Lockhart Clarke, Esq.; John Evans, Esq.; Capt. Douglas Galton, C.B.; John Peter Gassiot, Esq.; John Hall Gladstone, Esq., Ph.D.; Sir Rowland Hill, K.C.B., D.C.L.; William Huggins, Esq.; Thomas Henry Huxley, Esq., Ph.D.; Prof. John Phillips, M.A., LL.D.; Prof. Andrew Crombie Ramsay, LL.D.; Colonel William James Smythe, R.A.; Lieut.-Col. Alexander Strange; Thomas Thomson, M.D.

The President stated that Colonel John Le Couteur, who by reason of non-payment of his annual contribution ceased to be a Fellow of the Society at the last Anniversary, had applied for readmission; and an extract from his letter to the Council was read, explaining the circumstances under which, during his absence on the Continent, the omission of payment had taken place. Notice was accordingly given that the question of Colonel Le Couteur's readmission would be put to the ballot at the next meeting.

Mr. Gassiot, Mr. Gwyn Jeffreys, Sir John Lubbock, Mr. Rennie, and Mr. Savory, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

Mr. W. Boyd Dawkins was admitted into the Society.

The following communications were read:—

I. "On a New Class of Bodies Homologous to Hydrocyanic Acid." I.—By A. W. Hofmann, F.R.S. Received August 20, 1867. (See page 144.)

II. "On a New Series of Bodies Homologous to Hydrocyanic Acid." —II. By A. W. Hofmann, LL.D., F.R.S. Received August 31, 1867. (See page 148.)

III.—"On a New Series of Bodies Homologous to Hydrocyanic Acid."—III. By A. W. Hofmann, LL.D., F.R.S. Received September 7, 1867. (See page 150.)

IV.—"Second Supplementary Paper on the Calculation of the Numerical Value of Euler's Constant." By WILLIAM SHANKS, Houghton-le-Spring, Durham. Communicated by the Rev. Professor PRICE. Received August 29, 1867. (See page 154.)

V. "Addition to Memoir on the Resultant of a System of Two Equations." By A. CAYLEY. Received August 6, 1867.

(Abstract.)

The elimination tables in the memoir on the Resultant of a System of two Equations (Phil. Trans. 1857, pp. 703–715), relate to equations of the form $(a, b\,..\between x, y)^m=0$, without numerical coefficients; but it is, I think, desirable to give the corresponding tables for equations in the form $(a, b\,..\between x, y)^m=0$, with numerical coefficients, which is the standard form in quantics. The transformation can of course be effected without difficulty, and the results are as here given. It is easy to see *à priori* that the sum of the numerical coefficients in each table ought to vanish; these sums do in fact vanish, and we have thus a verification as well of the tables of the present addition as of the tables of the original memoir, by means whereof the present tables were calculated.

VI. "Contributions to the History of Methylic Aldehyde." By A. W. HOFMANN, LL.D., F.R.S. Received September 30, 1867.

"The aldehyde of the methyl-series is not known;" all the chemical manuals say so, and for the last twenty years my students have been duly informed thereof. It will scarcely appear strange that more efforts to become acquainted with that body should not have been made, since the masterly picture which Liebig has delineated of the aldehyde *par excellence* embraced as it were the history of the whole class, and of course also of the aldehyde in question. Nevertheless methylic aldehyde deserves our consideration for more than one reason. As one of the simplest terms of the monocarbon-series, occupying a position intermediate between marsh-gas and carbonic acid, as a link of transition connecting methylic alcohol and formic acid, as either aldehyde or acetone, according to the point of view from which we look upon it, the compound CH_2O illustrates a greater variety of relations than any one of the higher aldehydes. But in addition to the interest with which the methyl-compound has thus always been invested, this substance possesses special claims upon our attention at the present moment. Our actual method of treating organic chemistry for the purposes of instruction almost involves the necessity of starting from the methyl-series. The simplest of aldehydes thus acquires quite an especial importance, and all those who, like the author of this note, are engaged in teaching, cannot fail to have sadly missed a compound which is the carrier of such varied and interesting considerations.

The desire which I have frequently felt in my lectures of developing the idea of the genus aldehyde, when speaking of the methyl-compounds, has

more than once induced me to attempt the preparation of methyl-aldehyde, but it was only at the conclusion of my last summer course that I succeeded, to a certain extent at all events, in attaining the object of my wishes.

A substance possessing the composition and the properties of methylic aldehyde is formed with surprising facility if a current of atmospheric air, charged with the vapour of methylic alcohol, be directed upon an incandescent platinum spiral.

The bottom of a strong three-necked bottle, of two litres' capacity, is covered to the height of about five centimetres with moderately warm methylic alcohol. The first neck is provided with a tube descending to the very surface of the liquid; into the second is fixed a loosely-fitting cork, which carries the platinum spiral; the third one, lastly, communicates with the upper end of a condenser, the lower end of which is fastened into a two-necked receiver. This receiver is in its turn connected with a series of wash-bottles, and the last of these communicates with a water-jet aspirator, by which a current of air can be sucked through the whole system.

The apparatus being disposed in this manner, the platinum spiral is heated to redness and introduced into the three-necked bottle. After a few minutes the flameless combustion of the methyl-alcohol begins to manifest itself by the evolution of a vapour powerfully affecting the nose and eyes. Gradually the temperature of the apparatus rises, and soon droplets of a colourless liquid are condensed in the receiver. The formation of methyl-aldehyde is now fairly proceeding, and if the current of air be appropriately adjusted, the platinum spiral remains incandescent for hours and even for days. There is no difficulty in collecting from 50 to 100 grammes of a liquid rather rich in methyl-aldehyde.

Instead of establishing the current of air by a water-jet aspirator, a pair of bellows may be conveniently employed. I have often used with advantage the bellows of an ordinary glass-blowing table. This mode of proceeding is more particularly adapted to the requirements of the lecturer, who is thus enabled, by simply accelerating the movement of the foot, to enliven the combustion, so as to keep the whole spiral in a state of incandescence. By thus proceeding it happens, however, occasionally that the gaseous mixture in the three-necked bottle is fired; but these explosions are perfectly harmless, the whole effect being the forcible ejection of the loosely-fitting cork which carries the platinum spiral.

The liquid which is being collected in the receiver has all the properties which theory assigns to the aldehyde of the methyl-series, or, more properly speaking, to its methyl-alcoholic solution. When rendered slightly alkaline by a few drops of ammonia, and mixed with nitrate of silver, it yields, on gently warming, a silver mirror of irreproachable perfection, which is indeed more readily and more certainly produced than with the aldehyde of the ethyl-series. The reduction in this case is the result of two consecutive reactions; in the first stage the aldehyde yields formic acid, which in the second stage is converted into water and carbonic acid.

On heating the methyl-alcoholic solution of the aldehyde with a few drops of a fixed alkali, the liquid becomes turbid on ebullition, acquires a yellowish coloration, and soon deposits droplets of a brownish oil, possessing in the highest degree the peculiar odour of ethyl-aldehyde-resin.

After the observation which I have mentioned, it was scarcely doubtful that the product of the slow combustion of methylic alcohol contained the aldehyde of this alcohol in considerable proportion. Nevertheless it appeared necessary to fix the nature of this compound by some numbers. The commencement of the vacations being at hand, there was but little hope of preparing the liquid in sufficient quantity for the purpose of obtaining the aldehyde, which will probably be found to be either gaseous at the common temperature or extremely volatile, in a state of purity for analysis. Under these circumstances I have been compelled to limit myself to the preparation of an easily accessible derivative of methyl-aldehyde possessing a characteristic composition, and the analysis of which would not be less conclusive than that of the aldehyde itself. The slight solubility and the powerfully crystalline tendencies of the sulphaldehyde of the ethyl series could not fail to indicate the direction in which I had a right to hope that the object which I was aiming at might be accomplished.

If a current of sulphuretted hydrogen be passed through the methyl-alcoholic solution of methyl-aldehyde, the liquid becomes turbid after a few minutes, and on allowing the saturated solution to stand for some hours, a body of an alliaceous odour begins to be separated at the bottom of the flask. If the liquid be now mixed with half its volume of concentrated hydrochloric acid, and heated to ebullition, it becomes limpid, and solidifies on cooling into a mass of felted needles of dazzling whiteness. These needles fuse at 218°; they are volatile without decomposition. Slightly soluble in water, they are more readily dissolved by alcohol, and still more so by ether. For the purpose of analysis they were recrystallized from boiling water, in order to exclude free sulphur, with which they might have possibly been contaminated. The numbers obtained in the analysis of the crystals unmistakeably establish their nature. The white crystals, as might have been expected, have the composition of the sulphaldehyde of the methyl-series, CH_2S.

The analysis of the sulphur-compound fixes, of course, the presence of the corresponding oxygen compound among the products of the slow combustion of methylic alcohol.

A more minute examination of methylic aldehyde and its derivatives remains still to be made. It will be absolutely necessary to isolate the oxygen-term and to determine its vapour-density, in order to ascertain its molecular weight. If we remember the facility with which the aldehydes are polimerized, the question presents itself, whether the aldehyde formed by the slow combustion of methylic alcohol is represented by the formula

$$CH_2O,$$

or a multiple thereof. A similar remark applies to the sulphur-derivative.

It deserves, moreover, to be mentioned that a compound isomeric with methylic aldehyde, the dioxymethylene ($C_2 H_4 O_2$) of M. Boutlerow, is known already; also that a sulphur-compound of the formula

$$CH_2 S$$

has been obtained by M. Aimé Girard, who observed that bisulphide of carbon is reduced by the action of nascent hydrogen with disengagement of sulphuretted hydrogen.

In the course of next winter I propose to perform some further experiments on the product of the slow combustion of methylic alcohol for the purpose, if possible, of isolating methylic aldehyde in a state of purity, and of thus completing this inquiry.

VII. "On the New Reflecting Telescope to be used at Melbourne, Australia." In a Letter to the President. By the Rev. Dr. Robinson, F.R.S. Received October 15, 1867.

Observatory, Armagh, October 14, 1867.

My dear Friend,—As you express a wish to know my recent impressions respecting the great telescope, I can say that they are very satisfactory. When I saw it six weeks ago the first of the two great specula was just polished; and though the essential parts of the equatoreal were in position, and one could estimate the facility with which it could be managed, the optical part of the telescope remained incomplete. Now, I found the great and small specula in their places, a finder of four inches aperture attached, the circles divided, and the clock for driving the telescope enshrined in the pier. One thing was wanting, weather fit for trying its power; and during eighteen nights there was only one of even middling goodness. That, however, was sufficient to prove that the instrument is thoroughly up to its intended work. I examined several nebulæ and clusters, with whose appearance in Lord Rosse's six-feet reflector I am familiar, and the difference was far less than I expected. I may specify among them 51 Messier, whose spirals were seen on strong aurora, and the nebula in Aquarius, with its appendages like the ring of Saturn. Its definition of stars is very good: α Lyræ had as small and sharp an image as I ever saw on such a night; and a few pretty close double stars were well and clearly separated. Part of this is probably due to the lattice-tube, which permits the escape of heated air, but more to the figure of the speculum, which is truly parabolic. The peculiar nature of the mounting brings the circles completely within reach of the observer's assistant; and the mechanical appliances for the motions in right ascension and polar distance are so perfect, that we set the instrument on the faint objects which we were examining with great facility and rapidity. One man can reverse the telescope in a minute and a quarter; the quick motion in polar distance is of course far easier, and the slow one acts more like the tangent screw of

a circle than the mover of such a huge mass. The clock is rather gigantic, but does its work with great precision, the objects which I examined remaining steady on the wire as long as I watched them; and there is an ingenious and new contrivance for suiting its speed to planets or the moon. There remain but a few matters to be completed; the second great speculum is nearly polished, the glass small one is ready; the micrometer and observing-chair are not commenced, nor the photographic apparatus and spectroscope. These two last are no part of Mr. Grubb's contract; but the Committee thought themselves justified by the correspondence in ordering them, as their cost is small, and they will add greatly to the utility of the telescope. In the fine sky of Melbourne it will, I trust, yield spectroscopic results surpassing any that have as yet been obtained*. That it will realize fully the expectations of the people whose enlightened liberality has ordered its construction I am quite certain; but I am not so certain that it will retain its present perfection *very* long if exposed without some shelter. It is true that Mr. Cooper's great achromatic has stood exposed to the rain and wind of Connaught for more than thirty years, and is still serviceable; but besides its inferior size it is of coarser workmanship, and is provided with fewer of those beautiful contrivances which in this instrument make its movements so easy. At Melbourne the rain of Markree is not to be feared; but if one may judge from its position on the verge of a great continent, and from the analogy of India and the Cape, another enemy is to be dreaded, the fine dust which winds from the interior will probably bring. This would find its way into all the bearings, and besides clogging their action would grind them out of truth. The danger of this induces me, after careful discussion with Messrs. Le Sueur and the two Grubbs, to lay before you my views, which (if you think them sound) you may hold it advisable to mention to the authorities of Victoria.

Three modes occur to me of covering the telescope. In any case it must be surrounded by a wall, for the comfort of the observer and to prevent intrusion. This wall may support a moveable covering of such a kind as to let the instrument be pointed to every part of the sky.

The most usual form of this covering is a dome running on a circular railway, and with an opening or chase on one side reaching from its base to its summit, and closed by a sliding shutter. The disadvantages of this plan are, that the performance of the telescope is somewhat injured by currents of warm air rising through the chase, and that it is much heavier and more costly than either of the others. In this instance its diameter could not be less than 56 feet; and though that magnitude is not beyond the resources of an accomplished engineer, yet it is not one to be encountered without the prospect of some adequate advantage. The largest dome which I know (Sir James South's, of 36 feet diameter) is a total failure; but this does not weigh much with me—for, though planned by the celebrated Brunel, it transgresses against the elements of mechanical science.

* I send a photogram of the instrument taken last week.

A much simpler plan is the sliding roof. In this case the walls are rectangular, enclosing a space rather broader than the instrument, and about three times as long. The longer sides carry two rails, on which runs a kind of house long enough to cover the instrument and pier, and high enough to clear the latter. That end which at Melbourne will be its north is closed by doors, which are opened at the time of observation, and the roof is wheeled away, leaving all in the open air. It will be the cheapest and least bulky of the three. Its defects are, that the open end presents some engineering difficulty, that the roof will hide about 12° under the pole, and that the whole machinery is exposed to any dust that may be stirring during the hours of observing.

That which appears the best is the revolving roof. Its vertical part is a prism of sixteen sides, six feet high, springing from a ring of cast iron, which revolves by rollers on a circular rail borne by the wall. The top is nearly flat, with a chase large enough to let the telescope work freely, which can be covered by sliding shutters. The tube, when in use, would project through the chase, and be essentially in free air, at other times could be lowered and completely sheltered; while the other parts would be as well protected as under a dome. In this case the internal diameter should be about 46 feet, with a chase 16 feet wide. These dimensions would give complete command of the heavens, and such a roof would give less hold to a high wind than either of the others. I enclose a rough sketch of its framing. The panels and the three girders at the top to be of angle-iron, light but strong, and these covered with tin plate. If it were adopted, I suppose the frame would be made here, sent out in pieces, and put together and covered on its arrival. The weight would be about 5 tons. As to its cost, no estimate can be given, as labour costs more at Melbourne than with us; but in Ireland it would be about £1200.

I will conclude this long letter by telling you how much I am satisfied with our selection of the astronomer who is to work this glorious instrument. He is not a mere mathematician; such a one might be very helpless when he came to the practical details of observing, but he is thoroughly versed in its optical and mechanical requirements, and in the daily work of an observatory. For this last he has been trained by Professor Adams during the past year; one of the Committee, Mr. Warren De la Rue, the first of celestial photographers, has instructed him in the mysteries of that surprising art; and for the last three months he has been constantly in Mr. Grubb's works, studying all the mechanism of the telescope (of which I see he has acquired full command), and taking an active part in the polishing of the great specula. He seems fully to understand this most delicate process; and it is my opinion that, if repolishing becomes necessary, he is fully competent to do it successfully.

I may therefore congratulate you in full hope on the inestimable harvest of discovery and triumph which will soon crown this magnificent enterprise.

Yours ever,

(Signed) T. R. Robinson.

VIII. "Copy of a Despatch addressed to Her Majesty's Secretary of State for Foreign Affairs by the British Consul at Canea, Crete, giving an account of an Earthquake which took place in that Island." Communicated by the Lord STANLEY, F.R.S. Received October 17, 1867.

Canea, Crete, September 23, 1867.

MY LORD,—I have the honour to acquaint your Lordship that two shocks of earthquake were felt here at 5½ on the afternoon of the 19th instant, and again on the following morning, after an interval of twelve hours. The latter shock was severe, and lasted about ten seconds. The oscillations were horizontal, and appeared to proceed from east to west. Several houses have been cracked and otherwise damaged, and one of the old Venetian galley-arches fell sideways in a block, killing a Turkish sentry and a hospital attendant. A remarkable phenomenon occurred that morning soon after the earthquake. The sea receded at the rate of about ten inches per minute, until, attaining a maximum depression of some four feet, it gradually rose again above its former level. The water in the wells was affected in the same degree, and on rising it became much agitated. At 5¾ A.M. the sea-water was tepid, with a temperature of 87° (Fahr.), being equal to that of the air.

The morning was unusually still and sultry, and it was not until the afternoon that the sea subsided into its normal condition, tempered by a cool northerly breeze. A strong eddy had set in, causing much damage to shipping, several of the smaller craft parting their cables and coming into collision with each other. With reference to the state of the sea, it is worthy of note that the copper-bottom of Her Majesty's Ship 'Wizard,' now in this port, became suddenly clean and bright.

Many of the town inhabitants have fled to the country, and the Generalissimo, with His Highness's staff, has since removed to the camp outside the town.

Mr. Hall, second master of Her Majesty's Gunboat 'Wizard,' has kindly given me the following results of his observations :—

Thursday, Sept. 19, 1867.—5.40 P.M. Therm. 86° Fahr. Sea-water 85°.—Experienced the shock of an earthquake, four seconds' duration.

Friday, Sept. 20.—5.44 A.M. Mean time. Place Chr. Therm. 85° to 87° Fahr. Sea-water 87°.—Experienced a second shock of an earthquake, five seconds' duration, followed by a series of violent effluxes and influxes of the sea at intervals of ten minutes.

6.0 A.M. Therm. 78°. Water 77°.—Rate of efflux and influx *on perpendicular* three feet in four minutes.

6.45 A.M.—Duration of efflux and influx six minutes. Rate of do. six minutes. Perpendicular two feet in four minutes.

Noon.—Duration of efflux and influx four minutes.

I have the honour to be, with the highest respect, my Lord,

Your Lordship's most obedient humble Servant,

(Signed) C. H. DICKSON.

November 30, 1867.

ANNIVERSARY MEETING.

Lieut.-General SABINE, President, in the Chair.

Mr. Savory, on the part of the Auditors of the Treasurer's Accounts appointed by the Society, reported that the total receipts during the past year, including a balance of £666 1*s.* 6*d.* carried from the preceding year, amount to £4932 3*s.*; and that the total expenditure in the same period amounts to £4436 12*s.* 9*d.*, leaving a balance of £483 16*s.* at the Bankers, and of £11 14*s.* 3*d.* in the hands of the Treasurer.

The President announced that the Council had elected Sir Philip Egerton as a Trustee of Sir John Soane's Museum for the next five years.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

On the Home List.

Major-General John George Bonner.
William Brinton, M.D.
Edward Burton, Esq.
William Cotton, Esq., D.C.L.
Walter Crum, Esq.
Colonel Sir George Everest, C.B.
Michael Faraday, Esq., D.C.L., LL.D.
Professor John Goodsir.
William John Hamilton, Esq.
Sir John Jacob Hansler, Knt.
Sir William Snow Harris, Knt.
Sir William Lawrence, Bart.
Ashhurst Majendie, Esq.
Commander James Mangles, R.N.
John Mercer, Esq.
Thomas Richardson, Esq., M.A., Ph.D.
Charles Milner Ricketts, Esq.
William Parsons, Earl of Rosse, K.P., M.A., LL.D.
James Smith, Esq.
Sir James South, Knt.
Alexander John Sutherland, M.D.
Lord Justice Sir George James Turner, Knt., D.C.L.
Robert Warington, Esq.
John, Lord Wrottesley, M.A., D.C.L.

On the Foreign List.

Alexander Dallas Bache.

Fellows elected since the last Anniversary.

Lord Chief Justice The Right Hon. Sir William Bovill.
William Baird, M.D.
W. Boyd Dawkins, Esq.
Baldwin Francis Duppa, Esq.
Albert C. L. G. Günther, M.D.
Julius Haast, Esq., Ph.D.
Capt. Robert Wolseley Haig, R.A.
Daniel Hanbury, Esq.
John Whitaker Hulke, Esq.
Edward Hull, Esq.
Edward Joseph Lowe, Esq.
James Robert Napier, Esq.
Benjamin Ward Richardson, M.D.
J. S. Burdon Sanderson, M.D.
Henry T. Stainton, Esq.
Charles Tomlinson, Esq.

The President then addressed the Society as follows:—

Gentlemen,

The year which has passed since I last addressed you has been to us a mournfully eventful one. Death has taken from us three of our most eminent and respected Members. Two were my predecessors in this Chair, for whom, only a few months ago, we might well have hoped that many years of useful life were yet in store. In regard to the third, the declining health of Faraday (one of the greatest names in our annals) for a considerable time forbade such hope in his case.

Whilst you deplore with me the losses we have sustained, you will be prepared to read with strong interest the biographical notices of these distinguished men, which are very shortly to appear in our obituary. It is well that I am able to announce that these will be so soon in your hands, for even the feeble attempt on my part to do justice to such a theme on this occasion, which might otherwise have been expected of me (altogether inadequate as it must have been), would have occupied the greater part, if not the whole, of the time claimed by our more ordinary topics.

I pass therefore at once to the relation of the action taken by the Royal Society in the promotion of science in the past year.

At the last Anniversary I gave an account of the progress made up to that time in printing the Catalogue of Scientific Papers. I am happy now to be able to announce that the first volume lies before us ready for publication. It comprehends a portion of the first part of the Catalogue, in which the titles are arranged alphabetically, according to authors' names, and extends from A to Clu. An explanatory Preface and Introduction are prefixed, as is also a list of the periodical works from which the titles have been extracted, with the abbreviations under which they are referred to. I need scarcely remind you how often the appearance of the first volume of a series is necessarily subject to delays not incident to the succeeding volumes. This has happened in the present case; and we may now confidently anticipate that the work will progress rapidly and uninterruptedly. You are aware that the work is being printed at Her Majesty's Stationery Office, and that arrangements will be made, with the approval of the Government, for distributing a certain number of copies as presents to scientific institutions and other parties, while the remainder will be offered for sale at such a price as may defray the cost of printing.

The attention of the President and Council has continued to be much occupied during the past year in aiding, at the request of Her Majesty's Government, in the reorganization of the meteorological department of the Board of Trade, and in preparing the preliminary arrangements of a system of British Land-Meteorology to be carried out under the authorization of that Board.

In my Anniversary Address of last year, I brought before you, as fully as

the time at our command would permit, the reasons which had influenced both the Government and the Royal Society in desiring the establishment in this country of meteorological observatories conducted on a systematic plan, and directed towards the attainment of a more perfect knowledge of the meteorology of our country than we at present possess.

The scheme, which, at the instance of the Board of Trade, had been suggested by the President and Council, consisted in the establishment of six or seven observatories, well distributed over the area of the British Islands, furnished with self-recording instruments on the pattern of those devised and in use at the Kew Observatory, and transmitting their records of the temperature, pressure, electric and hygrometric state of the atmosphere, and of the direction and force of the wind to a central office, where (under the general superintendence of a committee of scientific men) they should undergo the processes of reduction and combination, and be applied to the general study of the phenomena.

The Government, acting with due caution, determined on submitting this suggestion, as well as, generally, the functions of the meteorological department of the Board of Trade as it had previously existed, to a committee of scientific and practical men to be nominated by the Government itself, the Royal Society being invited to name one of the members.

The suggestion to which I have adverted, of the establishment of a small number of meteorological observatories supplied with self-recording instruments for the purpose of making a full, accurate, and *continuous* record of meteorological phenomena at certain selected stations, appears to have received the unqualified and emphatic approval of the committee, and to have been viewed by them as the most effectual means of supplying a secure and adequate basis for the discussion of the variations of the weather in the British Islands. Self-recording instruments are spoken of in the report as likely to prove of eminent local and international utility; it is anticipated that the establishment of observatories furnished with them in England may be expected to confer a wide benefit, that they would give precision and fulness to the charts of our own weather, and would set an example that foreign governments would probably soon follow, and that they would afford material in a very acceptable form to meteorologists at home and abroad for the discussion of weather phenomena.

The Board of Trade, after reference to the Admiralty and to the Treasury, adopted the Report of their Committee in a "Circular" dated November 29th, 1866; and at the same time asked the Royal Society whether they would be willing to name a committee of their own members to give their gratuitous services in the organization of the observatories recommended, and in the general superintendence of the Meteorological Department.

The public service thus requested was unhesitatingly undertaken; and on the 13th of December, 1866, a committee was named of eight of the Fellows of the Royal Society who were willing to devote themselves to the onerous and responsible duties of such an undertaking. Estimates

were required, and were furnished by them, of the probable cost of the organization, and of the annual expense of the Observatories. In August of the present year (1867) the estimates were passed in the House of Commons; and I have now the satisfaction of announcing, on the part of the Committee, that they have reason to believe that in January 1868, being not more than six months after the passing of the estimates, Observatories supplied with self-recording instruments which have been prepared and verified at Kew, working under competent superintendence, and with a trained staff at each Observatory, will have commenced their observations at Falmouth, Kew, Stonyhurst, Armagh, and Glasgow, and that there is ground of expectation that, in a month or two later, Valentia and Aberdeen will be added to the list.

The estimates passed in August 1867 will have defrayed the expenses of organization and maintenance of these seven Observatories until the 31st of March 1868 (the close of the present financial year). The *continuance* of the Observatories must necessarily depend upon the disposition of Government to recommend, and of the House of Commons to supply, the necessary funds. The Superintending Committee of the Royal Society are prepared for either alternative, viz. either to continue their general superintendence, or to regard their honourable and laborious undertaking as terminated. In the former case, it will become their office to trace the variations of the weather, as presented in the continuous and well-distributed records over the area of the British Islands, viewed in conjunction with the telegrams from the Ports and with the information received from other countries, and thus so to contribute to a knowledge of the laws which govern those variations as to enable meteorologists gradually, and as far as may be possible, to place the practice of forecasting the weather on a sound and dependable basis.

The Report of the Committee of the Board of Trade contains many valuable suggestions regarding the treatment which the information accumulated in the office of the meteorological department of that Board should undergo, with the view of extracting from it the information it is capable of affording on the meteorological statistics of the *Ocean*, and specially of the parts most frequented by British ships. This great branch of meteorological research, so eminently befitting a great maritime and commercial nation, was most prominently urged on the consideration of Her Majesty's Government by the President and Council of the Royal Society in a letter dated February 22, 1855; and in the subsequent establishment of the Meteorological Department of the Board of Trade it was recognized as being one of the chief functions of the office so constituted. The collection of a very considerable mass of information, embodied in the logs of ships to which instruments and instructions have been supplied, has been the result; but comparatively little advance appears to have been made in the labour of extracting, collating, combining, and discussing the valuable materials thus obtained. The work, both of collecting further information, and of discussing and arranging for communication to the public the

information already in the office and that which may hereafter be obtained, has been resumed under the general superintendence of the Meteorological Committee of the Royal Society, profiting by the valuable suggestions contained in the Report of the Committee of the Board of Trade. This forms the second portion of the duties which they have taken upon themselves. A third portion consists in the endeavour to make available for the benefit of mariners the information which reaches the office by telegraph early in the day as to the state of the weather at different points of the coast.

A copy of this information is transmitted, by the first post after its reception, to any port which desires to receive it.

If the authorities at any port require any special telegraphic intelligence, it is furnished to them without unnecessary delay, on their agreeing to defray half the cost of transmission of the message, and stating the precise nature of the information required.

Lastly, the Committee are prepared to convey, free of cost, telegraphic intelligence of the existence of any serious atmospherical disturbance which may have come to their knowledge, to all ports to which it appears to them that such information would be of importance. Such a telegram may be, for example :—

"Storm from West at Penzance and South coast."

On the receipt of such a message the local authorities are expected to hoist a drum as a general warning, on seeing which masters of vessels or other interested persons may learn by inquiry at the local office (or by other arrangements) the precise nature of the information received, together with any additional particulars which may have been transmitted from the central office.

It is clearly understood by all parties that any telegraphic message of a warning nature (like the example here mentioned) is merely meant to imply that *there is a serious atmospherical disturbance existing along a certain region of coast, and consequently that there is, or may be, danger impending in other districts.*

Some such arrangement as that which has been now described was the subject of early discussion between the Board of Trade and the Committee of the Royal Society. The arrangement as adopted was proposed by the Committee in a letter dated the 8th of June, being some weeks before the estimates had passed, and consequently before they were authorized to incur any expenditure whatever on the public account. It has been since approved by the Board of Trade, and is now in operation.

The telegraphic messages, which are now limited to a notice of "*existing facts*," are obviously capable of extension hereafter, in proportion as the basis upon which sound meteorological anticipations may rest shall be enlarged; and this we may reasonably hope for, as one of the fruits of the establishment and action of the "Land Meteorological Observatories."

Meantime a not unimportant preliminary measure has received its due consideration. From an early period the attention of the Committee had

been drawn to the importance of improving, as far as possible, the *quality* of the intelligence received from the coast stations. With this view they gave directions that all the telegraph stations at which observations are made should be inspected—a practice which had never before been carried out. The inspection of all the stations situated in the British Islands has now been completed. It is hoped that, as the result of these measures, the accuracy and consequent value of the reports received will be in future materially improved; and such desirable improvement has indeed already been in part effected.

The four-foot reflector destined for the Melbourne Observatory approaches its completion, with a full prospect of its being ready to proceed to its destination early in the coming year, under the charge of Mr. Le Sueur. The spectroscope and photographic apparatus, which are to be used with it, are in progress. A question has arisen as to the expediency of providing it with some roof or covering which, while admitting of the telescope being directed to any part of the heavens, shall be an efficient protection for it from the weather at all times when not in actual use. Three designs for this purpose, viz. a dome, a sliding roof, and a revolving roof, with the estimated cost of each, have been supplied by Mr. Grubb, and have been sent to Melbourne to be submitted to the choice of the Board of Visitors of the Observatory; their decision may be expected to arrive very shortly, and Mr. Grubb is prepared to carry it into effect with all promptitude. There is therefore full reason to expect that this magnificent instrument will be at work in the splendid field which awaits its operations, in the hands of a thoroughly skilled and competent observer, before our next Anniversary.

The Superintending Committee, whose assistance in this important undertaking has been unremittingly given, have sustained a loss, which all who hear me will appreciate, in the lamented decease of the Earl of Rosse. Deeply as the death of one so highly gifted, and who devoted his gifts to such high objects, is to be deplored, it is some consolation that his son and successor is one who will add to the lustre of their name. He is already known to you by the important paper on the Nebula of Orion which was read at the close of our last session, and is now in course of publication in the Philosophical Transactions. This paper appears clearly to show that, in the course of the last fifteen years, considerable changes have taken place in that remarkable object, such as cannot be attributed either to atmospheric difficulties of vision, or changes in the instrument, or in the observer's eye. It confirms fully the researches of Mr. Huggins, and at the same time explains what had presented some difficulty, the absence of a continuous spectrum when the telescope shows a multitude of stars.

In conformity with the course of proceeding directed by the Melbourne Board of Visitors, in the event of such an emergency as the death of one of the three members of the Committee of Superintendence, I have consulted

with the two surviving members, Dr. Robinson and Mr. De la Rue, and, in agreement with their recommendation, have named the present Earl of Rosse as their associate.

The year 1868 will be signalized by the occurrence of a total solar eclipse of almost the greatest possible duration, affording therefore more leisure than usual for such observations as can only be made during the brief interval of the totality. The total phase will be visible in India, but elsewhere only in countries practically unavailable. Recent observations on the spectra of the heavenly bodies render spectroscopic observations of the red protuberances and of the corona a matter of peculiar interest at the present time. The President and Council have therefore considered how far they might contribute to a full use of so rare an opportunity in regard to these more especially *physical* phenomena.

Having already experienced, in the case of the pendulum experiments in India reported in my last year's Address, the advantage of acting in concert with the distinguished officer who now holds the post of Superintendent of the Great Trigonometrical Survey, Colonel Walker of the Royal Engineers, and having ascertained his readiness to charge himself with the practical arrangements which would be required for the observation of the eclipse, the President and Council determined on employing a portion of the Parliamentary grant placed at their disposal for the present year in the preparation of the necessary instruments, consisting of a telescope of five inches aperture, by Messrs. Cooke and Sons, mounted as a portable equatoreal with clock movement, and provided with a star spectroscope; and as clouds might interfere with the observations with this instrument at the critical moment, they have added four direct spectroscopes for observing the general character of the spectra of the red protuberances and the corona, and have entrusted them to Colonel Walker, to be placed in the hands of different observers.

It has happened fortunately that a son of Sir John Herschel, an assistant in the Trigonometrical Survey, was about to return to India from leave of absence in this country, and, being applied to, expressed his readiness, subject to the approval of Colonel Walker, to undertake any desired share in the observations, and to make himself acquainted with, and receive instruction in, the use of the instruments before his departure, as well as to take charge of their conveyance to India. This arrangement having received the cordial approval of Colonel Walker, has been duly proceeded with, and Lieutenant Herschel with the instruments is now on his way to India.

Mr. Hennessey, First Assistant on the Indian Trigonometrical Survey, having expressed in a letter to the President his wish to render available for scientific researches, not incompatible with his professional duties, his residence for great part of the year at the elevated station and in the clear atmosphere of Mussoorie (7000 feet above the sea), his offer, which had received the cordial sanction of Colonel Walker, has been embraced;

advice and instructions for the observation of the terrestrial lines of the solar spectrum, and for observations of the zodiacal light (for which the situation is particularly favourable) and for other desirable inquiries have been sent to him; and Lieutenant Herschel is taking out spectroscopes, prisms, actinometers, and other suitable instruments which the Society has provided for his use.

The Society have been already apprised of the desire of the Government of Mauritius to establish in that colony a Magnetical Observatory, working with the instruments and adopting the methods of discussing the results as practised at Kew. Early in the Session a communication was received from the Colonial Office, conveying the Earl of Carnarvon's request for the opinion of the Royal Society regarding the instruments to be employed in, and the plans for the building of, a new observatory, which should be both magnetical and meteorological. After full communication and discussion with Mr. Meldrum, Director of the Mauritius Observatory, who had arrived in England, a reply was returned to the Colonial Office particularizing the remaining instruments still required for the complete equipment of such an observatory, together with plans for the buildings and estimates for the whole, submitted by Mr. Meldrum, and approved by the President and Council. The instruments have been prepared, verified, and practised with by Mr. Meldrum, at Kew, and are ready to proceed to their destination.

The self-recording magnetical instruments prepared and verified at the Kew Observatory by the request of the Government of Victoria have been forwarded to their destination, and are now at work at Melbourne under the superintendence of Mr. Ellery. An application received in the course of the present year from the same colony for self-recording meteorological instruments on the pattern of those at Kew has been already complied with in part, and will be so fully as soon as the present urgent demands for the British Land Meteorological Observatories shall have been supplied.

Since the information conveyed in my last year's Address respecting the Magnetical Observatory at Bombay, Mr. Chambers's application for self-recording instruments, similar to those at Kew, has been received at the India Office, accompanied by the approval and recommendation of the Bombay Government. Happening to arrive about the time when Lord Cranbourne had referred the general subject of the *Astronomical* Observatories in India to the Astronomer Royal, the *Magnetical* Observatory at Bombay was included in the reference, the distinction between astronomical and magnetical observatories not being perhaps very clearly understood. Mr. Chambers's application for efficient instruments seems, however, to have dropped out of consideration, and (to use an ordinary term) was "shelved." A renewal of the application made through Sir Bartle Frere, the Governor of Bombay, caused a second reference to the Astronomer Royal, from

whose official reply, printed by the Bombay Government, I extract the following passage:—

"I should certainly recommend that any new magnetic observatory be furnished with magnetic instruments on the pattern of those at Kew. I would propose that an answer of this tenor be given to the Superintendent of the Bombay Observatory, that the Secretary of State for India in Council, having taken the opinion of the Astronomer Royal, approves highly of his (the Superintendent's) acting in concert with the Kew Observatory."

Still, possibly from inadvertence, Mr. Chambers's application for the instruments required to enable him to obey the instruction of "acting in concert with Kew" yet remains without a reply. In the meantime the cost of the observatory runs on, whilst the very valuable services for magnetical science, approved and recommended by the Astronomer Royal, and which Mr. Chambers, having been educated at Kew, is singularly qualified to carry into execution, are in abeyance for want of the necessary instrumental means to execute them, the whole cost of which would be under £400. We may hope that this oversight will shortly be rectified.

The publication in the last year of the verification and extension of La Caille's Arc of the Meridian in Southern Africa, by Sir Thomas Maclear, Astronomer at the Cape of Good Hope, announces the completion of a national work, pursued unremittingly for above thirty years, and establishing by its result a conclusion too important in its scientific interest to pass without recognition by the Royal Society. Our sole knowledge of the figure of the southern hemisphere rests on the arc of the meridian measured by La Caille, and now remeasured and extended by Maclear. The original measurement, notwithstanding the well-known ability of the great astronomer under whose superintendence it was executed, has not commanded confidence. The degree inferred from it is far too great, and, if accepted, would lead to the conclusion that the dimensions of the two hemispheres are dissimilar. But La Caille's triangles were observed with a quadrant, not with a circle, and were therefore liable to errors of eccentricity and of figure; while the effects of local attraction, if recognized at all, were very imperfectly appreciated. These considerations induced Maclear, shortly after his appointment to the Cape Observatory, to plan the verification which he has now accomplished. Pursuing the still earlier inquiries of Sir George Everest, he succeeded, though with considerable difficulty, in recovering La Caille's terminal stations; and, aided by the advice and encouragement of Sir John Herschel (then at the Cape) and of the Astronomer Royal, he commenced the work of a remeasurement in 1836. The proceedings were necessarily tedious; the measurements of the base, of the triangles, and of the zenith distances were repeated to an extent and with precautions unpractised at the earlier period. The zenith distances were observed with the sector with which Bradley discovered the aberration of light and the nutation of the earth's axis, entrusted to Maclear by the Ad-

miralty; and though made more fit for use in the field by improvements suggested by the Astronomer Royal, the transport of an instrument at once ponderous and delicate, through a wild and rugged country, was an undertaking of no ordinary difficulty; but it was performed without injury. The terrestrial angles were taken with a 20-inch circle by Jones, and a smaller theodolite by Reichenbach, both of remarkable precision. The base, from which all the distances were derived, was measured with the compensation bars used in the Irish Triangulation. Thus, in respect to the means employed, this arc of the meridian may be regarded as inferior to none on record. A full account of the whole was completed in 1866, and has been published by the Admiralty in two quarto volumes. It does not confirm the abnormal value obtained by La Caille, but shows a probable cause for the discordance. La Caille's northern station was in a hollow surrounded by mountains, one of which, half a mile distant to the north, was a mass of rock 2000 feet high, and others, at distances somewhat greater, were still near enough to create disturbance. A station so situated was obviously ill suited to be a terminal station; and the triangulation was extended across an immense plain of sand (the Bushman's Flat) to a point without any visible source of local attraction. By this extension, and by a similar one to the south, Maclear's arc has an amplitude nearly four times as great as that of La Caille, and is on this account, as well as on account of the greater accuracy in detail, far more deserving of confidence. The degree which is derived from it is 1133 feet shorter than that of La Caille; and as La Caille's is 1051 feet longer than that given by the spheroid, which, according to Airy, represents the average of northern arcs, it is evidently a near approximation to the truth. This is even more distinctly shown by the close agreement of the latitudes computed from the geodetic measurements with those given by the sector—that of the north extremity being 0''·4 in defect, that of the south extremity 0''·5 in excess.

The Philosophical Transactions of the past year contain an important memoir by Mr. Abel, F.R.S., to which has been assigned the distinction of forming the Bakerian Lecture for the year. It is a most careful and exhaustive treatise upon the circumstances which influence the *stability* of gun-cotton. He has made numerous experiments, both in the laboratory on small quantities, and in store upon large quantities, of the material; and some of his experiments have been carried on upon the same sample for three or four years. The results arrived at in these investigations show that gun-cotton, purified according to Von Lenk's directions, may be kept either in the open air or in closed vessels, and may be exposed to diffused daylight for very long periods, without undergoing any change. The preservation of the material for between three and four years has been perfect. By prolonged exposure to *sunlight*, ordinary gun-cotton suffers a gradual decomposition, which is somewhat more rapid when the cotton is damp than when it is dry; but, even under these circumstances, the change produced by several months of exposure is of a very trifling nature, and may

be counteracted by very simple means, which in no way interfere with the essential qualities of the material. All ordinary products contain small quantities of organic (azotized) impurities, which are comparatively unstable. It is the presence of these impurities in *ordinary* gun-cotton which gives rise to the development of free acid when the substance is exposed to a high temperature; and the acid thus generated may eventually exert a destructive action upon the pure portion of the mass (or true gun-cotton), and thus establish a decomposition which is materially accelerated by heat. Mr. Abel has, however, arrived at the important practical conclusion that this mischief can be averted by neutralizing the acid as it is liberated; and this is readily effected by distributing through the finished gun-cotton a small quantity, say one per cent., of carbonate of soda. By adopting this precaution, damp gun-cotton may be stored, closely packed, in large quantities, and may be exposed to a heat equal to 212° Fahrenheit in confined spaces for long periods, without undergoing any alteration. The introduction into the finished gun-cotton of one per cent. of carbonate of soda affords, therefore, security to the material against any destructive effects of the highest temperature to which it is likely to be exposed, even under very exceptional climatic conditions.

Actual immersion in water is not necessary for the most perfect preservation of gun-cotton. The material, if only damp to the touch, does not sustain the smallest change even if closely packed in large quantities. If as much water as possible be expelled from wet gun-cotton by the centrifugal extractor, the cotton is obtained in a condition which, though only damp to the touch, is perfectly non-explosive. It is therefore in this condition that all reserve stores of the substance should be preserved, and that it should be transported to distant places. The proper proportion of the carbonate of soda may be conveniently introduced by being dissolved in the water by which the gun-cotton is wetted.

It is in this immunity from danger in storage and in transport that properly prepared gun-cotton possesses so great an advantage over gunpowder.

Mr. Abel has also elaborately investigated the effects of various kinds of defective preparation of gun-cotton, combined with systematically varied circumstances of exposure to heat, moisture, and light of the products so obtained. It is seen by these investigations that modifications in the processes of conversion and purification, which appear at first sight of a very trifling nature, exert most important influences on the composition and purity, and consequently on the stability, of the product. It is shown by Mr. Abel that to such causes are to be attributed the conclusions condemnatory of gun-cotton which had been drawn by foreign chemists of considerable note.

The distrust, not unreasonably entertained at the time, of the stability of the material, was a principal cause of the desire on the part of Her Majesty's Government to refer the subject of gun-cotton to a Committee which should include some scientific members taken from the Royal Society. This great and primary question being now satisfactorily solved, the remaining secondary

questions regarding the best forms or modes of adaptation of this material to some of the varied exigencies of the naval and military services, in which its employment might be preferable to that of gunpowder, may be regarded as more properly belonging to the executive professional officers of Her Majesty's Navy and Army.

I have the great satisfaction of stating, on the part of the Committee, that no injury to life or limb has taken place in the course of their experiments.

At the Nottingham Meeting of the British Association, the sum of £100 was granted to a Committee for the purpose of exploring the Tertiary Plant-beds of North Greenland. The collections of fossil vegetable remains from the arctic regions which had been brought to this country and presented to various museums by Sir Leopold M'Clintock, Capt. Inglefield, and others, have all been sent to Prof. Oswald Heer, of Zurich, so well known for his researches into the Tertiary Fossil Flora of Europe. The similar collections which were preserved in the museums of Denmark and Sweden had also been submitted to the same authority; and the results of his investigation seem to show that North Greenland enjoyed, during part of the Tertiary epoch, a climate very much milder than that which is now experienced in those latitudes.

The description of the fossils is in process of publication by Prof. Heer; and in order to procure additional information on this very interesting subject, the grant was made by the Association.

The Greenland Committee, finding that Mr. Edward Whymper, one of their members, was proceeding to Greenland in the summer of 1867, handed the entire sum over to him; and finding that additional funds would be requisite, they made application to the Royal Society, who gave £200 from the Government Grant Fund, placed at the disposal of the Society.

Mr. Whymper has now returned from Greenland with a large and valuable collection of specimens. These will at once be subjected to examination; and when this work has been effected, a complete series of specimens will be deposited in the British Museum, according to the conditions of the grant, as made by the British Association and by the President and Council of the Royal Society.

I proceed to the award of the Medals.

The Copley Medal has been awarded to Karl Ernst von Baer of St. Petersburg, For. Memb. of the Royal Society, for his discoveries in Embryology and Comparative Anatomy, and for his contributions to the Philosophy of Zoology.

Forty-one years ago it was believed by all the great authorities in anatomy and physiology that the embryos of man and of other *Mammalia* originated in quite a different manner from those of oviparous animals. As to the latter, everyday observation of fowls, snakes, frogs, and fishes had been

sufficient to demonstrate, even without special scientific investigation, that their young arose within eggs, and that these eggs were preformed within the body of the virgin female. Further, the researches of Fabricius, of Harvey, of Haller, of Caspar Friedrich Wolff, of Cruikshank, of Döllinger, of Pander, of Prevost and Dumas, and of Dutrochet and Cuvier had traced back the embryos of the *Ovipara* to a very early stage, and had thrown much light upon the changes undergone by those of the *Mammalia.* But the earliest condition of the mammalian embryo was unknown. Haller's authority was still predominant; and Haller's researches had enabled him to discover in the mammalian uterus, shortly after impregnation, nothing more than a semifluid substance, in which, it was imagined, the embryo appeared by a kind of crystallization. The origin of this semifluid embryonic matter was sought for in a mixture of the seminal fluid of the male with the contents of the remarkable vesicles long before discovered by De Graaf in the ovary of the female, and called after him the Graafian follicles.

But in 1827 all such speculations were at once abolished, and the identity in mode of origin between the embryos of the *Mammalia* and those of other animals was demonstrated by a young Professor in the University of Königsberg, whose unwearied patience, sagacity, and sharpsightedness had enabled him to trace back the fœtus, step by step, to the minute egg, not a hundredth of an inch in diameter, to demonstrate that the Graafian follicle is simply the chamber in which that egg is contained, and to prove that the first step in mammalian generation, as in that of other animals, is the detachment of the egg from the organ of the parent in which it is developed.

This capital discovery forms one of the grounds upon which the Copley Medal is to-day awarded to the sometime Professor in Königsberg, but now, and for many years past, the honoured Academician of St. Petersburg, Karl Ernst von Baer.

Von Baer's great discovery was not the result of accident, but was the reward of long-continued and most laborious investigations into the development, not only of the chick and of the mammalian embryo, but of other animals. The first part of a great work entitled "Ueber Entwickelungsgeschichte der Thiere. Beobachtungen und Reflexionen," embodying some of the results of these inquiries, and mainly of the investigations into the development of the chick, appeared in 1828; the second part, in which the mammalia are chiefly treated of, was published in 1837.

It is impossible to overestimate the value of this remarkable book, or to doubt the great influence which it has exerted, and still exerts, upon the growth of a sound philosophy of Biology.

At the time of its appearance there was nothing that could be compared with it, as a special monograph upon the formation of the chick, or as a treasury of accurately observed facts respecting the development of the *Vertebrata* in general, or as an exposition of the significance of development and of the bearing of the study of embryology upon classification. And, as a whole, it may be safely said that it remains at the present day, though

surrounded by the splendid works of Rathke, Bischoff, Remak, Coste, and others, *primus inter pares.*

It is to Von Baer that we owe the great generalization that all development is a progress from the general to the special—a law which has its application in wide regions not contemplated by its author. It is to him that we are indebted for the truth that zoological affinity is the expression of similarity of development, and that the different great types of animal structure are the result of different modes of development.

The authorship of the 'Entwickelungsgeschichte der Thiere,' and of the 'Beiträge zur Kenntniss der niederen Thiere' (1824–26), would have sufficiently justified the award of the Copley Medal to Von Baer had he not been the discoverer of the mammalian ovum.

Besides these labours of primary importance, the energy, versatility, and wide learning of Von Baer have been shown in multitudinous other directions—in numerous memoirs on Comparative Anatomy, Systematic Zoology, and Zoological Distribution, in most valuable and original essays on Anthropology and Ethnology, and in scientific expeditions to different parts of the widespread Russian Empire, from Nowaja Zemlja to the Caspian.

Von Baer was born in Esthonia in the year 1792. His father was a gentleman of landed property and "Ritterschafts-Hauptman" of Esthonia. Two years ago, on the occasion of the fiftieth anniversary of the venerable Academician's Doctorate, the nobility of Esthonia, headed by their present Ritterschafts-Hauptman, the Baron von der Pahlen, formed themselves into an association for the purpose of celebrating the occasion; and as a memento of proceedings honourable alike to their eminent countryman and to themselves, published the autobiography which he wrote at their request with all the accessories of typographic luxury. Thirty-six years ago the Académie des Sciences of Paris, at the instance of Humboldt, and on the report of Cuvier, awarded Von Baer a medal. In 1854 he was chosen a Foreign Member of the Royal Society.

We may rejoice that it is not yet too late to offer the highest honour at the disposal of the Royal Society of London to a man who has so long been recognized on the continent as one of the great lights of biological science, who will take his place hereafter beside Cuvier, Wolff, and Harvey.

PROFESSOR MILLER,

I will request you, on the part of the Society, to transmit this Medal to our venerated colleague, Dr. von Baer, and to express to him our hope that this testimonial of the very high esteem in which the labours of his life are held in England will be a welcome and valued addition to the honours which fitly crown his latter years.

The Council have awarded a Royal Medal to Messrs. John Bennet Lawes and Joseph Henry Gilbert for their researches in Agricultural Chemistry.

Messrs. Lawes & Gilbert have been engaged for the last twenty-four

years in a systematic series of researches upon Agricultural Chemistry, with a view of determining, by exact experiments, the principles, chemical and physiological, which are involved in the general and fundamental processes of successful agriculture.

These investigations have embraced :—

1. Researches into the exhaustion of soils, including experiments on wheat, on barley, on turnips, on clover, and leguminous crops.
2. Researches on the principles of rotation and fallow.
3. On the mixed herbage of grass-land.
4. On the process of vegetation generally, including researches upon the action of manures.
5. On the origin of nitrogen in plants (Phil. Trans. 1861).
6. Researches on the feeding and fattening of animals (Phil. Trans. 1859).

It is difficult to give in a short compass the practical conclusions arrived at from a series of investigations upon a number of subjects each so complicated in its nature, so important in its object, and continued systematically over so protracted a period. At the time your medallists commenced their experiments it was generally supposed that certain saline bodies, or so-called *mineral constituents*, were essential to the growth and development of the plant, and that such substances must be furnished to it by the soil. The necessity of a certain quantity of nitrogen was also recognized; but it was imagined, since wild plants could thrive without any artificial supply of nitrogen, that a sufficient amount of this element existed in the atmosphere (in the form chiefly of salts of ammonia) to render it unnecessary to take any steps for increasing this supply; and it was supposed that the fertility of a soil might be maintained for an indefinite period if the different mineral constituents carried off by the crop were annually returned in due quantity as mineral manure to the soil.

This *mineral-ash theory*, as it was termed, was proposed by Liebig; but it has been proved by Messrs. Lawes & Gilbert to be erroneous, as it embraces a part only of the truth.

The field experiments upon which this conclusion rests were commenced in 1843. Fourteen acres, divided into about twenty plots, were devoted to experiments upon wheat, and seven acres, divided into about twenty-four plots, to experiments upon turnips. Subsequently similar experiments were made upon beans, clover, barley, and the mixed herbage of permanent meadow-land. The general plan of the field experiments consisted in selecting fields in a condition of agricultural exhaustion, that is, in a state in which a fresh supply of manure was needed to fit the soil for the growth of another crop. Upon this exhausted soil each of the most important crops in the rotation were grown, year after year, upon the same spot, both without manure and with many different descriptions of manure, each of which was, as a rule, applied year after year to the same plot. By this means it was possible to determine the point of relative exhaustion or excessive supply of any of the constituents of the manure.

Wheat, for example, was grown year after year upon the same land for twenty-four years; turnips (with an interval of three years) for twenty-five years; and in the experiments on rotation (which comprised the "four course" of turnips, barley, leguminous crop [or fallow], and wheat) the last of the fifth "four course" rotation was completed, comprising twenty years in all.

Parallel with the field experiments, records relating to the fall of rain, atmospheric pressure, temperature of the air, and of the dew-point were kept or collated, so as to enable the observers to ascertain the effects of the varying season upon the quantity and quality of the field produce.

It soon became evident that much remained to be done in perfecting the methods of chemical analysis before comparative analyses could afford much assistance in determining the relative productiveness of different soils; and to this object our medallists addressed themselves both with skill and success.

The practical value of these experiments may be seen from the fact that, taking the results of twenty years, the annual average produce in bushels of wheat per acre without manure was $16\frac{1}{4}$, with farmyard manure exactly double, and with artificial manures $35\frac{3}{4}$ bushels, the latter being considerably more than the average produce of Great Britain when wheat is grown in the ordinary course.

The produce of wheat grown successively on the same plot without manure scarcely altered from year to year, whilst that of the turnips became reduced to nothing; the effect of a manure of superphosphate being most marked upon the turnips, whilst the employment of salts of ammonia mixed with alkaline salts and phosphates was most suitable for wheat, although these are not the manures indicated by a simple analysis of the ashes of the two crops. The authors remark, "Indeed the whole tendency of agricultural investigation seems to show the fallacy of alone relying upon the knowledge of the composition of a crop, as directing to the constituents probably more especially required to be provided for it by manures; and rather that the elucidation of agricultural principles must be looked for from a due consideration of Vegetable Physiology, as well as Chemistry, of the special functional peculiarities and resources of different plants, as well as their actual percentage composition."

The investigation into the feeding of animals was even more laborious; but it was a necessary complement to the experiments upon the growth of crops. It was directed to the solution of the following among other important problems:—

1. The amount of food consumed, and its several constituents, in relation to the production of a given live weight, for different animals.

2. The comparative development of the different organs in the fattening of animals, and their composition.

3. The relation of the manure produced, both in quantity and quality, to the food consumed.

4. The expenditure or loss, by respiration and exhalation of the animal, considered as a meat-producing and manure-making machine.

It is impossible to go into detail into this portion of the inquiry, the principal results of which are given in a paper published in the Philosophical Transactions for the year 1859.

It may be sufficient to sum up these remarks by stating that the various inquiries, to which a brief reference has been made, have been conducted with a skill, perseverance, and success which have placed their authors, by general consent, at the head of those who have pursued this important branch of experimental inquiry.

MR. LAWES and MR. GILBERT,

Receive this Medal in testimony of the Royal Society's recognition of your joint labours, and of their approval of the object to which those labours have been directed,—which, while not outstepping the wide limits of a Society devoted to the promotion of natural knowledge, is yet in an unusual degree connected with the supply of man's primary wants. It will, I trust, be more especially prized by you as marking the Society's high appreciation of the long devotion, the patient unbiassed desire for truth, and the sound scientific manner of proceeding which have characterized your investigations.

The Council have awarded a Royal Medal to Sir William Logan for his geological researches in Canada, and the construction of a geological map of that colony.

Sir William Logan was early known to English geologists for very meritorious work in the Coal-fields of South Wales, which was highly approved at the time by the authorities of the English Geological Survey, and is understood to have furnished the model for similar surveys in other British Coal-fields.

In 1843 he undertook the direction of the Geological Survey of his native country, Canada, instituted by the Provincial Government. The results of this survey have been published in Annual Reports; and a large and important volume was published in 1863, condensing the whole of the geological and palæontological information which had been amassed by Sir William and the assistants who acted under his direction.

Under difficulties of which British geologists acquainted with both countries affirm that little idea can be formed here, he has made clear the relations of all the formations of Canada to each other. These consist of Lower and Upper Laurentian rocks, Huronian, numerous divisions of the Lower and Upper Silurian strata, and the Devonian series. Most of these he correlated as far as possible with the European series and with the subdivisions described by the American geologists of the United States.

One of the most important services that Sir W. Logan has rendered to geological science was the discovery of the relations of the Laurentian rocks to each other and to the later formations. These Laurentian rocks had

been previously only called granite and gneiss, and vaguely confounded with granitic and gneissic rocks of Silurian age. Sir William first proved their great antiquity by showing that they consist of strata which had been intensely disturbed and metamorphosed before the deposition of the oldest Silurian beds. Next, he showed that the Laurentian rocks consist of two series of metamorphic strata, and that the Upper Laurentian strata or gneiss lies quite unconformably on the Lower Laurentian masses. Thirdly, he made the important discovery of the *Eozoon Canadense* in the Limestone beds of the Lower Laurentian series.

The great importance of this discovery becomes manifest when we consider the evidence of the enormous antiquity of the strata thus proved to be fossiliferous, compared with the Lingula-flags and Cambrian strata, in which the oldest previously known fossils had been found. It has seriously modified the speculative opinions of many geologists and zoologists.

PROFESSOR RAMSAY,

I will beg you to transmit this Medal to Sir William Logan, in testimony of the appreciation by the Royal Society of his arduous labours in the accomplishment of the great work of the Geological Survey of Canada, of the critical skill and acumen which he has manifested in its course, and of the high scientific importance of the discoveries which have been established by his investigations.

On the motion of Dr. Alderson, Coll. Reg. Med. Præses, seconded by by Mr. Cæsar Hawkins, it was resolved,—"That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed."

The Statutes relating to the election of Council and Officers having been read, and Mr. Stainton and Mr. Hanbury having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Lieut.-General Edward Sabine, R.A., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., LL.D.

Secretaries.— { William Sharpey, M.D., LL.D.
George Gabriel Stokes, Esq., M.A., D.C.L., LL.D.

Foreign Secretary.—Professor William Hallows Miller, M.A., LL.D.

Other Members of the Council.—Frederick Augustus Abel, Esq.; William Benjamin Carpenter, M.D.; Prof. A. Cayley, LL.D.; J. Lockhart Clarke, Esq.; John Evans, Esq.; Capt. Douglas Galton, C.B.; John Peter Gassiot, Esq.; John Hall Gladstone, Esq., Ph.D.; Sir Rowland Hill, K.C.B., D.C.L.; William Huggins, Esq.; Prof. Thomas Henry Huxley, Ph.D.; Prof. John Phillips, M.A., LL.D; Prof. Andrew Crombie Ramsay, LL.D.; Colonel William James Smythe, R.A.; Lieut.-Col. Alexander Strange; Thomas Thomson, M.D.

The thanks of the Society were voted to the Scrutators.

Receipts and Payments of the Royal Society between December 1, 1866, and November 30, 1867.

Receipts	£	s.	d.
Balance at Bank and on hand	666	1	6
Annual Subscriptions, Admission Fees, and Compositions	1647	16	0
Rents	253	8	0
Dividends	1407	18	1
Ditto, Trust Funds	283	0	6
Sale of Transactions, Proceedings, &c.	425	19	3
Sale of Land at Acton (23½ poles)	100	0	0
Repayments	57	19	8
	£4932	3	0

Payments	£	s.	d.	£	s.	d.
Salaries, Wages, and Pension				1031	10	0
Purchase of £600 Consols				567	0	0
The Scientific Catalogue				289	2	6
Books for the Library and Binding				250	1	9
Printing Transactions and Proceedings, Paper, Binding, Engraving, and Lithography				1509	5	0
General Expenses (as per Table subjoined)				351	15	10
Rumford Medal Fund	136	16	3			
Donation Fund	251	0	0			
Wintringham Fund	33	8	0			
Copley Medal Fund	4	15	5	437	17	8
F. A. Abel, Bakerian Lecture	4	0	0			
Rev. T. S. Evans, Fairchild Lecture	2	19	0			
Dr. Sanderson, Croonian Lecture	2	19	0			
				4436	12	9
Balance at Bank				483	16	0
Balance of Catalogue Account				9	17	3
" Petty Cash Account				1	17	0
				£4932	3	0

WILLIAM ALLEN MILLER,
Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £126 0s. 0*d.* per annum.
Estate at Acton, Middlesex (34 A. 2 R. 27¼ P.), £100 10s. 0*d.* per annum.
Fee Farm near Lewes, Sussex, rent £19 4*s.* per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
£14,000 Reduced 3 per Cent. Annuities.
£29,569 15s. 7*d.* Consolidated Bank Annuities.
£513 9s. 8*d.* New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.

Scientific Relief Fund.

	£	s.	d.
Investments up to July 1865, New 3 per Cent. Annuities	£6052	17	8
	£6052	17	8

Dr.

	£	s.	d.
Balance	213	14	10
Donation	5	0	0
Dividends	178	11	2
	£397	6	0

Cr.

	£	s.	d.
By Grants	200	0	0
Balance	197	6	0
	£397	6	0

Statement of Income and Expenditure (apart from Trust Funds) during the Year ending November 30, 1867.

				£	s.	d.
Annual Subscriptions				1099	16	0
Admission Fees				160	0	0
Compositions				388	0	0
Rents				253	8	0
Dividends on Stock (exclusive of Trust Funds)				997	3	10
„ on Stevenson Bequest				500	14	3
Sale of Transactions, Proceedings, &c.				425	19	3
Sale of Land at Acton				100	0	0
Chemical Society, Tea Expenses	£18	0	0			
Linnean Society, Tea Expenses	14	0	0	43	0	0
Zoological Society, Tea Expenses	11	0	0			
Geographical Society, Gas at Evening Meetings	8	4	8			
Cambridge Local Examination Committee, Gas	5	0	0	14	19	8
Sundry Petty Receipts	1	15	0			
				3983	1	0
Income available for the Year ending Nov. 30, 1867				3983	1	0
Expenditure in the Year ending Nov. 30, 1867				3431	15	1
Excess of Income over Expenditure in the Year ending Nov. 30, 1867				£551	5	11

				£	s.	d.
Salaries, Wages, and Pension				1031	10	0
The Scientific Catalogue				289	2	6
Books for the Library				149	3	3
Binding ditto				100	18	6
Printing Transactions, Part II. 1866, and Part I. 1867	475	4	1			
Ditto Proceedings, Nos. 87–95	320	13	9			
Ditto Miscellaneous	63	14	11	1509	5	0
Paper for Transactions and Proceedings	278	16	0			
Binding and Stitching ditto	77	18	6			
Engraving and Lithography	292	17	9			
Fittings, Cleaning, and Repairs				51	16	1
Miscellaneous Expenses				57	17	1
Coal, Lighting, &c.				107	10	0
Tea Expenses				58	1	9
Fire Insurance				28	11	6
Taxes				8	19	2
Advertising				11	9	6
Postage, Parcels, and Petty Charges				27	10	9
				£3431	15	1

WILLIAM ALLEN MILLER,
Treasurer.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£2 12*s*. annually.	£4 annually.	Total.
November 30, 1866.	5	49	302	3	267	626
Since elected			+6		+10	+16
Since compounded..			+2		−2	
Since deceased		−1	−12	−1	−11	−25
November 30, 1867.	5	48	298	2	264	617

"Observations on the Anatomy of the Thyroid Body in Man." By George W. Callender, Lecturer on Anatomy at St. Bartholomew's Hospital. Communicated by James Paget, Esq. Received June 8, 1867*.

By examination of the thyroid body in the fœtus, we learn that it has from an early period, much the same relations and appearances as belong to it in childhood, and during the adult condition, and we observe those diversities of its parts which are exceptionally recognized during the later periods of life. We may thus trace out the origin of such exceptional conditions, and notice, more especially, how the isthmus of Eustachius and the pyramid of Lalouette are connected with the formation of the thyroid, and depend for their after characters upon early changes during development of size.

As I have reason to believe that the formation of the thyroid in man may be fairly reexamined, I shall venture to refer, in the first place, to some of the opinions advanced with regard to its earliest appearances.

The late Mr. Gray† has alluded to the views of Huschke, Arnold, Bischoff, and Goodsir, respecting the development of this body. It is enough for my present purpose to state that Arnold considered the thyroid to be developed from the membranous air-tube, where the larynx is formed, whilst Goodsir‡ thought that it originated in that portion of the membrana intermedia of Reichert which remains in connexion with the anastomosing vessels between the first and second aortic arches, or carotid and

* Read June 20, 1867: see abstract, p. 24.

† Philosophical Transactions, 1852.

‡ Philosophical Transactions, 1846.

subclavian arteries. At first, he states, no isthmus is observed (in sheep), but presently lateral masses join across and in front of the base of the heart and root of the neck. Then the thyroid separates from the thymus, from which it differs in not being divided into lobules. Simon[*] has no reason for believing that its origin has any particular relation to that of the thymus. Undoubtedly, he adds, there is a period when it is impossible to say how much of the unshaped blastema of the neck belongs to one organ, how much to another, but so soon as the microscope can discover the first traces of their development, it likewise affords unquestionable evidence of their distinctness, and shows each as separate in itself, and as peculiar in structure, as at any later period of growth.

Handfield Jones[†] found in a fœtal sheep two inches long that the thyroid presented the usual appearance; it possessed an isthmus, and in a human fœtus of four months and a half, the isthmus appeared of the same standing as the lateral masses.

The absence of an isthmus in an entire class, that of birds, the observations of Gray on the formation of this body in the chick, and the suggestions of Meckel, Cruveilhier and others, countenance the supposition that the thyroid is developed from two lateral masses.

There are no reliable observations respecting the development of the pyramid, but Haller and Arnold have hazarded the opinion that it is probably the duct of the thyroid during fœtal life.

In describing the following dissections, I may state that the specimens were examined as they came into my possession without selection, save for age, so that they may be assumed to represent very fairly the conditions commonly existing during fœtal life.

In a human fœtus, measuring in length eight-tenths of an inch (between the seventh and eighth week), the thyroid is a body of a pale yellowish colour, lying across the front of the trachea, just below the mass out of which the cartilages of the larynx are being developed. It is closely connected with the trachea and with the lower edge of the larynx, either of which would be torn in endeavouring to remove it, but the thyroid is easily uncovered by stripping off the parts superficial to it, and has no connexion with these or with the thymus (fig. 1)[‡]. Although consisting at this period of but one piece, the thyroid is deeply notched, and thus looks as though made up of three distinct lobes, one sometimes bifid, in the centre, and this is the smallest, and another on either side elongated and inclined upwards by the side of the larynx. Similar divisions are seen in a fœtus measuring two inches and eight-tenths in length (fig. 2)[§], the thyroid consisting of three lobes, one being central, a

* Essay on the Thymus Gland, 1845.

† Cyclopædia of Anatomy and Physiology, Art. Thyroid.

‡ This specimen is in the Museum of St. Bartholomew's Hospital.

§ See also specimen No. 1.

second on the right side, measuring one-tenth of an inch from end to end, and a third on the left side, measuring one-fourteenth of an inch.

Relations with the Thymus.

That the connexions of the thyroid are from the first with the larynx and trachea, rather than with the thymus, is rendered more probable by the appearances observed in the young of animals. In a fœtal rabbit (fig. 3)*, eight-tenths of an inch long, the thymus may be seen to consist of two lobulated masses, lying side by side just above the heart and its great vessels, broader at the base towards the thorax, diverging a little as they pass upwards, and ending in the root of the neck by a somewhat pointed extremity. The trachea in the middle line is surmounted by the larynx, but at its upper extremity is a minute elevation, contrasting by its pale colour with adjacent parts; and connected with this are two divergent ridges, of the same pale colour, which embrace, horseshoe fashion, the lower portion of the larynx, tapering as they ascend, and resembling, so far as the mere look is concerned, the division of the trachea at its lower extremity into the two bronchi. In a fœtal pig (fig. 4)†, one inch and two-tenths in length, the thyroid is notched below, thus acquiring, though somewhat indistinctly, a three-lobed appearance; and here also, whilst firmly attached to the trachea, it is no way visibly in relation with the thymus.

One cannot but be attracted by this connexion with the trachea, on which tube the thyroid (even if it be not developed from the membranous air-tube) buds and attains some little size, a formation reminding one of that of the lungs coming out from the front wall of the œsophagus, that is, from the trachea, and of the view of Mr. Simon, as afterwards expressed by the editors of Cuvier ‡ respecting the thyroid, "C'est la fausse branchie, branchiole des poissons." Indeed, from its relation to the air-tube during the early period of life, or in fish to the vertebral or hyoidal extremity of the gill, from its curious alternation with the supplementary gill of Broussonet§, and from its structure (Kölliker), it may be not inaptly referred to as a pseudo-lung rather than as an associate with the thymus and the so-called ductless glands.

I may add that, in the human fœtus, no distinct evidence of the thyroid appears to me to exist before the sixth week, up to which time it cannot, I believe, be isolated from the structures in the front of the neck; it seems to come out from the blastema in the form of a mass in front of the trachea, which quickly acquires an imperfectly lobed condition, but I have not been able to distinguish at any period, during development of size, three completely distinct parts.

* See also specimen No. 8.

† See also specimen No. 9.

‡ Leçons d'anatomie comparée, 2me édit. tom viii. 1846, p. 678.

§ Simon, Philosophical Transactions, 1844, p. 301.

The Isthmus.

In the dissections already referred to, the presence of a middle portion, and its equal development with the lateral lobes, leads to the inference that this central part is present from the earliest period, and that the thyroid isthmus is not formed by a growing together of two distinct side-pieces. Yet in the human fœtus, at four months, Fleischmann and Meckel say that they found, as they described them, two lateral lobes only; and the hare, for example, has been written of as having but two distinct lateral masses, as also were the Cetacea until Professor Turner* explained that the thyroid of a well-grown male porpoise was a single mass extending across the trachea, of which the median portion could hardly be described as an intervening isthmus; for in its supero-inferior diameter it equalled that of the lateral portion. In addition, however, to the dissections already detailed, I have examined the thyroid in fœtal hares, and have always found the middle portion equally developed with the side lobes, and bounded by notches, which seem to define it from them (fig. 5). With the growth of this fœtus, as also in the young of cats and of dogs, I have observed that the central part appears to flatten and to lose the rounded lobular condition, and sometimes it disappears altogether.

The Lobes.

Whilst, however, three lobes are chiefly indicated, lesser notches may be occasionally seen, and continue to be noticeable as the fœtus grows, though they are very irregular and uncertain. Thus in a human fœtus, three inches and nine-tenths long, the left lobe (fig. 6)† is divided into two portions by a deep fissure, one-half of it ascending to the left of the middle line in front of the cricoid and thyroid cartilages, and there are other notches faintly outlining a middle lobe. In a fœtus four inches and three-tenths long, the middle lobe is bifid, a cleft dividing it above; and in another fœtus, four inches and six-tenths long, the entire thyroid is very irregularly formed, broken into several lobes, but still showing at its lower margin a division into three chief portions (figs. 7 & 8)‡. Here also a process ascends, budding out from the left side, tapers almost to a point, and ends by being fixed to the under surface of the os hyoides.

Omitting lesser varieties, I will describe the following. The thyroid from a fœtus eight inches and seven-tenths long (fig. 10)§, consists of two chief lobes, which meet, but are not united, in the middle line, being separated by a deep fissure. From the left lobe, just anterior to the lower angle of the thyroid cartilage, a small process projects upwards,

* Transactions of the Royal Society of Edinburgh, vol. xxii. p. 320.

† This specimen is in the Museum of St. Bartholomew's Hospital.

‡ See also specimens Nos. 2 and 3.

§ See also specimen No. 6.

and resembles the base of the process in either figure 6 or 7, wanting the stalk-like continuation towards the os hyoides. The right lobe is somewhat irregular along its upper border; but just where it reaches the middle line there is a lozenge-shaped piece of gland, more closely connected with the right than with the left lobe, from which latter it is separated by a distinct fissure, a faintly marked line extending above for a short distance between it and the right lobe. This lozenge-shaped portion ascends, and is adherent to the lower notch in the middle line of the thyroid cartilage; its extremities are pointed, and the lower one just falls short of the level of the inferior margin of the lateral masses. In another fœtus (fig. 11)*, the left lobe, three-tenths and a half of an inch in length, is irregularly and slightly notched. The right lobe, traced towards the middle line, shows scarce a sign of a middle portion; but there is a small distinct mass adhering closely to it, and this narrowing rapidly, becomes over the thyroid a slender band, and can be traced upwards until it ends by adhering to the posterior inferior surface of the hyoid bone.

The evidence obtained from these dissectious goes to show that the thyroid is connected, in man, from a very early period with the upper portion of the air-tube. It does not consist, at all events after the seventh week, of distinct lateral masses, and the appearances it presents at that date are in favour of the middle portion being of equal standing with the rest. It is marked out, more or less distinctly, into three principal parts or lobes, but these are united at the seventh week of fœtal life and form, save exceptionally, one thyroid body.

The isthmus appears to consist of the smaller middle division uniting the other two, but there may be an absence of isthmus through failure of this union†, the middle portion joining the right or left lobe, and thus making one lateral portion larger than the other, a condition sometimes recognized in the adult; or a small middle lobe may remain distinct (fig. 10), and this, with the various irregularities observable in the lateral portions, may account for the partial and isolated outgrowths of this body in various forms of goitre‡.

The pyramid of Lalouette may be seen in figs. 6, 7, 8, 10, and 11, where it is drawn as springing from the middle, the right and the left of the thyroid. It is very commonly met with in the fœtus, and is clearly an outlying part of the body, of which the buds seen in figs. 7 and 10 are the simplest forms, and the cleft of the left lobe in fig. 6, or the distinct process in fig. 8, the larger development. Just as the cornua of the thyroid body are fixed by fibrous tissue to the wings of the hyoid bone, so also, as would be expected, any one of these processes is equally fixed to one of the adjacent cartilages, or, if prolonged upwards, to the os hyoides, as in

* See also specimen No. 7.

† See Sir Astley Cooper's specimens in the Museum of the College of Surgeons, O. 38.

‡ In the Museum of the College of Surgeons is a specimen showing enlargement of the middle lobe of the thyroid, No. 1504.

fig. 11. In the adult, the pyramid is less often met with than in the fœtus. I found it in some form or other, in ten out of forty-two adult male subjects, in the fœtus four times in eight. Thus it is probable that these outgrowths from the fœtal thyroid often shrink and disappear with advancing years.

Explanation of Figures.

Fig. 1. Thyroid from human fœtus (eight-tenths of an inch long), about three times the natural size.

Fig. 2. Thyroid from human fœtus (two inches and eight-tenths long), natural size.

Fig. 3. Thyroid from fœtal rabbit (eight-tenths of an inch long), about three times the natural size.

Fig. 4. Thyroid from fœtal pig (one inch and two-tenths long), about three times the natural size.

Fig. 5. Thyroid from fœtal hare (six inches long), natural size.

Fig. 6. Thyroid body from human fœtus (three inches and nine-tenths long).

Fig. 7. The same (four inches and three-tenths long).

Fig. 8. The same (four inches and six-tenths long).

Fig. 9. The same (six inches and four-tenths long).

Fig. 10. The same (eight inches and seven-tenths long).

Fig 11. The same (twelve inches long).

Figs. 6–11 are drawn, by measurement, the exact natural size.

December 5, 1867.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

It was announced from the Chair that the President had appointed the following Members of Council to be Vice-Presidents:—

The Treasurer,
Dr. Carpenter,
Mr. Gassiot.

In accordance with the notice given at the last Meeting, the question of Col. Le Couteur's readmission into the Society was put to the ballot, and, the ballot having been taken, Col. Le Couteur was declared to be readmitted.

The following communications were read:—

I. "On some Alterations in the Composition of Carbonate-of-Lime Waters, depending on the influence of Vegetation, Animal Life, and Season." By Robert Warington, F.R.S., F.C.S. Received October 19, 1867.

In carrying out through a series of years the principles of the aquarium for sustaining animal life in a confined and limited portion of water through the medium of growing vegetation *, I had observed that, during the summer months of the year, a considerable deposit made its appearance on the leaves of the plants and the glass front of the containing vessel, which was found to consist of carbonate of lime in a crystalline condition. This deposit formed a nidus for the growth of confervoid vegetation, which, at certain seasons of the year, increased very rapidly. These observations were alluded to at one of the Friday-evening meetings of the Royal Institution, March 27, 1857, when portions of the deposit were exhibited, and its composition demonstrated by experiment.

The formation of this deposit was then explained as arising from the fact that, as the summer season advances, and we have a longer continuance and also a greater intensity of the light of the sun, the absorption and consequent decomposition of carbonic acid by the plants is carried to a much greater extent, while the quantity of carbonic acid produced by the fish remains unchanged. The solvent of the carbonate of lime contained in the water being thus withdrawn, a deposit slowly takes place, incrusting the sides of the tank, particularly towards the light, where the confervoid growth, consequent upon it, accumulates in large quantities.

In continuing these observations, my attention was particularly arrested by the steady increase of deposition, attendant upon the renewed activity of the leaves, during the spring; and this determined me to ascertain by experiment the quantity of carbonate of lime existent in the water at fixed

* Quarterly Journal of the Chemical Society, vol. iii. p. 52.

intervals during a long period of time. And inasmuch as the degrees of hardness, indicated by the measures of Clark's soap-test, presented a very ready, accurate, and simple means of arriving at this result, that mode of estimation was adopted, care being taken to displace any uncombined carbonic acid by agitating the sample with atmospheric air prior to the addition of the test, as directed by Dr. Clark, the indications or degrees thus obtained representing the quantity of lime-salts contained in an imperial gallon of the sample (70·000 grains of distilled water) in terms of carbonate of lime.

In order that the nature of the experiment may be more clearly understood, it will perhaps be better for me, before stating the results thus obtained, to describe briefly the construction and arrangement of the aquarium, its position, and its contents. The tank consisted of a rectangular zinc framing, twenty inches long by thirteen broad, and twenty-one in depth, having slate cemented into it at the bottom and sides, and being glazed at the back and front. It was filled with water to the height of twelve inches, or a volume equal to ten gallons, and on the slate sides were cemented, at the water-line, ledges of rockwork composed of sandstone and tufaceous limestone from Matlock, on which were planted a few ferns, chiefly Trichomanes, for ornament. The bottom of the tank was covered, for about two inches, with a mixture of sandy loam and gravel, into which several plants of the *Vallisneria spiralis*, the vegetable member of the arrangement, were inserted. Some large fragments of rough rockwork, principally limestone, were also placed upright on the bottom to break up the stiff outline of the square framing, and give a pleasing effect to the eye. The animal branch of the circle consisted of four small crucian carp with a gold carp. Several freshwater mollusks, principally *Planorbis corneus* and *Limneus palustris*, were also introduced to act as scavengers and consume the decaying vegetation. The tank was loosely covered with a plate of glass, so as to allow of a free admission of the external air, and at the same time keep out a great deal of the soot and dust of the London atmosphere and impede the too rapid evaporation of the water. As the Trichomanes were stated to delight in shade, a thin muslin blind was placed over the covering glass.

The aquarium was located in a window-way having an eastern aspect, but, being surrounded within a few yards by the high walls of adjoining houses, the direct rays of the sun only reached it for about three hours in the morning during the months of June and July. It was established in January 1851, and has not since been disturbed, except by occasional supplies of distilled or rain-water, to replace the loss in volume arising from evaporation. It had been my custom to weed out the excessive growth of the Vallisneria during the summer, and also to remove some of the flaky deposit of calcareous matter from the surface of the glass nearest the light; but as I considered that such disturbances might interfere with the course of the investigation, these operations were discontinued.

The results that have been obtained from this investigation during the years 1861 and 1862 are as follows :—

1861.	March 13	.... 26°·2	degrees of hardness, or grains of lime-salts, per imperial gallon, in terms of carbonate of lime.	
	May 1	.... 19·5	„	„
	July 3	.... 12·5	„	„
	August 1	.... 13·6	„	„
	Sept. 17	.... 15·0	„	„
	Oct. 8	.... 15·5	„	„
	Nov. 12	.... 18·0	„	„
	Dec. 9	.... 20·5	„	„
1862.	Jan. 8	.... 23·5	„	„
	Feb. 8	.... 25·0	„	„
	March 3	.... 23·0	„	„
	April 3	.... 21·0	„	„
	May 2	.... 19·0	„	„
	June 4	.... 16·5	„	„
	July 4	.... 14·0	„	„
	August 5	.... 12·0	„	„
	Sept. 2	.... 12·5	„	„

The amount of calcareous matter dissolved will be seen to have steadily decreased during the spring and summer months, from its maximum in March 1861 and February 1862 to its minimum in July 1861 and August 1862, and then to have increased as steadily during the autumn and winter months.

Part of this hardness, however, unquestionably arose from the presence in the water of other salts of lime besides the carbonate. To determine how much was the next point for investigation. Portions of the water were taken on several occasions and boiled for a considerable time, filtered, and the volume restored to its original bulk with distilled water. On examining these portions with the soap-test, it was found that the hardness was lowered to 5·6 degrees, equivalent to 5·6 grains of carbonate of lime. But inasmuch as carbonate of lime is soluble in water to the extent of 2·4 grains in the imperial gallon*, this will be reduced to 3·2 grains, which amount will therefore have to be deducted from each of the above results, in order to arrive at the true quantity of carbonate present in solution.

The maximum and minimum results will then stand thus :—

		CaO, CO_2 in the imperial gallon.			CaO, CO_2 in the imperial gallon.
1861.	Maximum	23·0	1862.	Maximum	21·8
	Minimum	9·3		Minimum	8·8

* Chemical Report on the Supply of Water to the Metropolis, June 17, 1851, by Messrs. Graham, Miller, and Hofmann; and Quarterly Journal of the Chemical Society, vol. iv. p. 381.

The data thus obtained will help to elucidate several very important and interesting phenomena in respect to all the three elements of the arrangement—the water, the fish, and the vegetation.

1. *The Water.*

The importance of growing submerged vegetation in maintaining waters, rich in carbonate of lime, in a meliorated state by diminishing their hardness has been clearly demonstrated by the foregoing data; and how necessary, therefore, it is that this association should be kept in view whenever a soft and healthful water is required for domestic purposes. Unfortunately this appears hitherto not to have been well understood, or at all events has been little attended to, since the very agent which has been provided naturally for effecting these beneficial results has been most commonly regarded as an evil, and studiously eradicated in all directions. These data will also explain the cause of the rapid growth of vegetation in well-waters rich in carbonic acid, when pumped into tanks or reservoirs and exposed to the full light of day. The plant-germs, naturally contained in the water or absorbed from the atmosphere, being supplied with an abundance of appropriate nourishment, rapidly vegetate, and the containing vessels, particularly during the summer months, soon become thickly coated with a dense confervoid growth.

It will also follow that all fish, as generators of carbonic acid, should be excluded from waters flowing over carbonate-of-lime strata, and intended for the supply of towns &c., as tending to increase their hardness. Of course the absence of calcareous matter would prevent such an effect taking place—a fact borne out by the well-known softness of springs and rivers flowing out of or over granite or sandstone rocks, even when thickly inhabited by the scaly tribe.

2. *The Fish.*

It is well known that water has the property of absorbing air from the surrounding atmosphere, and holding it in solution to the extent of from one-fortieth to one-thirtieth of its volume, not, however, without somewhat changing the proportion of its constituents; for when the absorbed air is abstracted from water it usually contains about thirty-two per cent. of oxygen gas, instead of twenty-one. This oxygen is converted by the respiration of the fish into carbonic acid, which is held dissolved by a still stronger affinity, the water being capable of retaining as much as its own volume of this gas in solution at the ordinary temperature and pressure of the atmosphere.

In the above-described arrangement the carbonic acid thus produced is absorbed by the submerged vegetation under the influence of the sun's light; the carbon is appropriated for its growth, while the oxygen is again liberated and held in solution by the water, provided the evolution is not too rapid, an effect produced by too great an exposure to the sun's light. When this is the case, much of the oxygen necessarily escapes into the air

in a gaseous state and is lost. During the winter season, however, when the active functions of vegetation are to a great extent dormant, from the diminished quantity and intensity of the sun's light, the amount of carbonic acid produced by the respiration of the fish is greater than the plants are capable of consuming, and the excess must necessarily accumulate in the water. Were the production of carbonic acid confined to a short period, the water would doubtless right itself after a time, the poisonous gas passing away and fresh atmospheric air being absorbed. As, however, the production of carbonic acid is constant, this ameliorating action can have little effect; the water must remain always highly charged with carbonic acid. Here, then, its solvent action on the carbonate of lime, present in the rockwork and gravel, comes into play, and the hardness of the water is gradually increased in proportion as the light diminishes. Now, supposing for an instant that no carbonate of lime had been present in the arrangement, the question arises, what must then have ensued? The fish would have continued to respire, and would produce carbonic acid as before, which, remaining in a free state dissolved in the water, would unquestionably have had a most detrimental effect upon their health. Every one must have noticed the manner in which the golden carp confined in a globe of water, in which there is no growing vegetation to decompose the carbonic acid generated, or no limestone to combine with it, rise to the surface and continually gulp in the air required for their vital functions. Nothing whatever of this kind has ever been noticed in the aquarium under consideration, although the quantity of carbonic acid dissolved in the water has been at times very large.

From the experiments of Bischof*, we glean that the carbonic acid contained in a saturated aqueous solution is entirely displaced by a current of atmospheric air passed through it for five minutes; and also † that, by the same means, a solution of carbonate of lime, in water previously saturated with carbonic acid, will have all the excess of gas displaced in fifteen minutes, leaving the water with *bicarbonate of lime* in solution. It is in this form of combination that MM. Peligot ‡ and Poggiale § consider the carbonate of lime to exist in the water of the Seine, and M. Bineau ‖ in that of the Rhone, in which rivers they state there is no free carbonic acid. In the present investigation we shall therefore assume it to be in the same state of combination. We have, in the series of experiments detailed above, an increase in the quantity of carbonate of lime held in solution, amounting to 14·2 grains in the imperial gallon, which would require nearly $6\frac{1}{4}$ grains of carbonic acid gas to dissolve it. Besides this there is also the quantity already present in the water at its minimum, which amounts to nearly four grains more, or in all to about ten grains, equal to nearly

* Bischof's 'Elements of Chemical Geology,' Cavendish Society's edition, vol. iii. p. 5.
† *Op. cit.* vol. iii. p. 7.
‡ Comptes Rendus, vol. xl. p. 1121, and Bischof's 'Elements,' vol. iii. p. 117.
§ Journal de Pharmacie, vol. xxviii. p. 321, and *op. cit.* vol. iii. p. 118.
‖ Comptes Rendus, vol. xli. p. 511, and *op. cit.* vol. iii. p. 118.

215 cubic inches of that gas in the ten gallons of water, or more than $\frac{1}{13}$th its volume. The exact numbers will be seen in the following Table:—

		CaO, CO_2 in the gallon.		CO_2.	
1861	Maximum	23·0	grains, requiring	10·120	grains to form $CaO,2CO_2$.
	Minimum	9·3	,, ,,	4·092	,, ,, ,,
1862	Maximum	21·8	,, ,,	9·592	,, ,, ,,
	Minimum	8·8	,, ,,	3·872	,, ,, ,,

Carbonic acid required to dissolve the increase 6·248 grains = 13·269 cub. in.
,, ,, minimum 3·872 ,, 8·228 ,,
10·120 ,, 21·497 ,,

Yet, although the quantity of poisonous gas had been thus increased, we find no deleterious action on the health of the fish, no disturbance in the ordinary respiration, no gulping at the surface of the water for fresh air. It is quite evident, therefore, that the carbonic acid, by entering into combination with carbonate of lime, however weak that combination may be, is thereby rendered perfectly innocuous, and a wonderful provision is thus afforded for preventing this poisonous agent from becoming fatal to animal life.

We turn now to the next member of our arrangement.

3. *The Vegetation.*

It will be seen from the foregoing numerical results that the maximum quantity of dissolved carbonate of lime, and consequently of carbonic acid, is found just before the period of the reviving energies of the plant's growth, namely, the spring time of the year, when the days are lengthening and the sun's light is continually increasing in strength; the minimum quantity when this growth has attained its greatest exuberance, namely, when the summer months are past and the light is beginning to decrease in its intensity and the days to shorten. So exactly, indeed, are the energies of the plants regulated by the amount of light to which they are exposed, that a constant arrangement, such as that here described, affords an excellent indication of the variation of the seasons in different years, or might even be made a rough measure of the total amount of light from month to month.

But while the demand for carbonic acid on the part of the plant varies in this manner with the seasons, the amount of that gas produced by the respiration of the fish is very nearly the same all through the year. Whence, then, does the plant obtain that additional quantity of food which its stimulated energies require during the spring and early summer months, and which its rapid and luxurious growth show to be readily supplied? After what has been stated, I think the source must be apparent to every one—it is from the carbonic acid which has been gradually accumulated, and rendered innocuous to animal life from its being held in combination with carbonate of lime, in so marvellous a manner during the winter

months. Stored up, yet held in feeble combination, a combination so weak that the vital forces of the fresh-growing vegetation can easily overcome it, and resolve once more into carbonate of lime, carbon, and oxygen the bicarbonate of lime contained in the water *.

Thus beautifully are the necessary irregularities in the purifying action of the plant compensated and provided for, that the balance of existence between the animal and vegetable organisms be not disturbed or overthrown, and thus additional proof is furnished, if such were needed, of the wisdom of that creative power that has ordered all things to work together for good, and by endowing certain bodies with such seemingly minute and insignificant affinities, maintains the glorious harmony of the whole.

II. "Results of Observations of Atmospheric Electricity at Kew Observatory, and at Windsor, Nova Scotia." By JOSEPH D. EVERETT, D.C.L., F.R.S.E., Assistant to the Professor of Mathematics in the University of Glasgow. Communicated by Sir WILLIAM THOMSON. Received October 14, 1867.

(Abstract.)

The paper commences with an account of the concluding observations taken by the author at Windsor, N.S., of which the previous portion has already been published in the 'Proceedings,' vols. xii. & xiv.

It then goes on to describe the self-recording apparatus employed at Kew Observatory for the observation of atmospheric electricity, and the method of procedure employed in measuring and reducing the curves thus obtained, this portion of the work having been performed in the Physical Laboratory of the University of Glasgow.

Tables are given showing the mean hourly values of the electrical potential for each month, and the mean monthly values are hence derived. These values for Kew are compared with the corresponding values for Windsor, N.S., and remarkable differences are shown to exist between the curves, both diurnal and annual, for the two places.

The hourly means at Kew for the mean of the year are represented by the following numbers :—

23^h	0^h	1^h	2^h	3^h	4^h	5^h	6^h	7^h
1·91	1·96	1·92	1·93	1·95	2·08	2·29	2·58	2·86
8^h	9^h	10^h	11^h	12^h	13^h	14^h	15^h	16^h
2·96	2·93	2·74	2·42	2·12	1·86	1·68	1·58	1·54
17^h	18^h	19^h	20^h	21^h	22^h			
1·52	1·64	1·96	2·26	2·28	2·13.			

These numbers indicate a principal maximum between 8^h and 9^h, and a

* The rapid growth of submerged vegetation in rivers and waters containing a considerable amount of carbonate of lime must have been observed by all interested in the subject, in some cases obliging the cleansing of such streams three or four times during the year.

secondary maximum between 20^h and 21^h. At Windsor, on the other hand, the mean potential about 9^h was in every month, without exception, less than at the other principal times of observation, viz. about 21^h and 14^h.

The following Table shows the ratio of the mean monthly to the mean annual potential for the whole series of observations at both places:—

Kew.

June 1862	·770	June 1863	·681
July „	·773	July „	·643
Aug. „	·836	Aug. „	·685
Sept. „	·845	Sept. „	·854
Oct. „	·981	Oct. „	1·000
Nov. „	1·600	Nov. „	1·390
Dec. „	1·188	Dec. „	1·460
Jan. 1863	1·033	Jan. 1864	1·226
Feb. „	1·333	Feb. „	1·263
March „	1·160	March „	1·375
April „	·920	April „	·831
May „	·672	May „	·549

Windsor, N.S.

Oct. 1862	·832	Oct. 1863	1·033
Nov. „	·766	Nov. „	·949
Dec. „	1·010	Dec. „	1·110
Jan. 1863	1·057	Jan. 1864	1·125
Feb. „	1·432	Feb. „	?
March „	1·396	March „	1·416
April „	1·023	April „	1·026
May „	·796	May „	·985
June „	·720	June „	·799
July „	·755	July „	·885
Aug. „	·952	Aug. „	(·862)
Sept. „	·985		

The last step in the reductions consisted in expressing the variations, both diurnal and annual, at Kew, and the annual variations at Windsor, by the first two terms of an harmonic series.

In the case of the diurnal variations at Kew, the amplitudes of the two terms were nearly equal, but the epoch was much more uniform in its values (whether in comparing one year with the other or in comparing one month with another in the same year) for the second term than for the first.

In the case of the annual variations, the amplitude of the second term at Kew was almost inappreciable, while at Windsor it was greater than that of the first term.

III. "On the Orders and Genera of Quadratic Forms containing more than three Indeterminates." By H. J. STEPHEN SMITH, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.—Second Notice. Received October 30, 1867.

The principles upon which quadratic forms are distributed into orders and genera have been indicated in a former notice (Proceedings of the Royal Society, vol. xiii. p. 199). Some further results relating to the same subject are contained in the present communication.

I. *The Definition of the Orders and Genera.*

Retaining, with some exceptions to which we shall now direct attention, the notation and nomenclature of the former notice, we represent by f_1 a primitive quadratic form containing n indeterminates, of which the matrix is $\| \overset{n\times n}{A_{i,j}} \|$; by $f_2, f_3, \ldots f_{n-1}$, the fundamental concomitants of f_1, of which the last is the contravariant. The matrices of these concomitants are the matrices derived from the matrix of f_1, so that the first coefficients of $f_2, f_3, \ldots f_{n-1}$, are respectively the determinants $|\overset{2\times 2}{A_{i,j}}|$, $|\cdot \overset{3\times 3}{A_{i,j}}|, \ldots |\overset{n-1\times n-1}{A_{i,j}}|$, taken with their proper signs. The discriminant of f_1, i. e. the determinant of the matrix $|\overset{n\times n}{A_{i,j}}|$, which is supposed to be different from zero, and which is to be taken with its proper sign, is represented by ∇_n. The greatest common divisors of the minors of the orders $n-1$, $n-2$, ... 2, 1 in the same matrix are denoted by $\nabla_{n-1}, \nabla_{n-2}, \ldots \nabla_2, \nabla_1$, of which the last is a unit; we shall presently attribute signs to each of these greatest common divisors. The quotients

$$\frac{\nabla_n}{\nabla_{n-1}} \div \frac{\nabla_{n-1}}{\nabla_{n-2}}, \quad \frac{\nabla_{n-1}}{\nabla_{n-2}} \div \frac{\nabla_{n-2}}{\nabla_{n-3}}, \quad \ldots \quad \frac{\nabla_2}{\nabla_1} \div \frac{\nabla_1}{1},$$

which are always integral, we represent by $I_{n-1}, I_{n-2}, \ldots I_1$; so that

$$I_k = \frac{\nabla_{k+1}}{\nabla_k} \div \frac{\nabla_k}{\nabla_{k-1}}.$$

The numbers $I_1, I_2, \ldots I_{n-1}$ are the first, second, ... last invariants of the form f_1, and remain unchanged when f_1 is transformed by any substitution of which the determinant is unity and the coefficients integral numbers. Forms which have the same invariants have of course the same discriminant; but (if the number of indeterminates is greater than two) forms which have the same discriminant do not necessarily have the same invariants; for example, the quaternary forms

$$x_1^2 + x_2^2 + 2x_3^2 + 6x_4^2, \quad x_1^2 + x_2^2 + x_3^2 + 12x_4^2$$

have the same discrminant 12, but their invariants I_1, I_2, I_3 are respectively 1, 2, 3, and 1, 1, 12. As forms which have the same discriminant, but different invariants, do not necessarily have any close relation to one another, we shall not employ the discriminant in the classification of qua-

dratic forms; but we shall regard the infinite number of forms, which have the same invariants, as corresponding, in the general theory, to the infinite number of forms which have the same determinant, in the theory of binary quadratic forms.

If the index of inertia of the form f is k, i. e. if f_1 can be transformed by a substitution of which the coefficients are real into a sum of k positive and $n-k$ negative squares, we attribute to the invariant I_k the sign $-$, and to every other invariant the sign $+$. Thus the numbers $\nabla_1, \nabla_2, \ldots \nabla_k$ are all positive; $\nabla_{k+1}, \nabla_{k+2}, \ldots \nabla_n$ are alternately negative and positive, so that the discriminant ∇_n is of the same sign as $(-1)^{n-k}$, as it ought to be. This convention with respect to the signs of the invariants will enable us to comprehend in the same formulæ the theory of the generic characters of forms of any index of inertia. We shall, however, suppose that the index of inertia is at least 1, *i. e.* we shall exclude negative definite forms. The invariants of a positive definite form are all positive; and the index of inertia of any indefinite form, of which the invariants are given, is always indicated by the ordinal index of its negative invariant. We shall represent by D the product $-I_1 \times -I_3 \times -I_5 \times \ldots$, the last factor being $-I_{n-1}$, or $-I_{n-2}$, according as n is even or uneven.

If $\theta^i = \dfrac{1}{\nabla_i} f^i$, the forms $\theta_1, \theta_2, \theta_3, \ldots \theta_{n-1}$ are the primitive concomitants, and the last the primitive contravariant, of f_1 or θ_1; each one of them is either uneven, *i. e.* properly primitive, or even, *i. e.* improperly primitive. Two forms, which have the same invariants, are said to belong to the same order when the corresponding primitive concomitants of the two forms are alike uneven or alike even. When the invariants are all uneven, and the number of the indeterminates is also uneven, there is but one order, none of the primitive concomitants being in this case even. Again, when the invariants are all uneven, and the number n of the indeterminates is even, there is either one order or two, according as $D \equiv -1$, or $\equiv +1$, mod 4; for in both cases there is an order in which all the primitive concomitants are uneven, and in the latter case, besides this uneven order, there is an even order, in which these forms are alternately even and uneven, the two extreme forms θ_1 and θ_{n-1} being even. In the general case, when the invariants have any values even or uneven, if I_i is even, θ_i cannot be even; again, if I_i is one of a sequence of an even number of uneven invariants, preceded and followed by even invariants, θ_i cannot be even. But if there be a sequence of an uneven number of uneven invariants $I_i, I_{i+1}, \ldots I_{i+2j}$, preceded and followed by even invariants, the sequence of primitive concomitants $\theta_i, \theta_{i+1}, \ldots \theta_{i+2j}$ are all uneven if θ_i is uneven, and are alternately even and uneven if θ_i is even; a *sequence* of forms or invariants may consist of a single form or invariant. We attribute the value 0 to the symbols I_0 and I_n, the value 1 to the symbols θ_0 and θ_n; thus the invariant I_1

is always to be regarded as preceded by an even invariant, and I_{n-1} as followed by an even invariant; similarly the forms θ_1 and θ_{n-1} are to be regarded as respectively preceded and followed by uneven forms. Two even forms cannot be consecutive in the series $\theta_1 \, . \, . \, \theta_{n-1}$.

The preceding observations enable us to assign all the orders which may exist for any given invariants; if the series of invariants $I_0, I_1, \ldots I_{n-1}, I_n$ present ω different sequences each consisting of an uneven number of uneven invariants, preceded and followed by even invariants, there are 2^{ω} assignable orders. These orders, in general, all exist; there are, however, the following exceptions to this statement:—

(1) If, the number of indeterminates being even and equal to 2ν, D is uneven, there is an assignable order in which the concomitants $\theta_1, \theta_2, \ldots \theta_{n-1}$ are alternately even and uneven. But, as has been already said, this order does not exist if $D \equiv -1$, mod 4; and, if the invariants are all squares, it does not exist, even if $D \equiv 1$, mod 4, unless the equation

$$(-1)^{\frac{\nu(\nu-1)}{2}} = (-1)^{\frac{k(k-1)}{2}}$$

(in which k is the index of inertia) is also satisfied.

(2) If, the number of indeterminates being uneven and equal to $2\nu+1$, D is uneven, and $I_{2\nu}$ even, there is again an assignable order in which the concomitants $\theta_1, \ldots \theta_{2\nu}$ are alternately even and uneven. But, when $I_{2\nu}$ is the double of a square, and the other invariants are squares, this order does not exist unless the equation $(-1)^{\frac{1}{8}(n^2-1)} = (-1)^{\frac{1}{2}k(n-k)}$ (in which k is still the index of inertia) is satisfied.

The reciprocal case (that obtained by changing I_s and θ_s into I_{n-s}, and θ_{n-s}, for every value of s from 0 to n) presents a similar exception, which it is not necessary to enunciate separately.

The generic characters of the form θ_1, or, more properly of the system of concomitant forms $\theta_1, \theta_2, \ldots \theta_{n-1}$, so far as they depend on uneven primes dividing the invariants, have been already defined in the former notice, and the definition need not be repeated here. These characters we shall term the *principal* generic characters of the system. When the invariants and primitive concomitants are all uneven, the principal characters are the only generic characters, with the exception of a certain character, which we shall define hereafter, and of which the value is not independent of the principal characters. In other cases, the forms of the concomitant system may acquire generic characters with respect to 4 or 8: these we shall term *supplementary*. What supplementary characters exist in any given case may always be ascertained by applying the following rules. In their enunciation we represent by I'_i the greatest uneven divisor of I_i taken with the same sign as I_i, by μ_i the exponent of the highest power of 2 contained in I_i, increased by 1 if one of the two forms $\theta_{i-1}, \theta_{i+1}$ is even, and by 2 if both those forms are even; we suppose $0 < i < n$.

I. If $\mu_i \overline{\overline{>}} 2$, θ_i has the character $(-1)^{\frac{\theta_i-1}{2}}$

II. If $\mu_i \overline{\overline{>}} 3$, θ_i, in addition to the character $(-1)^{\frac{\theta_i-1}{2}}$, has also the character $(-1)^{\frac{\theta_i^2-1}{8}}$.

III. If $\mu_i = 1$, and also $\mu_{i-1} \overline{\overline{>}} 2$, $\mu_{i+1} \overline{\overline{>}} 2$, θ_i (which, as well as θ_{i-1} and θ_{i+1}, is necessarily uneven) has the character

$$(-1)^{\frac{\theta_i^2-1}{8}} \text{ or } (-1)^{\frac{\theta_i-1}{2}+\frac{\theta_i^2-1}{8}}$$

according as

$$(-1)^{\frac{1}{2}(\theta_{i-1}-1)+\frac{1}{2}(\theta_{i+1}-1)} = (-1)^{\frac{1}{2}(I'_i+1)}, \text{ or } = (-1)^{\frac{1}{2}(I'_i-1)}.$$

It will be observed that (by I) the forms θ_{i-1} and θ_{i+1} have the characters $(-1)^{\frac{\theta_{i-1}-1}{2}}$ and $(-1)^{\frac{\theta_{i+1}-1}{2}}$.

IV. If $\mu_i = 0$, and also $\mu_{i-1} \overline{\overline{>}} 2$, $\mu_{i+1} \overline{\overline{>}} 2$, θ_i, if uneven, has the character $(-1)^{\frac{\theta_i-1}{2}}$, or no character at all, according as

$$(-1)^{\frac{1}{2}(\theta_{i-1}-1)+\frac{1}{2}(\theta_{i+1}-1)} = (-1)^{\frac{1}{2}(I_i-1)} \text{ or } (-1)^{\frac{1}{2}(I_i+1)}.$$

No even concomitant has any supplementary character. But if θ_i is an even concomitant, the uneven forms preceding and following it have, by I, the characters

$$(-1)^{\frac{\theta_{i-1}-1}{2}}, \text{ and } (-1)^{\frac{\theta_{i+1}-1}{2}}.$$

These characters are not independent but are connected by the equation

$$(-1)^{\frac{1}{2}(\theta_{i-1}-1)+\frac{1}{2}(\theta_{i+1}-1)} = (-1)^{\frac{1}{2}(I_i+1)}.$$

Thus if $I_i, I_{i+1}, \ldots I_{i+2j}$ is a sequence of an uneven number of uneven invariants preceded and followed by even invariants, and corresponding to a sequence of alternately even and uneven concomitants $\theta_i, \theta_{i+1}, \ldots \theta_{i+2j}$, the character, mod 4, of every uneven form of this sequence, and of the next following form θ_{i+2j+1}, is determined by the character of the form θ_{i-1}. We have, in fact, if $s = 1, 2, \ldots j+1$,

$$(-1)^{\frac{1}{2}(\theta_{i+2s-1}-1)} = (-1)^s \times (-1)^{\frac{1}{2}[I_i \times I_{i+2} \times \ldots I_{i+2s-2}-1]} \times (-1)^{\frac{1}{2}(\theta_{i-1}-1)}.$$

Besides these supplementary characters, which, no less than the principal characters, are attributable to individual forms of the concomitant system, there exist, or may exist, other characters, which we shall term simultaneous, attributable to certain sequences of those forms considered conjointly. Such a character is attributable to every sequence of uneven forms, of which none possesses any supplementary character but which are immediately preceded and followed by forms having such characters. The fol-

lowing definition is requisite in order to explain the nature of these simultaneous characters.

If $\left\| \overset{n-1\times n}{a_{i,j}} \right\|$ is a matrix of the type $n-1\times n$, of which the determinants are not all zero, and if m_k represent the value acquired by θ_k, when we attribute to the indeterminates of that form the values of the determinants

$$\left\| \overset{k\times n}{a_{i,j}} \right\|$$

$$i=1, 2, \ldots k; \; j=1, 2, \ldots n,$$

taken in the same order in which the determinants of any k horizontal rows of the matrix $\left\| \overset{n\times n}{A_{i,j}} \right\|$ are taken in forming the matrix of θ_k, the numbers $m_1, m_2, \ldots m_{n-1}$ are said to be simultaneously represented by the forms $\theta_1, \theta_2, \ldots \theta_{n-1}$.

Let $\theta_{i+1}, \ldots \theta_{i+i'}$ be a sequence of i' uneven concomitants, $\mu_{i+1}, \mu_{i+2}, \ldots \mu_{i+i'}$ being 0, or 1, but μ_i and $\mu_{i+i'+1}$ being greater than 1; the uneven numbers simultaneously represented by $\theta_{i+1}, \theta_{i+2}, \ldots \theta_{i+i'}$ are all such as to render the unit

$$(-1)^{\sum_{s=i}^{s=i+i'} \frac{1}{4}(\theta_s-1)(\theta_{s+1}-1)} \times (-1)^{\sum_{s=i+1}^{s=i+i'} \frac{1}{4}(I'_s+1)(\theta_s-1)} \times (-1)^{\sum_{s=i+1}^{s=i+i'} \mu_s \frac{\theta_s^2-1}{8}}$$

(which we shall symbolize by $\psi(i, i')$), equal to $+1$, or else are all such as to render that unit equal to -1. We therefore attribute to the sequence of forms $\theta_{i+1} \ldots \theta_{i+i'}$, the simultaneous character $\psi(i, i')=+1$, or $\psi(i, i')=-1$, according as the former or latter of those equations is satisfied. If $i'=1$, the sequence consists of but one form, so that the character $\psi(i, i')$ ceases to be a simultaneous character; in fact, if $\mu_{i+1}=1$, it coincides with the supplementary character attributable to θ_{i+1} by III.; if $\mu_{i+1}=0$, it either becomes nugatory (*i. e.* identically equal to $+1$, irrespective of the value of m_{i+1}), or it coincides with the supplementary character of θ_{i+1}, according as that form (by IV.) has not or has a supplementary character.

The complex of all the particular characters (principal, supplementary, and simultaneous) constitutes the complete character of the system of concomitants $\theta_1, \theta_2, \ldots \theta_{n-1}$. Not every complete generic character, assignable *à priori*, corresponds to actually existing forms, but only such characters as satisfy a certain condition of possibility. This condition is expressed by the equation

$$\psi(0, n-1)\times \prod_{s=1}^{s=n-1} \left(\frac{\theta_s}{I'_s}\right)=+1, \quad \ldots\ldots \quad \text{(A)}$$

in which, if θ_s is an even form, we understand by the symbol $\left(\frac{\theta_s}{I_s}\right)$ the quadratic character with respect to I_s of the *half* of any number, prime to I_s, which is represented by θ_s. The unit $\psi(0, n-1)$ is formed in the same

way as the unit $\psi(i, i')$: we may omit, however, from the exponent of -1 in its expression every term into which an even form enters; if, for example, θ_s is an even form, that exponent contains the terms

$$\left(\frac{\theta_{s-1}-1}{2}+\frac{\theta_{s+1}-1}{2}+\frac{\mathrm{I}_s+1}{2}\right)\times\frac{\theta_s-1}{2}+\mu_s\frac{\theta_s^2-1}{8},$$

and no other term into which θ_s enters; but $\mu_s=0$, and the coefficient of $\frac{1}{2}(\theta_s-1)$ is even; so that θ_s disappears from the expression of the unit $\psi(0, n-1)$. It will thus be seen that the equation (A) involves only generic characters (principal, supplementary, or simultaneous) of the concomitant system: that equation therefore expresses a relation which the complete character must satisfy.

In using these formulæ, we must attend to the significations which we have assigned to the symbols I_0, I_n, θ_0, and θ_n. Thus

$$(-1)^{\frac{\theta_0-1}{2}}=1=(-1)^{\frac{\theta_0^2-1}{8}},\ \mu_0>3, \text{ etc.}$$

We shall conclude this part of our subject with the two theorems:—

(i) Every genus, of which the character satisfies the condition of possibility, actually exists.

(ii) Two forms, of the same invariants, of the same order, and of the same genus are transformable, each into the other, by rational linear substitutions of which the determinants are units, and in which the denominators of the coefficients are prime to any given number.

The first of these theorems shows that the condition of possibility is sufficient as well as necessary; the second establishes the completeness of the enumeration of ordinal and generic characters.

II. *Determination of the Weight of a given Genus of Definite Forms.*

It has been shown by Gauss, in the digression on ternary forms in the fifth section of the 'Disquisitiones Arithmeticæ,' that the solution of the problems "to obtain all the representations of a given binary form, or of a given number, by a given ternary form," depends on the solution of the problem "to determine whether two given ternary forms are equivalent, and, if they are, to obtain all the transformations of either of them into the other." Similarly the solution of the problem "to obtain all the representations of a given quadratic form of i indeterminates ($i=1, 2, \ldots n-1$) by a given form of n indeterminates" depends on the solution of the problem of equivalence for quadratic forms of n indeterminates. The following proposition is here of primary importance:—

"If the form ϕ_1, of $n-1$ indeterminates and of the invariants $\mathrm{I}_1, \mathrm{I}_2, \ldots \mathrm{I}_{n-3}, \mathrm{MI}_{n-2}$, is capable of primitive representation by the form θ_1, of n indeterminates, and of the invariants $\mathrm{I}_1, \mathrm{I}_2, \ldots \mathrm{I}_{n-3}, \mathrm{I}_{n-2}, \mathrm{I}_{n-1}$, then $-\mathrm{I}_{n-1}\times\phi_{n-2}$ (where ϕ_{n-2} is the primitive contravariant of ϕ_1) is a quadratic residue of M."

The converse is true, subject to certain limitations:—

"If M is prime to I_{n-1}, and not negative except when I_{n-1} is negative, and if $-I_{n-1}\times\phi_{n-2}$ is a quadratic residue of M, ϕ_1 is capable of primitive representation by f_1."

"If, in addition, M is prime to I_{n-2}, there is always either one or two genera of forms of the invariants $(I_1, I_2, \ldots M\, I_{n-2})$ capable of primitive representation by forms of a given genus of the invariants $(I_1, I_2, \ldots I_{n-2}, I_{n-1})$; and if there are two genera capable of such representation, they are of different orders."

These theorems are especially useful in the theory of definite forms, to which, for the remainder of this paper, we shall confine our attention. In the case of such forms we understand by the weight of a form the reciprocal of the number of its positive automorphics, by the weight of a class the weight of any form representing the class; the weight of a genus, or order, is the sum of the weights of the classes contained in the genus or order; the weight of a representation of a number by a form is the weight of the representing form; the weight of a representation of a form by a form is the product of the weights of the representing and represented forms.

Let Γ denote a system of forms, representatives of a given genus of the invariants $I_1, I_2, \ldots I_{n-1}$; let M be a number divisible by μ different uneven primes, none of which divide any of the invariants, and let M be uneven or unevenly even, according as the contravariants of the forms Γ are uneven or even; we then have the theorem—

"The sum of the weights of the representations of M by the contravariants of the forms Γ, is 2^{μ} times the weight of the single genus, or the two genera, of invariants $I_1, I_2, \ldots MI_{n-2}$, which admit of representation by the forms Γ."

The method which this theorem may serve to indicate supplies a solution of the problem "to determine the weight of a given genus of definite forms of n indeterminates, and of the invariants $I_1, I_2, \ldots I_{n-1} \ldots$" We shall represent the weight of the given genus by the formula

$$W=\zeta_{2\nu+1}\times \Pi\,.\,\chi(\delta)\times B_{2\nu+1}\times \prod_{s=1}^{s=2\nu} I_s^{\frac{1}{2}s(n-s)}$$

when n is uneven and equal to $2\nu+1$, and by the formula

$$W=\zeta_{2\nu}\times \Pi\,.\,\chi(\delta)\times B_{2\nu}\times \prod_{s=1}^{s=2\nu-1} I_s^{\frac{1}{2}s(n-s)}\times\frac{1}{\pi^{\nu}}\sum_{1}^{\infty}\left(\frac{D}{m}\right)\frac{1}{m^{\nu}}$$

when n is even and equal to 2ν; and we shall consider separately the factors of which these formulæ are composed.

(i) In the infinite series $\frac{1}{\pi^{\nu}}\sum_{1}^{\infty}\left(\frac{D}{m}\right)\frac{1}{m^{\nu}}$ (which enters into the expression of W only when the number of indeterminates is even) D still represents the

product $(-1)^{\nu} I_1 \times I_3 \times \ldots \times I_{2\nu-1}$, and the summation extends to all uneven values of m, which are prime to D, from 1 to ∞. The sum of this infinite series can in every case be obtained in a finite form by the methods employed by Dirichlet (in the 21st volume of Crelle's Journal) and by Cauchy (in the 17th volume of the Mémoires de l'Académie des Sciences, p. 679). As the result of the summation does not seem to have been given, we shall present it here in one of many various forms which it may assume. Let D_1 represent the quotient obtained by dividing D by its greatest square divisor; let q be any uneven prime dividing D, but not D_1, and let $V = \frac{1}{\pi^{\nu}} \sum_1^{\infty} \left(\frac{D_1}{m}\right) \frac{1}{m^{\nu}}$, the sign of summation extending to all values of m prime to $2D_1$; we then have the equation

$$\frac{1}{\pi^{\nu}} \sum_1^{\infty} \left(\frac{D}{m}\right) \frac{1}{m^{\nu}} = \Pi \left[1 - \left(\frac{D_1}{q}\right) \frac{1}{q^{\nu}}\right] \times V.$$

To obtain the value of V, let Δ represent the positive value of D_1, so that $\Delta = D_1$ when ν is even, and $\Delta = -D_1$ when ν is uneven. Also let

$$F_{2\sigma}(x) = \frac{x^{2\sigma+1}}{2\sigma+1} - \tfrac{1}{2} x^{2\sigma} + \frac{\Pi . 2\sigma}{\Pi . 2\sigma-1 . \Pi . 2} \beta_1 x^{2\sigma-1}$$
$$- \frac{\Pi . 2\sigma}{\Pi . 2\sigma-3 . \Pi . 4} \beta_3 x^{2\sigma-3} + \ldots + (-1)^{\sigma-1} \frac{\Pi . 2\sigma}{\Pi . 1 . \Pi . 2\sigma} \beta_{2\sigma-1} x,$$

$$F_{2\sigma-1}(x) = \frac{x^{2\sigma}}{2\sigma} - \tfrac{1}{2} x^{2\sigma-1} + \frac{\Pi . 2\sigma-1}{\Pi . 2\sigma-2 . \Pi . 2} \beta_1 x^{2\sigma-2}$$
$$- \frac{\Pi . 2\sigma-1}{\Pi . 2\sigma-4 . \Pi . 4} \beta_3 x^{2\sigma-4} + \ldots + (-1)^{\sigma} \frac{\Pi . 2\sigma-1}{\Pi . 2 . \Pi . 2\sigma-2} \beta_{2\sigma-3} x^2,$$

where $\beta_1, \beta_3, \ldots$ are the fractions of Bernoulli, so that $F^k(x)$ is the function which, when x is an integral number, is equivalent to the sum $\sum_{s=1}^{s=x-1} s^k$. Then, if $\epsilon = (-1)^{\frac{1}{2}(\nu+2)}$, or $(-1)^{\frac{1}{2}(\nu+1)}$, according as ν is even or uneven, the value of ϵV is

(1) when $D_1 \equiv 1$, mod 4,

$$\frac{2^{\nu-1}}{\Pi . \nu-1} \times \left[1 - \left(\frac{2}{\Delta}\right) \frac{1}{2^{\nu}}\right] \times \frac{1}{\sqrt{\Delta}} \times \sum_{s=1}^{s=\Delta} \left(\frac{s}{\Delta}\right) F_{\nu-1}\left(\frac{s}{\Delta}\right),$$

(2) in every other case,

$$\frac{2^{\nu-1}}{\Pi . \nu-1} \times \frac{1}{2\sqrt{\Delta}} \times \sum_{s=1}^{s=4\Delta} \left(\frac{D_1}{s}\right) F_{\nu-1}\left(\frac{s}{4\Delta}\right),$$

the summation $\sum_1^{\Delta}$ extending to every integral value of s inferior to Δ and prime to Δ, the summation $\sum_1^{4\Delta}$ extending to every integral value of s inferior to 4Δ, and prime to 4Δ. The formula (1) is inapplicable when $\Delta = D_1 = 1$;

but in this case ν is even, and the sum of the series $\frac{1}{\pi^\nu}\,\Sigma\,\frac{1}{m^\nu}$ is known.

(ii) The factor $\prod\limits_{s=1}^{s=n-1} I_s^{\frac{1}{2}(n-s)}$ requires no explanation; it is rational when n is uneven, and is a multiple of $\sqrt{\Delta}$ when n is even.

(iii) The factor B_n is determined by the equations

$$B_{2\nu}=\tfrac{1}{2}\beta_1\times\tfrac{1}{2}\beta_3\times\tfrac{1}{2}\beta_5\times\,..\,\tfrac{1}{2}\beta_{2\nu-3},$$

$$B_{2\nu+1}=\tfrac{1}{2}\beta_1\times\tfrac{1}{2}\beta_3\times\tfrac{1}{2}\beta_5\times\,..\,\tfrac{1}{2}\beta_{2\nu-1}\times\frac{1}{\Pi\,.\,\nu},$$

where β_1, β_3, ... are again the fractions of Bernoulli, so that $\beta_1=\frac{1}{6}$, $\beta_3=\frac{1}{30}$, etc.

(iv) The factors (i) and (ii) depend only on the invariants and on the number of the indeterminates, the factor (iii) only on the number of indeterminates. These factors are therefore the same for all genera of the invariants $I_1, I_2, .. I_{n-1}$. But the two remaining factors involve, or may involve, certain of the generic characters, and are therefore not always the same for all genera. In the factor $\Pi\,.\,\chi(\delta)$ the sign of multiplication extends to every uneven prime δ, dividing any one or more of the invariants I_1, I_2, ... I_{n-1}: it will suffice therefore to define the function $\chi(\delta)$, which depends on only one of those primes. Let i_1, i_2, ... be the indices of all the invariants which are divisible by δ; let these indices be arranged in order of magnitude, beginning with 0 and ending with n (because I_0 and I_n may be considered as divisible by δ). The positive differences $i_{s+1}-i_s$ we shall term *intervals.* By the *moiety* of any whole number a we understand $\frac{1}{2}\,a$ when a is even, $\frac{1}{2}\,(a-1)$ when a is uneven. Let κ_s be the moiety of the interval $i_{s+1}-i_s$; when that interval is even, let the barred symbol $\bar{\kappa}_s$ represent the product $(-1)^{\kappa_s} I_{1+i_s}\times I_{3+i_s}\times\ldots\times I_{-1+i_{s+1}}$; and let $\Gamma(\bar{\kappa}_s)=1+\left(\frac{\bar{\kappa}_s\times\theta_{i_s}\times\theta_{i_{s+1}}}{\delta}\right)\frac{1}{\delta^{\kappa_s}}$. Lastly, let $\Omega(h)$ represent the product $\prod\limits_{s=1}^{s=h}\left(1-\frac{1}{\delta^{2s}}\right)$; let σ be the moiety of $n-1$, and μ the number of the invariants $I_1, I_2, \ldots I_{n-1}$, which are divisible by δ. Then $\chi(\delta)$ is the integral function of $\frac{1}{\delta}$, defined by the equation

$$\chi(\delta)=\frac{1}{2^\mu}\times\frac{\Omega(\sigma)}{\Pi\,.\,\Omega(\kappa_s)}\times\Pi\,.\,\Gamma(\bar{\kappa}_s)$$

when n is uneven, and by the equation

$$\chi(\delta)=\frac{1}{2^\mu}\times\frac{\Omega(\sigma)}{\Pi\,.\,\Omega(\kappa_s)}\times\Pi\,.\,\Gamma(\bar{\kappa}_s)\times\left[1-\left(\frac{D}{\delta}\right)\frac{1}{\delta^{\frac{1}{2}n}}\right]$$

when n is even. If D is divisible by δ, the symbol $\left(\frac{D}{\delta}\right)$ is zero. In both formulæ the sign of multiplication Π extends to every value of κ_s or $\bar{\kappa}_s$; the value $+1$ is, as before, to be attributed to the symbols θ_0 and θ_n.

(v) Each factor $\chi(\delta)$ of the product $\Pi\,.\,\chi(\delta)$ thus depends on an uneven prime δ dividing the invariants, on the indices of the invariants divisible by δ, on the principal generic characters with respect to δ, and on the quadratic characters with respect to δ of the invariants not divisible by δ. The remaining factor ζ_n may be said to depend on the relation of the concomitants and invariants to the prime 2 and its powers. The determination of this factor presents no theoretical difficulty; but on account of the multiplicity of the cases to be considered, we shall confine ourselves in this place to the two cases in which the invariants are all uneven.

(A) When the invariants are all uneven, and the given genus is of an uneven order, let Σ_n represent the unit $(-1)^h\,\psi(0, n-1)$, where $\psi(0, n-1)$ is the simultaneous character of the given genus, and h is determined by the equation

$$\begin{aligned}4h=&(I_1-1)(I_2+1)+(I_2-1)(I_1I_3+1)+(I_1I_3-1)(I_2I_4+1)\\&+(I_2I_4-1)(I_1I_3I_5+1)+\dots\\&+(\dots I_{n-4}\,I_{n-2}-1)(\dots I_{n-3}\,I_{n-1}+1).\end{aligned}$$

The value of ζ_n then is

(1) if $n=4\lambda$,

$$\frac{1}{2^{2\lambda-1}}[2^{2\lambda-1}+(-1)^{\lambda}\Sigma_n], \text{ or } 1,$$

according as $D\equiv 1$, or $\equiv -1$, mod 4;

(2) if $n=4\lambda+2$,

$$\frac{1}{2^{2\lambda}}[2^{2\lambda}+(-1)^{\lambda}\Sigma_n], \text{ or } 1,$$

according as $D=1$, or $\equiv -1$, mod 4;

(3) if $n=4\lambda+1$,

$$\frac{1}{2^{2\lambda}}[2^{2\lambda}+(-1)^{\lambda}\Sigma_n];$$

(4) if $n=4\lambda+3$,

$$\frac{1}{2^{2\lambda+1}}[2^{2\lambda+1}+(-1)^{\lambda+\frac{D-1}{2}}\Sigma_n].$$

(B) When the invariants are all uneven, and the given genus of an even order, so that $n=2\nu$ is even, the value of ζ_n is

$$\frac{1}{2^{n-2}}\times\frac{1}{1-\left(\frac{2}{D}\right)\frac{1}{2^{\nu}}}.$$

It is easy to apply these general formulæ to particular examples; but our imperfect knowledge of quadratic forms containing many indeterminates, renders it practically impossible to test the results by any independent process. The demonstrations are simple in principle, but require attention to a great number of details with respect to which it is very easy to fall

into error. As soon as they can be put into a convenient form, they shall be submitted to the Royal Society.

Eisenstein has observed that, when the number of indeterminates does not surpass eight, there is but one class of quadratic forms of the discriminant 1, but that, when the number of indeterminates surpasses eight, there is always more than one such class. This observation is in accordance with our general formulæ, except that they imply the existence of an improperly primitive class of eight indeterminates and of the discriminant 1.

The theorems which have been given by Jacobi, Eisenstein, and recently in great profusion by M. Liouville, relating to the representation of numbers by four squares and other simple quadratic forms, appear to be deducible by a uniform method from the principles indicated in this paper. So also are the theorems relating to the representation of numbers by six and eight squares, which are implicitly contained in the developments given by Jacobi in the 'Fundamenta Nova.' As the series of theorems relating to the representation of numbers by sums of squares ceases, for the reason assigned by Eisenstein, when the number of squares surpasses eight, it is of some importance to complete it. The only cases which have not been fully considered are those of five and seven squares. The principal theorems relating to the case of five squares have indeed been given by Eisenstein (Crelle's Journal, vol. xxxv. p. 368); but he has considered only those numbers which are not divisible by any square. We shall here complete his enunciation of those theorems, and shall add the corresponding theorems for the case of seven squares. We attend only to primitive representations.

Let Δ represent a number not divisible by any square, Ω^2 an uneven square, a any exponent. By $\Phi_5(4^a\Omega^2\Delta)$, $\Phi_7(4^a\Omega^2\Delta)$, we denote the number of representations of $4^a\Omega^2\Delta$ by five and seven squares respectively; by $Q_5(4^a\Omega^2\Delta)$, $Q_7(4^a\Omega^2\Delta)$, we represent the products

$$5\times 2^{3a}\times\Omega^3\times\Pi\left[1-\left(\frac{\Delta}{q}\right)\frac{1}{q^2}\right]\times\frac{1}{\Delta},$$

$$7\times 2^{5a}\times\Omega^5\times\Pi\left[1-\left(\frac{-\Delta}{q}\right)\frac{1}{q^3}\right]\times\frac{1}{\Delta},$$

the sign of multiplication Π extending to every prime dividing Ω, but not dividing Δ; we then have the formulæ

(A) for five squares.

(1) If $\Delta\equiv 1$, mod 4,

$$\Phi_5(4^a\Omega^2\Delta)=Q_5(4^a\Omega^2\Delta)\times\eta\times\sum_1^{\Delta}\left(\frac{s}{\Delta}\right)s(s-\Delta),$$

where, if $\Delta\equiv 1$, mod 8, $\eta=12$; if $\Delta\equiv 5$, mod 8, $\eta=28$ or 20, according as $a=0$, or $a>0$. If, however, $\Delta=1$, we are to replace $\eta\times\Sigma$ by 2.

(2) In every other case,

$$\Phi_6(4^\alpha\Omega^2\Delta)=Q_3(4^\alpha\Omega^2\Delta)\times\eta\times\sum_1^{4\Delta}\left(\frac{\Delta}{s}\right)s(s-4D),$$

where $\eta=1$, or $\frac{1}{2}$, according as $\alpha=0$, or $\alpha>0$.

(B) for seven squares.

(1) If $\Delta\equiv3$, mod 4,

$$\Phi_7(4^\alpha\Omega^2\Delta)=Q_7(4^\alpha\Omega^2)\Delta\times\eta\times\sum_1^{\Delta}\left(\frac{s}{\Delta}\right)s(s-\Delta)(2s-\Delta),$$

where $\eta=30$, if $\alpha=0$, $\Delta\equiv3$, mod 8; $\eta=\frac{2}{3}\times37$, if $\alpha=0$, $\Delta\equiv7$, mod 8; $\eta=\frac{1}{3}\times140$, if $\alpha>0$.

(2) In every other case,

$$\Phi^7(4^\alpha\Omega^2\Delta)=Q_7(4^\alpha\Omega^2\Delta\times\eta\times\sum_1^{4\Delta}\left(\frac{-\Delta}{s}\right)s(s-2\Delta)(s-4\Delta),$$

where $\eta=\frac{1}{3}$, or $\frac{5}{12}$, according as $\alpha=0$, or $\alpha>0$.

The sums $\sum_1^{\Delta}$, and $\sum_1^{4\Delta}$ in these formulæ are easily reduced (by distinguishing different linear forms of the number Δ) to others more readily calculated (see the note of Eisenstein, to which we have already referred); but in the present notice we have preferred to retain them in the form in which they first present themselves.

We shall conclude this paper by calling attention to a class of theorems which have a certain resemblance to the important results established by M. Kronecker for binary quadratic forms.

Let $\frac{1}{4}\frac{F_4(M)}{\Pi\,.\,4}$ represent the weight of the quaternary classes of the invariants $[1, 1, M]$; $\frac{1}{4}\frac{F_6(M)}{\Pi\,.\,6}$ the weight of the senary classes of the invariants $[1, 1, 1, 1, M]$, then

$$F_4(M)+2F_4(M-1^2)+2F_4(M-2^2)+\ldots=\Sigma(-1)^{\frac{d+1}{2}}d^3,$$
$$F_6(2M)+2F_6(2M-1^2)+2F_6(2M-2^2)+\ldots=\Sigma d^3.$$

In the first of these formulæ M is any unevenly even number, or any number $\equiv3$, mod 4; in the second M is any uneven number: the series in both are to be continued as long as the numbers $M-s^2$, or $2M-s^2$, are positive; d is any uneven divisor of M. The origin of these formulæ (which may serve as examples of many others) is exactly analogous to that which M. Kronecker has pointed out as characteristic of the more elementary of the two classes into which his formulæ are naturally divided. Whether, for forms of four and six indeterminates, similar formulæ exist comparable to the less elementary formulæ of M. Kronecker, and whether, for forms containing more than six indeterminates, such formulæ exist at all, are questions well worthy of the attention of arithmeticians.

December 12, 1867.

Lieut.-General SABINE, President, in the Chair.

The President gave notice that, in addition to the three Fellows named at the last Meeting, he had appointed Professor John Phillips a Vice-President.

Mr. James Robert Napier was admitted into the Society.

The following communications were read :—

I. "On the Special Action of the Pancreas on Fat and Starch." By Horace Dobell, M.D. &c., Physician to the Royal Hospital for Diseases of the Chest &c. Communicated by E. Frankland, F.R.S. Received September 5, 1867.

I have been engaged for several years in experimenting with the secretion of the pancreas. The inquiry of which I now make known the results has reference especially to the mode of action of the pancreas upon fats—a point which has been the subject of investigation by various physiologists ever since the discovery of the influence of the pancreatic fluid on the absorption of fat by Claude Bernard, nearly twenty years ago.

In the chemical parts of my experiments I owe much to the efficient aid of my friend Mr. Julius Schweitzer, and to the energy and perseverance with which he carried out my suggestions under many difficulties.

The objects of my investigations have been as follows :—

1. To discover the exact character and nature of the influence exerted by the pancreas upon fats.

2. To discover a means of obtaining and preserving the active principles of the pancreas in a form suitable for experiment in the laboratory, and for administration as a remedial agent.

3. To discover the effects of the administration of the active principles of the pancreas as a remedial agent in certain wasting diseases, and to test, by an *experimentis crucis*, the truth of a conclusion on this subject, at which I had previously arrived by a process of inductive research.

I shall not occupy the valuable time of the Society by narrating the many more or less unsuccessful experiments, but restrict myself to a concise record of those attended with success.

Experiments were made with the pancreas of several different animals, but that of the pig was selected for the experiments of which I am about to give the results, as being nearest in the character of its functions to that of the human subject.

In order to ascertain the normal reaction of the pancreatic juice, and

whether this is altered by the length of time that has elapsed since the last meal, the following experiment was made with the assistance of Mr. Schweitzer and of Mr. Harris of Calne, who kindly placed his extensive pig-killing establishment at our service for the purpose.

On March 22, 1866, forty pigs were killed, and the pancreas of each examined immediately after death; the killing and examination were so rapidly conducted, that the pancreas was in each case examined while warm from the body; and the killing and examination of the forty pigs in succession occupied less than an hour.

The pigs were killed ten at a time. The first ten had been fed two hours before they were killed, the second ten five hours, the third ten nine hours, and the fourth ten had not been fed for two days.

The pancreas in each group presented the same characters in size, colour, and reaction. Each pancreas was cut through so as to lay open the principal duct, but in no case was there any fluid in the duct. Litmus-paper was applied to the interior of the duct and to the divided gland-cells, and on being pressed sufficiently against the tissues to absorb moisture, the paper was in each case reddened where it was moistened. This acid reaction was not found in the fat and muscles of the animal.

At my request, Dr. Collins, of Albert Terrace, Regent's Park, examined the reaction of the pancreas in a series of cases at the moment when all the digestive organs were under active excitement. He gave the pigs a good and relishing meal, and while they were eating it, divided the spinal marrow in the neck, so as to destroy sensation in the body. The pigs were then immediately cut open, the pancreas removed, and its reaction examined. On August 3rd he wrote me, "As you requested, I have tried a series of experiments upon the pancreas, parotid, and sublingual glands. The two latter have a decidedly acid reaction, but the pancreas I am not quite so certain about; in one batch of pigs killed in Buckinghamshire it was alkaline, but in another lot in Hertfordshire it was acid."

The reaction of the pancreas is always acid when it reaches the laboratory for experiment as quickly as possible after removal from the animal. This we have proved in many hundreds of instances.

To discover the influence of the pancreas upon fat, the fresh pancreas of the pig, freed from all adhering blood and other extraneous matters, was cut into small pieces, bruised, and mixed with lard; and to this mixture water was gradually added. In the bruised condition the pancreas had an acid reaction. By stirring this mixture of pancreas, lard, and water, the fatty character disappeared, a thick, white, creamy fluid being formed, which, on standing, solidified into a firm pasty mass. This mass had also an acid reaction. In order to free it from the *débris* of pancreas, it was pressed through muslin, and a uniform smooth creamy emulsion remained. This emulsion rapidly putrefied, but remained a permanent emulsion until putrefaction set in.

The following are the microscopical characters presented by pure lard before mixture with pancreas, and by this emulsion, which I call "crude emulsion:"—

1. "Lard" (pure).—*Aggregations* of ordinary acicular crystals of margarine. No oil-globules. No water.

2. "Crude emulsion."—A tolerably uniform granular mass with *separate* acicular crystals of margarine, oil-globules, and water abundantly distributed throughout the mass. In some places the crystals are aggregated as in No. 1. The granules range from the $\frac{1}{3000}$ to $\frac{1}{15000}$ of an inch in diameter.

This mixture of fat and water differs from all other mixtures or chemical combinations of fat and water in the following particulars.

When the "crude emulsion" is put into ether, the ether separates it into two strata—

a. An ethereal stratum above containing the fat.

b. A watery stratum below.

When the upper stratum (*a*) (ethereal solution of fat) is drawn off and the ether evaporated by a cautiously regulated heat, a pure crystalline fat remains which I call "pancreatized fat." This pancreatized fat has no tendency to putrefy, and will keep for an indefinite period. It presents the following characters under the microscope:—

3. "Pancreatized fat" (lard) consists of minute *separate* acicular crystals of margarine and fine granular matter uniformly distributed. The special character is the *complete loss* of aggregation of the crystals.

This "pancreatized fat" retains the property of mixing or combining with water, and forming a thick, smooth, creamy emulsion, that it possessed in the form of "crude emulsion" before solution in ether. The emulsion formed by mixture of "pancreatized fat" with water I call "purified pancreatic emulsion." It has, like the crude emulsion, an acid reaction, and will keep for a very long time, and presents the following microscopical characters:—

4. "Purified emulsion" (No. 3, spirit and water).—As nearly as possible the same as No. 2; the *separate* crystals more uniformly distributed, and fewer aggregations of them. No globules*.

On analysis of the lower watery stratum (*b*) resulting from the separation of the fat of the crude emulsion by ether, it is found to contain *no glycerine.*

On analysis of the pancreatized fat (3) obtained by evaporating the ether from stratum *a*, it is found that 100 parts of the pancreatized fat are saponified by 54 parts of oxide of lead, and yield 146·25 parts of lead-plaster, and 6·75 parts of glycerine.

It is also found that every 100 parts of lard used in making the crude emulsion produce 106·5 parts of pancreatized fat, the increase of 6·5 parts

* In cold weather it is necessary to gently warm the glass slide before placing the above specimens upon it, otherwise the solid constituents become agglomerated.

being solely due to absorption of water, as proved by heating the pancreatized fat, when the water separates, and the pancreatized fat is reconverted into ordinary lard.

In all the foregoing respects the pancreatic emulsion of fat differs entirely from all other kinds of emulsion of fatty matter, whether chemical or mechanical. All other emulsions of fat are destroyed by ether, the fat being restored at once to its original condition.

The influence exerted by the pancreas upon fats, therefore, appears to operate by breaking up the aggregation of the crystals of the fat and altering its hydration. It alters the molecular condition of the fat, mingling it with water in such a way that even ether cannot separate the fat from the water. A permanent emulsion is thus formed ready to mix with a larger quantity of water whenever it may be added.

The pancreas, therefore, in acting upon fat, does not decompose it into fatty acid and glycerine, the absence of the glycerine from the watery stratum (*b*), and the presence of the glycerine in the pancreatized fat of the ethereal stratum (*a*), having been demonstrated.

Action of the pancreas upon starch.—It is well known that, in addition to the influence of the pancreas upon fat, it has the power of converting starch into glycose by simple mixture. This property remains to a certain extent after the pancreas has exhausted its property of acting upon fat. The quantity of pancreas which before mixture with fat will convert about eight parts of starch into glycose, after saturation with fat will still convert about two parts of starch into glycose.

Second object.—To discover a means of preserving the active principles of the pancreas in a form suitable for experiment in the laboratory, and for administration as a remedial agent.

The properties of the pancreas can be extracted from the tissue of the gland by means of water. This watery fluid putrefies very rapidly. It has an acid reaction, a deep yellow colour, coagulates largely by boiling, leaving the colour of the fluid unaltered. It may be precipitated by lead-solution, and decomposed again by sulphuretted hydrogen.

When this watery fluid is evaporated, it forms a syrupy extract, which is highly hygroscopic and very difficult to dry. With great care and trouble, however, it may be dried. For general purposes, the drying is greatly facilitated by adding a dry absorbing-powder, such as powdered malt. For experimental purposes, it may be used in its pure undried state of syrupy extract, but must in that case be used fresh. In the dry state, either pure or mixed with malt-dust, it may be kept good for an indefinite length of time, if protected from moisture in a well-closed bottle. This extract of the pancreas, containing the active principles of the pancreas in the highest degree of efficiency, whether fluid or powdered, I call "pancreatine." This term is used only for convenience' sake, and must in no way be understood to signify that the property possessed by it is *single*. All attempts to isolate the several properties of the pancreas into separate

products have failed, no one of such products having been found to possess in perfection the property of acting upon fat in the manner described in this paper as peculiar to the pancreas. By the term "pancreatine," then, I desire to represent the *entire properties of the pancreas* extracted in a convenient form for keeping, for experiment, and for administration as a remedial agent.

One part of the pure pancreatine dried, without mixture with malt-dust, will digest at least sixteen parts of lard, and enable it to form a thick creamy emulsion, with about 100 parts of water. The emulsion thus formed presents in every respect the characters and qualities of the emulsion produced by the fresh pancreas already described. In this way therefore the active principles of the pancreas may be obtained and preserved in a form suitable for experiment in the laboratory and for administration as a remedial agent.

The third object of my investigations has especially occupied my attention in a long series of experiments at the Royal Hospital for Diseases of the Chest. Full details of these and of the results obtained have been published from time to time, during the last four years, in the medical journals; I shall not, therefore, occupy the time of the Society with any account of them in this paper.

II. "On a supposed Connexion between the Amount of Rainfall and the Changes of the Moon," being an extract of a Letter from J. H. N. Hennessey, Esq., First Assistant on the Great Trigonometrical Survey of India, to General Sabine, R.A., Pres. R.S. Communicated by the President. Received November 7, 1867.

Allow me now to say a few words in connexion with the enclosed paper. There appears to prevail a belief, more or less popular, to the effect that more rain falls at "the changes of the moon" than on the intermediate days of a lunation. As I happened to possess a record of the rainfall at the office of the Superintendent of the Great Trigonometrical Survey of Mussoorie, extending over thirteen consecutive years, I obtained Colonel Walker's permission to make use of the register, in connexion with this popular belief.

The results tabulated have been obtained by employing an *average daily* fall as the means for comparing the fall *at* "the changes" with that at intermediate intervals. The method of calculation adopted is explained in the footnote to the Table. The annual average result may be stated thus:—

	inch.
At "the changes" of the moon the *mean daily* fall of rain is..	0·466
Between "the changes" of the moon the mean daily fall is....	0·525

which is in opposition to the popular belief on the subject. I enclose the Table, on the chance of its proving sufficiently interesting to be noticed.

Average daily fall of rain between successive quarters and at each quarter of the moon from 1st of May to 31st of October of each year, measured at the Office of the Superintendent of the Great Trigonometrical Survey of India. The office stands in Mussoorie, on the most southern range of the Himalaya Mountains, lat. N. 30° 28′, long. E. of Greenwich 78° 7′; height above mean sea-level 6500 feet.

Year.	Average Daily Fall.								Total Fall from May 1 to October 31.
	☾ to ●	●	● to ☽	☽	☽ to ○	○	○ to ☾	☾	
	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inches.
1854	·644	·374	·813	·176	·630	·096	·512	·621	100·72
1855	·456	·204	·360	·918	·311	·376	·753	·733	85·85
1856	·732	·745	·703	·237	·397	·588	·347	·340	93·28
1857	·280	·319	·794	1·013	·521	·136	·368	·606	88·27
1858	·402	·448	·485	·298	·518	·157	·705	·373	84·61
1859	·665	·263	·253	·642	·306	·253	·570	·583	78·31
1860	·356	·228	·430	·719	·564	·205	·301	·073	65·81
1861	·685	·678	1·014	·372	1·332	·287	·577	·855	141·16
1862	·611	·620	·513	·651	·364	·852	·645	·530	93·91
1863	·348	·342	·862	·932	·511	·595	·291	·546	93·03
1864	·762	·409	·545	·292	·394	·328	·237	·352	82·19
1865	·543	·235	·276	·120	·443	·526	·518	·785	76·37
1866	·135	·360	·402	·580	·636	·809	·452	·483	81·15
Means of columns	·509	·402	·573	·535	·533	·399	·483	·529	89·589

General mean of ● ☽ ○ ☾ .... 0·466 inch.
General mean of ☾ to ●, ● to ☽, ☽ to ○, ○ to ☾ ...... 0·525 „

Note.—The rainfall during the *preceding* twenty-four hours was measured daily at mean noon. Suppose m_1, m_2, m_3, m_4, m_5, m_6, m_7, m_8, m_9 to denote nine such consecutive measurements of daily rainfall, registered at Mussoorie mean noon, respectively on the 1st, 2nd ... 9th of the month, and that the moon entered her first quarter at an hour nearer to noon of the 1st than to the preceding or succeeding noons. In this case the arithmetical mean of m_1 and m_2 has been entered in column ☽ as the average *daily* fall *at* the first quarter. Similarly, if full moon occurred nearest to noon of the 8th, the quantity $\frac{m_8+m_9}{2}$ has been reckoned as the average *daily* fall *at* full moon; and $\frac{m_3+m_4+m_5+m_6+m_7}{5}$ represents the average *daily* fall *from* ☽ to ○. The foregoing Table has been prepared under these conditions by Baboo Dwarkanath Dutt, Computer to the Great Trigonometrical Survey of India.

III. "Researches conducted for the Medical Department of the Privy Council at the Pathological Laboratory of St. Thomas's Hospital." By J. L. W. Thudichum, M.D. Communicated by John Simon, Esq., Medical Officer of the Privy Council. First Series.—The Chemical Nature and Composition, Combinations, and Metamorphoses of the Colouring-matters of Bile. Received November 14, 1867.

(Abstract.)

I. *On Cholophæine or Bilirubine and its Compounds.*

Sect. 1. The paper commences with a short historical retrospect on the literature of the subject under consideration, in which the researches of Berzelius, Scherer, Hein, Marchand, Heintz, Maly, and Städeler are mentioned. Sect. 2. The author then describes the mode of obtaining a red colouring-matter from ox-gallstones. These concretions have to be extracted with water, alcohol, ether, dilute hydrochloric acid, and ultimately, after repeated extraction with boiling alcohol and ether, with chloroform. This agent dissolves *bilirubine* or *cholophæine*, and deposits it, on concentration and the addition of absolute alcohol, in an amorphous condition, or in a crystallized state. Sect. 3. The crystals are dark brown, and have a splendid blue lustre. They are rhombic plates, as represented by a drawing in outline taken from a specimen magnified about twenty times. The amorphous or only crystalline modification is a powder of a splendid red, nearly orange-colour. Sect. 4. The elementary analysis of several specimens yielded results which led to the formula $C_9 H_9 N O_2$. Sect. 5. Bilirubine dissolves in ammonia, but does not form any permanent compound with it. Its combinations with fixed caustic alkalies are insoluble in an excess of lye. The neutral alkali solution, mostly the one in ammonia, yields neutral salts with monodynamic metals, half-acid salts with didynamic ones.

The *neutral monohydrated cholophæinate of silver*, $C_9 H_{10} Ag N O_3$, $Ag = 37 \cdot 5$ per cent., is a reddish-brown precipitate, which does not lose the atom of water at 110° C. As a hydrated silver-salt it is anomalous; but a few other animal products, such as hippuric acid, are known also to form such hydrated silver-salts. By means of this compound, the formula of bilirubine, or cholophæine, above given, is shown to express its atomic weight.

The basic anhydrous cholophæinate of silver, $C_9 H_7 Ag_2 N O_2$, $Ag = 57 \cdot 29$ per cent., is obtained from an alkaline solution of cholophæine and silver nitrate in ammonia, by cautiously reducing the amount of free alkali by means of nitric acid. The compound is analogous to a lead-salt, $C_9 H_7 Pb N O_2$ described lower down.

The *neutral cholophæinate of barium* is precipitated from an alkaline solution, and has the composition $C_{18} H_{20} Ba N_2 O_6$, $Ba = 27 \cdot 56$ per cent.

The *half-acid cholophæinate or sesquicholophæinate* is $C_{27} H_{28} Ba$

$N_3 O_8$, Ba = 20·75 per cent., and precipitated by neutral Ba salts from a neutral solution in ammonia. The differences between these salts were established in all their details by analyses, the means of which compare with theory as follows:—

	Neutral salt. Atom. W. 477. Theory.	Found.	Half-acid salt. Atom. W. 660. Theory.	Found.
C	43·46	44·58	49·09	50·63
H	4·02	3·98	4·39	4·37
Ba	27·56	27·55	20·75	20·66

The discovery of these salts was of particular importance, as they led to the discovery of similar calcium compounds, and thereby to important theoretical developments.

	The neutral calcium-salt, $C_{18} H_{20} Ca N_2 O_6$. Atom. W. 400. Theory.	Found.	The half-acid calcium-salt, $C_{27} H_{29} Ca N_3 O_6$. Atom. W. 563. Theory.	Found.
C	54	53·86	57·54	60·37
H	5	4·90	5·15	5·74
Ca	10	10·17	7·10	6·91

The calcium compound, on the basis of which Städeler had assumed $C_{16} H_{18} N_2 O_3$ to be the atomic formula of bilirubine, had yielded him (one analysis) 9·1 per cent. of calcium oxide, therefore less calcium than was found in the analysis of the half-acid salt, or 6·5 per cent. There is no doubt that Städeler had this half-acid compound before him. He unfortunately obtained the most unstable and uncertain of all the compounds of bilirubine, and mistook it for a neutral salt, abandoning his former correct analysis and formula of free bilirubine. With Städeler's last formula of bilirubine fall the formulæ of all other substances described by him under the names of biliverdine, biliprasin, bilifuscine, and bilihumine.

The *half-acid cholophæinate of zinc*, $C_{27} H_{29} Zn N_3 O_6$, with 11·05 per cent. of Zn, and *the neutral cholophæinate of lead*, $C_{18} H_{20} Pb N_2 O_6$, Pb = 36·50 per cent., were also obtained. *Basic cholophæinate of lead* is analogous to the basic silver-salt, as in it two atoms of hydrogen are replaced by one didynamic atom of lead. Formula = $C_9 H_7 Pb NO_2$, Pb = 56·25 per cent.

Some *copper compounds* were also obtained.

A new reaction for cholophæine is given. It consists in dissolving the dry powder in fuming sulphuric acid. A splendid green is at once produced. The substance is not biliverdine, but a product which, when isolated, contains an atom of water more than bilirubine, and is $C_9 H_{11} NO_3$, and is named cholothalline by the author. Cholothalline colours wool of a fast green, indestructible by acid, discharged by ammonia.

Cholophæine also yields a blue-coloured substance by treatment with

nitric acid (cholocyanine), of which the peculiar spectrum was determined. A great number of green, blue, violet, and red bodies can be produced by appropriate agents, which, if they could be obtained on a large scale, might find technical application.

II. *On Biliverdine or Cholochlorine and its Compounds.*

Sect. 1 treats of the mode of obtaining biliverdine. Cholophæine is dissolved in carbonate of potassium, and warmed while a current of air is passed through it. When the solution is green, it is precipitated with hydrochloric acid. The precipitated biliverdine is easily soluble in alcohol. Sect. 2 describes the physical properties of biliverdine, as a non-crystalline splendidly green substance, the solution of which gives no particular absorption phenomena in the spectrum. Sect 3 gives the elementary analyses and theory of biliverdine, which led to the formula $C_8\,H_9\,N\Theta_2$. Thus it was shown that it originated from cholophæine by the addition of oxygen and subsequent subtraction of carbonic acid.

$$C_9\,H_9\,N\Theta_2+2\Theta=C_8\,H_9\,N\Theta_2+C\Theta_2.$$

Sect. 4. treats of the compounds of biliverdine. The calcium-salt was not obtained pure. The barium-salt appeared to be $C_{24}\,H_{27}\,\mathrm{Ba}\,N_3\Theta_7$, and was precipitated by baryta-water from an alcoholic solution of biliverdine. It consequently consisted of one atom of the neutral salt with an atom of biliverdine and one of water. Lead and copper compounds were also obtained. No insoluble silver-salt could be obtained. The addition of oxide of silver to an alcoholic solution caused a reaction, consisting in an oxidation of the biliverdine.

A new reaction for biliverdine is stated. When dissolved in alcoholic ammonia, and boiled with an ammoniacal solution of silver nitrate, silver is deposited, and on addition of an acid a splendid purple matter is produced (bilipurpine).

Of these new substances and others the author hopes to treat in future communications.

Chlorine and other substitution-products are also mentioned.

The foregoing contributions will make the chemistry of the bile in the main complete. Human bile contains cholophæine, but most commonly by the side of it *bilifuscine*, a brown substance to be treated of hereafter.

Second Series.—The Chemical Nature and Composition, Combinations and Metamorphoses of the Colouring-matter of the Urine.

I. *On Uromelanine, a product of decomposition of Urochrome.*

In the Hastings Prize Essay for 1864 the author described a substance to which he gave the name of Uromelanine, on account of its origin and black colour. He now describes the method of obtaining it from putrid urine as well as fresh, and a method of purification by which it is obtained of uniform composition and in a pseudo-crystalline condition.

He has prepared twelve specimens by various processes, and analyzed many of them, as well as a great number of their compounds, with various metals. These preparations are marked respectively as in the following list:—

Prize Essay preparations. Compounds.

By boiling with $H_2\ SO_4$:—

A. I. From fresh urine. Elementary analyses. Neutral. Ba salt ($\widetilde{Ur}_2\ Ba$), Zn salt ($\widetilde{Ur}_8\ Zn_2$), Pb salt ($\widetilde{Ur}_3\ Pb_2$).

A. II. From fresh urine. Ag salt ($\widetilde{Ur}\ Ag$), Ba salt ($\widetilde{Ur}_4\ Ba_3$).

By addition of $H_2\ SO_4$, no boiling:—

A. III. From putrid urine. Ag salt ($\widetilde{Ur}\ Ag$), Ba salt ($\widetilde{Ur}_4\ Ba_3$), Ca salt ($\widetilde{Ur}_4\ Ca_3$).

Preparations made for the present research.

By boiling with $H_2\ SO_4$:—

B. I.

B. II. Two elementary analyses.

B. III.

Before boiling with $H_2\ SO_4$:—

C. I.

After boiling:—

C. II. Six elementary analyses.

Before boiling:—

D. I. Two N determinations. N = 13·58 per cent. Ag salt ($\widetilde{Ur}_2\ Ag_3$), Ca salt ($\widetilde{Ur}_3\ Ca$) and ($\widetilde{Ur}_2\ Ca_3$), Zinc-salt ($\widetilde{Ur}_3\ Zn$).

After boiling with $H_2\ SO_4$:—

D. II.

D. III.

D. IV. Two N determinations. N = 12·40 per cent. Ag salt ($\widetilde{Ur}_3\ Ag_5$), Ba salt ($\widetilde{Ur}_2\ Ba$) and Zinc-salt ($\widetilde{Ur}_2\ Zn$).

Synopsis of Uromelanates obtained with the foregoing preparations.

Silver-salts.

Preparation.	$\widetilde{Ur} \div Ag$.	Ag. found.
(A. II.), (A. III.) . .	1 ÷ 1	13·38 per cent.
(D. I.)	2 ÷ 3	18·57 ,,
(D. IV.)	3 ÷ 5	19·77 ,,

Barium-salts.

	$\widetilde{Ur} \div Ba$.	Ba found.
(D. IV.)	5 ÷ 2	7·20 per cent.
(A. I.), (D. IV.) . .	2 ÷ 1	8·34 ,,
(A. II.), (A. III.) . .	4 ÷ 3	13·28 ,,

Calcium-salts.

	$\widetilde{Ur}$ ÷ Ca.	Ca found.
(D. I.)	5 ÷ 2	2·03 per cent.
(A. II.)	4 ÷ 3	4·35 „
(D. I.)	2 ÷ 3	7·27 „

Zinc-salts.

	$\widetilde{Ur}$ ÷ Zn.	Zn found.
(D. I.)	3 ÷ 1	2·82 per cent.
(A. I.)	5 ÷ 2	3·54 „
(D. IV.)	2 ÷ 1	4·42 „

Lead-salt.

	$\widetilde{Ur}$ ÷ Pb.	Pb found.
(A. I.)	3 ÷ 2	15·70 per cent.

The analyses of (A. I.), (A. III.), (D. I.), (D. IV.), and of the two-thirds basic silver-salt ($\widetilde{Ur}_3$ Ag_5) determined uromelanine to be C_{36} H_{4} N_7 O_{10}.

Theory of atoms.		Per cent.	Found, mean.
C_{36}	432	58·93	57·21
H_{43}	43	5·86	5·74
N_7	98	13·36	12·88
O_{10}	160	21·85	24·17
	733	100·00	100·00

The *normal silver-salt* is C_{36} H_{40} Ag N_7 O_9, and is therefore $\widetilde{Ur}$+Ag —H_3 O. One atom of water leaves $\widetilde{Ur}$ when Ag enters, besides H.

The *half-basic silver-salt* shows no loss of water.

The *two-thirds basic silver-salt*, $\widetilde{Ur}_3$ Ag_5, is an exceedingly well-defined compound. Its formula is C_{108} H_{124} Ag_5 N_{21} O_{30}.

	Required in 100.	Found.
C	47·40	46·90
H	4·53	4·77
Ag	19·75	19·77
N	10·75	10·36
O	17·57	18·20
	100·00	100·00

The other nine salts mentioned in the synopsis have all been analyzed. They support each other's theory, and the details of their description must be seen in the main paper.

Uromelanine is a product of decomposition of the yellow-coloured ingredient of the urine, urochrome. Its atomic weight (733) is higher than that of any other product of decomposition of animal or organic matter; it contains neither sulphur nor iron. While the analyses of

cholophæine and biliverdine have shown that they have no apparent relation to hematine, as was formerly supposed, the analyses of uromelanine have made it probable that this substance is a derivative of the coloured part of the blood, cruorine or hematocrystalline, not, however, of hematine, for the atomic weight of hematine is apparently smaller than that of uromelanine. But crystallized cruorine has an atomic weight of about 13,000 ($Fe=0{\cdot}45$ per cent.). From such a body urochrome, including as it does uromelanine, uropittine, omicholine, and perhaps other matters (to be described in future communications), might be derived with an atomic weight of perhaps 1500, being itself near that of albumen (1612), but unable to derive from it. The author thinks it possible that the quantity of blood-disintegration might be measured by determining the amount of uromelanine obtainable from given quantities of urine excreted in given times. In any case uromelanine is one of the most remarkable substances in the whole domain of organic and animal chemistry, and the further study of its metamorphoses cannot fail to yield highly interesting results.

December 19, 1867.

Lieut.-General SABINE, President, in the Chair.

Pursuant to notice given at the last Meeting, Professor Stokes proposed, and Mr. De la Rue seconded, the Right Honourable the Earl of Rosse for election and immediate ballot.

The ballot having been taken, the Earl of Rosse was declared duly elected.

The Bakerian Lecture, "Researches on Vanadium," was delivered by Prof. H. E. Roscoe, Ph.D., F.R.S.

THE BAKERIAN LECTURE.—"Researches on Vanadium."—Part I. By HENRY E. ROSCOE, B.A., F.R.S. Received November 20, 1867.

(Abstract.)

I. *Introduction.*

Amongst the physical properties which point out the general relationship and classification of chemical compounds, none has so deservedly obtained the confidence of chemists as isomorphism. The vanadium compounds have, however, proved a remarkable and unexplained exception to the conclusions which generally follow from well ascertained identity of crystalline form. Rammelsberg, and afterwards, more completely, Schabus, pointed out the fact that the mineral vanadinite from several localities (a compound of lead vanadate and lead chloride) is isomorphous with apatite, pyromorphite, and mimetesite, minerals consisting of calcium phosphato-chloride, lead phosphato-chloride, and lead arsenato-chloride.

The crystalline form of all these minerals is an hexagonal prism, terminated by 6-sided pyramids. So far indeed has the isomorphism of these

compounds been traced, that in many specimens these minerals have been found to crystallize together in all proportions; and Heddle describes a crystal in his possession, the upper half of which consists of vanadinite and the lower half of pyromorphite.

Our knowledge concerning the chemical composition of the oxides of vanadium is derived from the accurate analytical results of Berzelius, to whose celebrated research (1831) on vanadium we are indebted for almost all we know of this metal and its compounds. From these experiments, more or less completely confirmed by Schafarik and Czudnowicz, it appears that the formula of vanadic acid is VO_3. Hence it is evident that we have here to do with either a case of dissimilarly constituted substances acting as isomorphous bodies and crystallizing together, or else the conclusions of Berzelius are mistaken, and the true formula of vanadic acid is V_2O_5, corresponding to phosphorus and arsenic pentoxides. The first of these alternatives has been properly accepted by most chemists as the only present solution of the difficulty, inasmuch as the definite experimental data given by Berzelius render the assumption of any other formula but VO_3 for vanadic acid perfectly gratuitous in the absence of experiments proving these data to be erroneous.

Berzelius based his conclusions on the following experiments, viz. (1) the constant loss of weight which vanadic acid undergoes on reduction in hydrogen at a red heat; (2) the action of chlorine on this reduced oxide, when a volatile chloride is formed and a residue of vanadic acid remains, which is found to be exactly one-third of the quantity originally taken for reduction in hydrogen. Hence Berzelius concludes that the number of atoms of oxygen in the oxide is to that in the acid in the proportion of 1 to 3; so that (assuming the lowest oxide to contain one atom of oxygen) the acid contains three atoms of oxygen, a result which he finds borne out by its capacity of saturation. The question whether the acid contains one or two atoms of metal Berzelius decides in favour of the former view, by finding that no compound corresponding to the alums is formed when vanadic acid is brought together with sulphuric acid and potash. The analyses of the volatile chloride made both by Berzelius and Schafarik confirm this conclusion, and place beyond all doubt the fact that, if the atomic weight of vanadium be taken to be 68·5 and $O=8$, the formula of vanadic acid is VO_3, that of the oxide prepared by reduction VO, and that of the chloride VCl_3.

In the present communication the author, whilst confirming these fundamental results in every particular, still arrives at a totally different conclusion from Berzelius respecting the constitution of vanadic acid and all the other vanadium compounds; for he proves that the true formula of vanadic acid is V_2O_5, when $O=16$, and the true atomic weight of vanadium $V=51·2$, inasmuch as the substance supposed to be vanadium is not the metal but an oxide, with an atomic weight of 67·2, nearly that of Berzelius's metal, whilst the supposed terchloride is an oxychloride.

The following are the grounds upon which these conclusions are based; the experimental proofs are contained in the memoir:—

(1) An oxide of vanadium exists having the atomic weight 67·2 (that of the metal of Berzelius). Hence vanadic acid contains more than three atoms of oxygen.

(2) The following vanadium oxides have been obtained, both in the dry and wet way, and their composition determined:—

(1) VO *, vanadium monoxide, or vanadyl	=	67·2
(2) V_2O_3, vanadium sesquioxide (Berzelius's suboxide)	,,	150·4
(3) VO_2, vanadium dioxide	,,	83·2
(4) V_2O_5, vanadium pentoxide (vanadic acid)	,,	182·4

(3) The so-called terchloride of vanadium, VCl_3($V = 67·2$), contains oxygen; it is an oxychloride having the formula $VOCl_3$ ($V=51·2$); it may be called vanadyl trichloride, or vanadium oxytrichloride, and corresponds to $POCl_3$, phosphoryl trichloride.

(4) Three other solid oxychlorides exist, having the composition
- (1st) $VOCl_2$, vanadyl dichloride, or vanadium oxydichloride.
- (2nd) $VOCl$, vanadyl monochloride, or vanadium oxymonochloride.
- (3rd) V_2O_2Cl, divanadyl monochloride.

(5) All the native vanadates are tribasic.

(6) Vanadium pentoxide fused with sodium carbonate displaces three molecules of carbon dioxide, showing that normal or ortho-sodium vanadate is tribasic, the formula of this salt being Na_3VO_4.

(7) The so-called monovanadates are salts corresponding to the mono-basic phosphates, and may be termed metavanadates; thus, $NaVO_3$, NH_4VO_3, $Ba2VO_3$. The so-called bivanadates are anhydro-salts, similar in constitution to the anhydro-salts of chromic and boric acids.

(8) Vanadium nitride has been prepared, which, on analysis, was shown to contain 51·2 parts by weight of vanadium to 14 parts of nitrogen.

All the reactions according to which vanadic acid was supposed (Berzelius, Rammelsberg, Schafarik) to contain three atoms of oxygen with an atomic weight $V=67·2$, can equally well be explained when V_2O_5 ($V=51·2$) is taken to represent the composition of this substance. That this is the case is seen from the following:—

	Berzelius's formulæ. (V=68·5. O=8.)	New formulæ. (V=51·2. O=16.)
(1)	$VO_3+H_2=VO+H_2O_2$	$V_2O_5+2H_2=V_2O_3+2\,(H_2O)$
(2)	$3\,(VO)+Cl_6=VO_3+2\,(VCl_3)$	$3\,(V_2O_3)+6Cl_2=V_2O_5+4\,(VOCl_3)$

* It is possible that the molecular formulæ of these substances (VO, VO_2), as well as those of the solid oxychlorides, may be a multiple of the above. Further experiment must decide whether or not these oxides, like the corresponding nitrogen compounds, are an exception to the law of even atomicities.

II. *Occurrence and Preparation of the Vanadium Compounds.*

The sources of vanadium have hitherto yielded the compounds of this metal in extremely small quantities, and consequently our knowledge of the substance is but limited. The attention of the author was drawn to the occurrence of vanadium in some of the copper-bearing beds of the Lower Keuper Sandstone of the Trias, worked at Alderley Edge and Mottram St. Andrews, in Cheshire. He obtained a large quantity of a lime precipitate, containing nearly 2 per cent. of vanadium, obtained in working up a poor cobalt-ore from Mottram in a mine now closed. The precipitate, containing mainly arsenic, iron, lead, copper, vanadium, and lime, with sulphuric and phosphoric acids, was first well furnaced with ground coal, to drive off the greater portion of the arsenic, and then roasted with one-quarter of its weight of soda-ash, so as to convert the vanadium into a soluble vanadate, and on lixiviation arsenic and the heavy metals were completely thrown down by sulphuretted hydrogen. The deep-blue solution was neutralized by ammonia, the precipitated vanadium oxide dried and oxidized by nitric acid, and the crude vanadium pentoxide thus obtained boiled out with a saturated solution of ammonium carbonate. The slightly soluble ammonium vanadate was washed and recrystallized. In order to prepare pure vanadium pentoxide from this salt it was roasted, and the powder thus obtained suspended in water into which ammonia gas was passed. The dissolved ammonium vanadate was separated by filtration from a residue containing silica, phosphates, &c. The pentoxide obtained by heating this salt was free from phosphorus. A second method of preparing the pure vanadium pentoxide consists in decomposing the pure oxychloride in water, and freeing the pentoxide from any traces of silica by exposure to hydrofluoric-acid gas. Great difficulty was experienced in obtaining vanadium free from phosphorus; all the native vanadates contain large quantities of phosphorus. The action of traces of this substance upon vanadium pentoxide is remarkable; 1 per cent. of phosphoric acid renders crystalline vanadium pentoxide black and amorphous, whilst the presence of the merest trace altogether prevents the reduction of the pentoxide in hydrogen.

III. *Atomic Weight determination of Vanadium by reduction of Vanadium Pentoxide in Hydrogen.*

This method was the one originally employed by Berzelius. It is perfectly reliable, and yields accurate results when carried out with care and due regard to the necessary precautions, which are detailed in the memoir.

In drying the pure hydrogen gas only sulphuric acid can be used, as phosphorus pentoxide used in the last drying-tube was found invariably to be carried over into the boat containing the substance; and the presence of a trace of phosphoric acid renders the complete reduction to sesquioxide impossible.

The formula of vanadium pentoxide being V_2O_5, and that of the oxide obtained by reduction being V_2O_3, the atomic weight of the metal is found from the equation

$$x = \frac{8\,(5b-3a)}{a-b}$$

where a = the weight of vanadium pentoxide taken, and
b = „ „ sesquioxide obtained.

In each of the determinations a weight of not less than 5 grammes of substance was operated on.

Nos.	Weight of vanadium pentoxide taken.	Weight of vanadium sesquioxide obtained.	Atomic weight of vanadium.
1	7·7397	6·3827	51·26
2	6·5819	5·4296	51·39
3	5·1895	4·2819	51·48
4	5·0450	4·1614	51·35

Hence the mean atomic weight from these experiments is 51·37, with a mean error of $\pm$ 0·066. Berzelius's number, calculated according to the above equation, is 52·55. The difference is probably owing to the fact that his vanadium contained a trace of phosphorus, which prevented the complete reduction.

IV. *The Vanadium Oxides.*

(1)	Vanadium monoxide, or vanadyl	VO	or V_2O_2
(2)	„ sesquioxide (Berzelius's suboxide)	V_2O_3	or V_2O_2+O
(3)	„ dioxide (Berzelius's vanadic oxide)	VO_2	or $V_2O_2+O_2$
(4)	„ pentoxide (vanadic acid)	V_2O_5	or $V_2O_2+O_3$

(1) *Vanadium Monoxide*, $VO=67·2$.—In its power of uniting with oxygen vanadium surpasses uranium, as observed by Peligot; and as this oxide is found to enter into many of its compounds, the name Vanadyl may appropriately be given to it.

Vanadium monoxide is a grey powder possessing a metallic lustre, and is obtained by passing the vapour of vanadyl trichloride, mixed with excess of hydrogen, through a combustion-tube containing red-hot carbon.

This oxide may be prepared in solution by the action of nascent hydrogen, evolved by metallic zinc, cadmium, or sodium amalgam, upon a solution of vanadic acid in sulphuric acid. After passing through all shades of blue and green, the liquid attains a permanent lavender tint, and contains the vanadium in solution as monoxide. This compound absorbs oxygen with such avidity as to bleach indigo and other vegetable colouring-matters as quickly as chlorine, and far more powerfully than any other known reducing agent. The degree of oxidation of the dissolved vanadium was estimated by a standard solution of permanganate, which had been

proved to give accurate results with a vanadium oxide of known composition, the point of maximum oxidation being obtained when the solution became pink. According to this method, 100 parts of vanadium pentoxide were shown to have lost 26·53 per cent. of oxygen on reduction with zinc; the percentage loss from V_2O_5 to V_2O_2 is 26·3.

When the neutral lavender-coloured solution of a monoxide salt is allowed to stand exposed to the air for a few seconds, the colour changes to a deep chocolate-brown from absorption of oxygen; indeed this reaction for oxygen is as delicate as that of an alkaline pyrogallate. If air be passed through the acid lavender-coloured solution of vanadous sulphate, oxygen is absorbed, and the liquid assumes a permanent blue colour, and the vanadium is contained in solution as dioxide. If the free acid contained in the lavender solution be neutralized by zinc, the liquid on exposure to air attains a permanent brown tint, which, on addition of acids, becomes green, and the solution contains sesquioxide.

(2) *Vanadium Sesquioxide*, $V_2O_3=150·4$ (Berzelius's suboxide).—Obtained as a black powder by reducing vanadium pentoxide in hydrogen at red heat. When exposed warm to the air it glows, absorbs oxygen, and passes into the highest oxide. At the ordinary atmospheric temperature it slowly absorbs oxygen, and is converted into dioxide. Vanadium sesquioxide is insoluble in acids, but may be obtained in solution by the reducing action of nascent hydrogen evolved from metallic magnesium upon a solution of vanadic acid in sulphuric acid. The changes of colour observed in the case of the monoxide solutions do not continue beyond the green, and the liquid contains vanadium in solution as sesquioxide. 100 parts of vanadium pentoxide were found to lose, on reduction with magnesium, 17·7 per cent. of oxygen; the loss on reduction to V_2O_3 is 17·5 per cent. Solutions of vanadium sesquioxide can also be obtained by partial oxidation of the lavender-coloured solution containing monoxide.

Chlorine attacks the sesquioxide in the manner first pointed out by Berzelius according to the formula $3(V_2O_3)+6Cl_2=V_2O_5+4(VOCl_3)$.

(3) *Vanadium Dioxide*, $VO_2=83·2$ (the vanadic oxide of Berzelius).—This oxide was obtained by Berzelius by precipitation from its salts. It may also be prepared in the form of blue shining crystals by allowing the sesquioxide to absorb oxygen at ordinary temperatures. It is contained in solutions, having a bright blue colour, prepared by the action of moderate reducing agents, such as sulphur dioxide and sulphuretted hydrogen, oxalic acid, &c., upon vanadic acid in solution. 100 parts of vanadium pentoxide were found to lose, on reduction with the two first-named agents, 9·03 per cent.; the loss on reduction to VO_2 is 8·75. Solutions containing the dioxide are obtained by passing air through acid solutions of the monoxide until a permanent blue colour is attained.

(4) *Vanadium Pentoxide*, V_2O_5 (vanadic acid$=182·4$).—The properties of this oxide and its compounds are considered only so far as is necessary for the elucidation of the true atomic weight of the metal.

Constitution of the so-called Monovanadates.—The analyses of Berzelius serve to point out, when the new atomic weight is adopted, that these compounds prove to be metavanadates; thus—

Berzelius's formulæ.
($V=68{\cdot}5$. $O=8$.)

Ammonia salt .... NH_3VO_3+HO
Barium „ .... $BaOVO_3$

New formulæ.
($V=51{\cdot}2$. $O=16$. $Ba=137$.)

Ammonium metavanadate ......NH_4VO_3 or $\left.\begin{matrix}NH_4\\VO\end{matrix}\right\}O_2$

Barium metavanadate.......... BaV_2O_6 or $\left.\begin{matrix}Ba\\2VO\end{matrix}\right\}O_4$

The bivanadates are anhydro-salts having the composition $2(NaVO)_3+V_2O_5$, or perhaps $Na_4V_2O_7+3V_2O_5$.

Berzelius's analysis of this ammonium salt was carefully confirmed, experiment showing that the salt yielded 77·75 per cent. of vanadium pentoxide, theory requiring 77·82 per cent. The bivanadates analyzed by Von Hauer prove to be anhydro-salts, analogous to certain chromates and borates, and possessing the composition $2(NaVO_3)+V_2O_5$.

The normal or ortho-vanadates are tribasic; the sodium salt is Na_3VO_4, or $\left.\begin{matrix}Na_3\\VO\end{matrix}\right\}O_3$; this is shown by the fact that vanadium pentoxide (V_2O_5), when fused with sodium carbonate, displaces 3 molecules of carbon dioxide.

It is the author's intention to investigate the composition of the vanadates at a future time.

V. *Vanadium Oxychlorides, and Second Atomic Weight determination of the Metal.*

(1) *Vanadium Oxytrichloride, or Vanadyl Trichloride,* $VOCl_3$.—Molecular weight 173·2. The fact that the lemon-coloured liquid chloride of vanadium prepared by the action of chlorine upon the sesquioxide (Berzelius) contains oxygen, contrary to the statements of previous experimenters, was ascertained (1) by obtaining carbon dioxide from the decomposition of the vapour of the oxychloride passing over red-hot charcoal, (2) by the production of magnesia by the action of magnesium, (3) by the formation of caustic soda by the action of sodium, (4) by the formation of vanadium sesquioxide when the vapour of the oxychloride was passed with pure hydrogen through a heated tube.

The specific gravity of vanadyl trichloride was found to be 1·841 at 14°·5, and its vapour-density 88·2 ($H=1$), or 6·108 ($air=1$), and its boiling-point 126·7 under 767·0 millims. (determined on 100 grammes of substance). Vanadyl trichloride, most carefully purified, was analyzed many times with every precaution, the chlorine being estimated both by Gay-Lussac's pro-

cess and by ordinary weight analysis. Nine volumetric analyses gave 61·306 per cent. of chlorine; seven gravimetric determinations gave 61·241 per cent. From these numbers an atomic weight of 51·05 for vanadium is obtained. The mean of 51·05 and 51·37, the number which the reduction experiments yielded, viz. 51·21, is taken as the true atomic weight of vanadium.

The vanadium in this chloride was determined as pentoxide. The result of the analyses is as follows:—

		Calculated.	Found.
V =	51·2	29·55	29·58
Cl_3 =	106·11	61·24	61·27
O =	16·0	9·21	——
	173·31	100·00	

(2) *Vanadium Oxydichloride, or Vanadyl Dichloride,* $VOCl_2 = 137·9$.— This substance is a light green crystalline solid body, obtained by the action of zinc on the trichloride at 400° in sealed tubes. It has a specific gravity of 2·88, is insoluble in water, but deliquesces on long exposure to air, and dissolves easily in acids. Analysis gave

	Calculated.	Found.
V	37·13	37·58
Cl_2	51·27	50·73
O	11·60	——
	100·00	

(3) *Vanadium Oxymonochloride, or Vanadyl Monochloride,* $VOCl$ = 102·57. This body is a brown, light, powdery solid, formed by the action of hydrogen upon vanadyl trichloride at a red heat. It is insoluble in water, but readily soluble in acids. Analysis gave

	Calculated.	Found.
V	49·96	50·21
Cl	34·45	34·53
O	15·59	——
	100·00	

(4) *Divanadyl Monochloride,* $V_2O_2Cl = 169·8$. This oxychloride is also formed by the action of hydrogen at a red heat on $VOCl_3$. It can readily be separated from the foregoing compound, as it is a heavy, shining, metallic powder, resembling "mosaic gold" in its appearance. Analysis gave

	Calculated.	Found.
V_2	60·37	61·69
Cl	18·82	18·93
O_2	20·81	——
	100·00	

VI. *Vanadium Nitrides.*

(1) *Vanadium Mononitride,* VN=65·2.—A greyish powder unalterable in the air, obtained by heating the ammonium oxychloride to whiteness in a current of ammonia. Both vanadium and nitrogen were directly estimated, with the following results:—

	Calculated.	Found.
Vanadium	78·6	77·8
Nitrogen	21·4	20·2

(2) *Vanadium Dinitride,* VN_2=79·2.—A black powder obtained by Uhrlaub on heating the ammonium oxychloride to a moderate temperature. The vanadium was estimated by Uhrlaub, but he did not understand the constitution of the substance, as he assumed the atomic weight of the metal to be 68·5, and did not estimate the nitrogen.

The vanadium nitrides not only demonstrate with absolute certainty the true atomic weight of the metal, but they also serve as the starting-point from which to commence the study of the metal itself, as well as an entirely new class of bodies, viz. the compounds of vanadium with chlorine and the other halogens.

The author hopes to describe these interesting substances in the next communication.

The Society then adjourned over the Christmas Recess to Thursday, January 9th.

January 9, 1868.

Lieut.-General SABINE, President, in the Chair.

The following communications were read :—

I. "On the Conditions for the existence of Three Equal Roots, or of Two Pairs of Equal Roots of a Binary Quartic or Quintic." By A. CAYLEY, F.R.S. Received November 26, 1867.

(Abstract.)

In considering the conditions for the existence of given systems of equalities between the roots of an equation, we obtain some very interesting examples of the composition of relations. A relation is either onefold, expressed by a single equation $U=0$, or it is, say, k-fold, expressed by a system of k or more equations. Of course, as regards onefold relations, the theory of the composition is well known: the relation $UV=0$ is a relation compounded of the relations $U=0$, $V=0$; that is, it is a relation satisfied if, and not satisfied unless one or the other of the two component relations is satisfied. The like notion of composition applies to relations in general; viz., the compound relation is a relation satisfied if, and not not satisfied unless one or the other of the two component relations is satisfied. The author purposely refrains at present from any further discussion of the theory of composition. The conditions for the existence of given systems of equalities between the roots of an equation furnish instances of such composition; in fact, if we express that the function $(*\between x, y)^n$, and its first-derived function in regard to x, or, what is the same thing, the first-derived functions in regard to x, y respectively, have a common quadric factor, we obtain between the coefficients a certain twofold relation, which implies either that the equation $(*\between x, y)^n=0$ has three equal roots, or else that it has two pairs of equal roots; that is, the relation in question is satisfied if, and it is not satisfied unless there is satisfied either the relation for the existence of three equal roots, or else the relation for the existence of two pairs of equal roots; or the relation for the quadric factor is compounded of the last-mentioned two relations. The relation for the quadric factor, for any value whatever of n, is at once seen to be expressible by means of an oblong matrix, giving rise to a series of determinants which are each to be put $=0$; the relation for three equal roots and that for two pairs of equal roots in the particular cases $n=4$ and $n=5$, are given in the author's "Memoir on the Conditions for the existence of given Systems of Equalities between the roots of an Equation," Phil. Trans. t. cxlvii. (1857), pp. 727–731; and he proposes in the present Memoir to exhibit, for the cases in question $n=4$ and $n=5$, the connexion between the compound relation for the quadric factor and the component relations for the three equal roots and for the two pairs of equal roots respectively.

II. "The Caudal Heart of the Eel a Lymphatic Heart.—Effect of the force with which the lymph-stream is propelled therefrom on the flow of blood in the Vein into which the heart opens. —Explanation of the appearance of blood propelled in successive drops, as if from the heart, along the Caudal Vein.—Influence which the force of the lymph-stream from the heart exerts in accelerating and promoting the flow of blood in the Caudal Vein." By Thomas Wharton Jones, F.R.S., Professor of Ophthalmic Medicine and Surgery in University College, London, Ophthalmic Surgeon to the Hospital, &c. Received November 26, 1867.

(Abstract.)

To explain the true nature of the phenomenon of drops of blood propelled in rapid succession, *as if* from the caudal heart, along the caudal vein,—to prove thereby that the caudal heart belongs, not to the blood-vascular system, but to the lymphatic system,—and to inquire into the influence which the force of the lymph-stream from the caudal heart exerts in accelerating and promoting the flow of blood in the caudal vein, constitute the object of this paper.

The great caudal vein of the eel is formed by the junction of two trunks, a larger and a smaller. It is into the smaller trunk, near its junction with the larger, that the caudal heart opens. At the opening, there is a valve which prevents regurgitation of the lymph back from the vein into the heart.

When by the contraction of the heart the lymph is propelled into the vein, the flow of blood from that vessel into the great caudal trunk is interrupted by the force of the lymph-stream. From the place where the heart opens into the vein to the junction of the latter with the caudal trunk, colourless lymph thus replaces red blood; whilst in the caudal trunk itself, the lymph, still under the influence of the heart's force, so far displaces the blood as to flow in a colourless stream on one side of the vessel for some distance, distinct from and unmingled with the blood-stream from the lower part of the vein and its lateral branches.

During the diastole of the heart, the stream of lymph into the vein intermitting, the flow of blood from that vessel into the great trunk of the caudal vein again takes place. No sooner, however, has a small quantity of blood entered than, systole of the heart ensuing, the stream of lymph thereby propelled into the vein, drives the small quantity of blood before it into the great caudal venous trunk, whilst it at the same time arrests, as before, the flow of blood into the great caudal vein from its tributary vessel.

Through the medium of the stream of lymph propelled into the great caudal vein at each stroke of the caudal heart, an impetus is communicated to the column of blood in that vessel, which we can see has the effect of accelerating and promoting its onward flow to the blood-heart of the animal.

We thus see that the caudal heart of the eel is a lymphatic heart, its

function being to receive lymph on the one hand, and to propel it into the great vein of the tail on the other, but that, besides this function, it at the same time performs the secondary one of accelerating and promoting the flow of the blood in the great caudal vein in its course back to the blood-heart.

So far as the author has been able to ascertain, no one has hitherto given a correct explanation of the phenomenon of small drops of blood propelled in rapid succession, *as if* from the caudal heart, along the caudal vein. Without first showing that these small drops of red blood are *not* propelled *from* the caudal heart, and without showing that it is colourless lymph alone which is *really* propelled *therefrom*, no one could be warranted in dissenting from Dr. Marshall Hall, the discoverer of the caudal heart, in his opinion as to the nature of the organ, viz. that it is an auxiliary blood-heart, or in pronouncing it, how correctly soever, to be a lymphatic heart.

January 16, 1868.

Lieut.-General SABINE, President, in the Chair.

The following communication was read :—

Notices of some Parts of the Surface of the Moon, illustrated by Drawings. By John Phillips, M.A., D.C.L., F.R.S., F.G.S., Professor of Geology in the University of Oxford. Received January 9, 1868.

(Abstract.)

My first serious attempts to portray the aspect of the moon were made with the noblest instrument of modern times, the great telescope of Lord Rosse, in 1852. The mirror was not in adjustment, so that the axes of the incident and reflected pencils of light were inclined at a very sensible angle. This being met by a large reduction of the working area of the mirror, the performance was found to be excellent. I have never seen some parts of the moon so well as on that occasion. But when I came to represent what was seen, the difficulty of transferring from the blaze of the picture to the dimly lighted paper, on a high exposed station, with little power of arranging the drawing-apparatus, was found to be insuperable, and the effect was altogether disheartening. It was like setting down things *ex memoriâ*, to give the rude general meaning, not like an accurate and critical copy. I present as a specimen of this memorial a sketch of the great crater of Gassendi (No. 1).

I next mounted, in my garden at York, a small but fine telescope of Cooke only 2·4 inches in the aperture; and, aware of the nature of the difficulty which beset me at Birr Castle, I gave it an equatorial mounting, without, however, a clock movement. The whole was adapted to a large solid stone pillar in the open air. It was not possible, with $\frac{1}{80}$ of the light of the Rosse mirror, to *see* so well; but it was easy to represent far better what one saw, with a conveniently placed board to hold the draw-

ing-paper, a well-arranged light, and no necessity of changing position. I made in this manner the drawing of Gassendi which is marked No. 2.

My next attempt was made in the same situation with a fine telescope by Cooke, of 6¼-inch aperture and 11 feet sidereal focus, mounted equatorially, in the old English mode, and carried by clockwork. With this excellent arrangement I was enabled to use photography very successfully, and to obtain selenographs 2 inches across in 5^s of time. The drawing of Gassendi, No. 3, was made with this instrument (1853).

From these experiments the conclusion was obvious:—that for obtaining good drawings of the moon, convenient mounting was actually more important than great optical power; and that for such a purpose it was desirable to increase in every way the comfort of the observer, and furnish him with special arrangements for his own position and the placing of his drawing-board and light.

Having been called to reside in Oxford (1853-54), my plan for continual work on the moon was entirely cut through; it was impossible to mount a large instrument near my dwelling till (in 1860) the ground was arranged about the museum, so as to give me the requisite space and security close to the house which had been appointed for me by the University. I then arranged with Mr. Cooke for a new telescope of 6 inches aperture, to be protected in a well-planned observatory, the construction of which was aided by the Royal Society. I now propose to give a short notice of some of the results of my work with this instrument, in connexion with remarks on the most advisable course to be followed by other surveyors of the moon.

In making drawings of ring-mountains on different parts of the moon's disk, the artist will be much aided by a projection of the mountain-border on the scale intended, from a few measures, with its proper figure due to the latitude and longitude. Eye-drafts not thus controlled are apt to become absurd, by the heedless substitution of an ideal circle for a real ellipse. Thus I have seen Gassendi forgetfully represented by more than one skilful artist. Even with the advantage of such a projection (of which I give an example for Gassendi, No. 5) very considerable difficulties occur. One is the variation of outline caused by the displacement of the boundary of light and shade—first when the incidence of light varies through different angles of elevation of the sun, and next when the moon's position causes her to receive the light at the point observed on a different lunar azimuth. Even on so great a ring as Copernicus the variation of the outline as given by different artists is remarkable—hardly any one agreeing with what is really the most accurate drawing of all, that by P. Secchi; and that represents, not a simple ring, but a seven-angled outline. Dates must always be annexed to the figures; and as it is rarely possible to complete a good drawing of a large crater, except in two or three lunations or more, it becomes very essential that a bold free sketch be made of the moon's shadows to control the special work. (No. 6 is given as an example.)

Strictly speaking, there should be at least three drawings of a ring-mountain—in morning light, at midday, and in evening. It would be better to have five drawings, one at sunrise and another at sunset being added to the three already named. It will be found most convenient to make the drawings within two hours of the moon's meridian passage.

Shadows thrown from objects on the moon have exactly the same character as those observable on the earth. They are all margined by the penumbra due to the sun's diametral aspect; this is always traceable except very near to the object; but in consequence of the smaller diameter and more rapid curvature of the moon's surface, the penumbral space is narrower. At the boundary of light and shade, on a broad grey level tract, the penumbral space is about nine miles broad, quite undefined, of course, but perfectly sensible in the general effect, and worthy of special attention while endeavouring to trace the minute ridges (of gravel?) or smooth banks (of sand?), which make some of these surfaces resemble the postglacial plains of North Germany, or central Ireland, or the southern parts of the United States, which within a thousand centuries may have been deserted by the sea.

To the same cause is due the curious and transitory extension of half-lights over some portions of the interior of craters, while other neighbouring portions have the full light. The effect is occasionally to produce half-tints on particular portions of terraces within the crater, as in the case of Theophilus, of which I present two drawings, one showing this peculiarity in the morning light, the other not. The central mountains of that great crater are high enough to throw long shadows; and these, as they catch upon other peaks or spread, softening with distance, over the surrounding plains, present far greater variety of shadow-tones than might be expected on a globe deficient, as the moon really appears to be, of both air and water.

The different parts of the moon's surface reflect light very unequally; the dark parts have several degrees of darkness, the light parts several degrees of light. On the same level, as nearly as can be judged, under the same illumination, neighbouring parts are not only unequally reflective, but their light seems to be of different tints. Within the large area of Gassendi, under various angles of illumination, but more conspicuously when the angle of incidence deviates least from verticality, patches of the surface appear distinctly marked out by difference of tint, without shadow. It is well known that in this particular photography has disclosed curious and unexpected differences of the light, which were not apparent, or not so obvious, to the eye. Reflecting telescopes seem to be indicated as most suited for direct observation of differences of the kind of light on the moon.

The surface of the moon is hardly anywhere really smooth, hardly any where so smooth as may be supposed to be now the bed of a broad sea on our globe. By watching carefully the curved penumbral boundary of light and shade—as it passes over ridge and hollow, rift and plain,—broad swells,

minute puckerings, and small monticules appear and disappear in almost every part. In several of the *maria*, minute angular cup-craters about half a mile across are frequent; and on several of the exterior slopes of the crater-rings are seen pits, ridges, fissures, and rude craters, something like the sloping surfaces of Etna. Copernicus is a good example of this common occurrence. It appears extremely desirable that the details of this magnificent mountain should be carefully reexamined on the basis of Secchi's fine drawing, for the purpose, amongst others, of determining exactly how many of the bosses and ridges bear cup-hillocks; for many inequalities, which in feeble telescopes have but the indistinct character of being heaped up, appear distinctly crateriform with superior optical power *.

On the very crest of a ring-mountain it is rare to find a cup-crater; quite common to find them in the interior, especially towards the middle, and, in several cases, exactly central. But it happens often that the central mountain-mass of a large crater, such as Gassendi and Theophilus, is of a different structure. In the former a complicated digitated mass of elevated land appeared to me for a long time to be entirely devoid of any small craters; by continued scrutiny at last I see on one of the masses a distinct depression. The area in Gassendi reminded me of the volcanic region of Auvergne, in which, with many crater-formed mountains, occur also the Puy de Dome, Puy Sarcoui, Puy Chopin, and others which are heaps of a peculiar trachyte not excavated at the top, while the others are formed of ashes and lava-streams and are all crateriform. The central masses of Theophilus (Nos. 7, 8, and 9) are very lofty and grandly fissured from the the middle outwards, with long excurrent buttresses on one side, and many rival peaks separating deep hollows, and catching the light on their small apparently not excavated tops. This is like the upheaved volcanic region of Mont d'Or, with its radiating valleys, wide in the central part, and contracted to gorges toward the outside of the district.

The Vesuvian volcanic system, including the Phlegræan fields, exhibits, in all respects but magnitude, remarkable analogy with parts of the moon studded with craters of all magnitudes, as those adjoining Mount Maurolycus, engraved for comparison by Mr. Scrope in his admirable treatise on Volcanos (p. 232). It is probable that many of the differences which appear on comparing lunar ring-mountains may be understood as the effects of long elapsed time, degrading some craters before others were set up, and turning regular cones and cavities into confused luminous mounds. It would much augment our confidence in the possible history of the moon which these differences seem to indicate, if we could believe it to have ever been under the influence of atmospheric vicissitude as well as changes of interior pressure.

That the latter cause has been in great activity at some early period of the moon's history is not only evident, by the many sharply cut fis-

* See "Comparative Remarks on Gassendi and Copernicus," Roy. Soc. Proc. for 1856, p. 74.

sures which range like great faults in our earthly strata for five, ten' twenty, and sixty miles, but is conspicuously proved by the great broken ridges of mountains which, under the names of Alps, Apennines, and Riphæan chains, make themselves known as axes of upward movement, while so many of the craters near them speak of local depression. I have not been able to discover in these great ridges any such marks of successive stratification, or even such concatenation of the crests, as might suggest symmetrical and anticlinal axes. The surface is, indeed, as rough and irregularly broken as that of the Alps and Pyrenees, and marked by as extraordinary transverse rents, of which one, in the Alpine range near Plato, is a well-known example. Must we suppose these mountains to have undergone the same vicissitudes as the mountain-chains of our globe —great vertical displacement, many violent fractures, thousands of ages of rain and rivers, snow and glacial grinding? If so, where are the channels of rivers, the long sweeps of the valleys, the deltas, the sandbanks, the strata caused by such enormous waste? If the broad grey tracts were once seas, as analogy may lead us to expect, and we look upon the dried beds, ought we not to expect some further mark of the former residence of water there than the long narrow undulations to which attention has already been called as resembling the escars of Ireland?

In any further attempts of my own to contribute facts toward the survey of the moon, now again taken in hand by the British Association, I shall probably select for careful work some particular features, such as the mountains in the midst of a large crater, the bosses and cup-like hills on the outward slopes of such a crater, the rents in mountain-ridges, and the low winding banks which appear on the broad grey tracts. But, for those who desire to perform a work of high value, I would earnestly recommend a strict reexamination of every element in the great picture of Copernicus, for which we are indebted to the Roman Astronomer.

The paper was accompanied by twelve drawings, which were exhibited to the Society, and of which the following is a list:—

No. 1. Sketch of Gassendi taken in 1852, at Birr Castle, with the great telescope of Lord Rosse.

No. 2. Sketch taken in 1852, at York, with an achromatic by Cooke, of 2·4 inches aperture.

No. 3. Sketch taken in 1853, at York, with an achromatic by Cooke, of 6¼ inches aperture. (Morning.)

No. 4. Sketch taken in 1862, at Oxford, with an achromatic by Cooke, of 6 inches aperture. (Evening.)

No. 5. Working plan of Gassendi and scale.

No. 6. Free-hand sketches to illustrate the mode of working for general effect. Oxford, 1864.

No. 7. Theophilus, Cyrillus, and Catharina, taken at Oxford in 1862. This is about the third attempt.

No. 8. Theophilus, reexamined in 1863. This is the most complete drawing which I *can* make with my 6-inch. I intended to repeat the whole group.

No. 9. The central mountain-group of Theophilus on a large scale. 1863.

No. 10. Posidonius, early morning. 1863. (Unfinished.)

No. 11. Posidonius, nearer to midday. 1863. (Unfinished.)

No. 12. Aristarchus and Herodotus. This is about the sixth drawing, and exhibits in Aristarchus a double crater-wall, the inner one being sharp and interrupted; a deep narrow fissure separates the two walls. The interior surface is more moulded than in any drawings yet published. Herodotus, the dark crater, is merely sketched to give the course of the seeming valley which conducts from it to the seeming delta.

January 23, 1868.

Dr. WILLIAM B. CARPENTER, Vice-President, in the Chair.

The following communications were read:—

I. "Contributions towards determining the Weight of the Brain in the different Races of Man." By JOSEPH BARNARD DAVIS, M.D. &c. Communicated by Prof. JOHN MARSHALL. Received November 30, 1867.

(Abstract.)

It would naturally be expected that great attention had been directed to the human brain, the organ of mental manifestation. Still little has been done to ascertain its relative magnitude in the different races of mankind. Opportunities for examining exotic brains are rare, and it is only by gauging the internal capacities of human skulls, and deducing the weight of the brain, that data can be obtained.

The inferiority of this method is not so clear as has been assumed, since we are able to fix upon an unchangeable substance of definite specific gravity for the purpose of this gauging, whereby we compensate for the variable condition of the brain, depending upon disease and other causes, and the immediate occasion of death.

The great difficulty hitherto has been to decide upon a definite allowance, or scale of allowance for other matters besides brain which always fill up the cavity of the skull, in different proportions at different ages, &c. In the present investigations it has been considered most advisable to fix upon a definite, and at the same time proportionate, rule for compensating for these fluids and membranes. And, after much inquiry, that rule has been laid down as a general tare of 15 per cent. on the capacity af the skull.

In former inquiries of this kind by Prof. Tiedemann and Prof. Morton, this allowance has been entirely or almost entirely overlooked, by which means their extended observations really refer to the internal capacities of human skulls, and not to the weights of the brain, as they supposed. No

doubt internal capacities themselves become a legitimate means of comparison.

Synostotic and artificially deformed skulls are mostly included, for the reason specified, that their internal capacities are usually not materially interfered with.

Morton also failed to distinguish the sexes, and his Tables give no indication how many of the skulls were those of men and how many those of women. When we recollect the great normal diversity in the size of the brain in the two sexes in any given race, this omission becomes of serious importance. In the series of skulls now examined, this diversity in the weight of the brain in the two sexes extends from less than 10 per cent. to something more than 12½ per cent.; so that Prof. Welcker's datum of 10 per cent. is tolerably correct. In our measurements the sexes are marked, and adult examples only included. All the crania have been carefully and as uniformly as possible filled with dry Calais sand of a definite specific weight, which has been afterwards weighed and then reduced to its equivalent in cerebral matter of 1040 specific gravity, after the deduction of the 15 per cent. The observations on the weight of the brain in all the races have been arranged in seven Tables, corresponding very nearly to the races of the great divisions of the globe. The results show that Prof. Tiedemann was misled when he hastily assumed that, inasmuch as a certain size and mass of brain was an essential condition for the exercise of the faculties of the mind, all human races are furnished therewith in an equal degree.

One important object has always been kept in view—namely, a careful comparison of the calculated observations of the Tables with the actual determinations of those who have weighed the brains of different races, as far as such determinations have extended. This has been done with the view of comparing and correcting our results.

It would be difficult to give any intelligible abstract of the Tables which accompany the memoir. Of the notes to these Tables some short account may be given here.

Dr. Peacock and other excellent and careful observers pretty nearly coincide in the conclusion that the brain of *Englishmen*, on the average, is about 49 oz. av. in weight, or 1389 grammes. Dr. Robert Boyd, in his memoir in the Philosophical Transactions, states as the result of his vast experience, that the adult *male* brain among the *insane* varied from 48·17 oz. to 43·87 oz.; among the *insane women* from 44·55 oz. to 40·55 oz.; whilst in the *sane* adults the averages varied in the *men* from 48·20 oz. to 45·34 oz., and among the *women* from 43·70 oz. to 39·77 oz. It thus appears that Dr. Boyd's investigations bring out an average brain-weight among the English of, speaking roundly, about 5 oz. less than Dr. Peacock's means, and rather more than 3 oz. less than our means. Dr. Thurnam, who examined and weighed the brains of 257 insane men and 213 insane women, agrees in his results with Dr. Boyd, being still rather below the averages

obtained by the latter. The general result of our Table is an average brain-weight for the English of 47·50 oz., or 1346 grms., which agrees tolerably well with the conclusions deduced by all these observers, being a mean term.

The mean derived from the 16 *French* skulls is 45·47 oz., or 1280 grms.—that is, 66 grms. less than the English. The general result of an extended series of measurements by Prof. Broca of 357 French crania, those of men and women undistinguished, when subjected to our rule, gives a mean of 44·58 oz., or 1263 grms.—that is, 17 grms. below our deduced average.

The skulls of *Italians*, of *Lapps*, and of *Swedes* agree in giving a brain-weight closely coinciding with that of the English. Those of the *Frisians* and *Dutch* come into the same category.

In the 15 *German* skulls (it should be observed that 13 are those of men) the average brain-weight exceeds that of the English. It is 50·28 oz., or 1425 grms., an excessive weight. This probably follows from the unusual size of these German skulls, as well as in some degree from the great predominance of men's skulls; for Prof. Huschke, who weighed upwards of 60 brains of Germans, two-thirds being those of men, found the mean weight to be no more than 1384 grms. Prof. Rudolph Wagner also tested by the balance 31 brains of Germans, the larger half being those of men, and obtained a mean of only 1300 grms., itself a sufficiently large weight. In the investigations of Prof. Welcker, who employed 30 adult normal skulls of men and 30 of women, the mean brain-weight of the series rises only to 42·83 oz., or 1214 grms. This seems to be conclusive that our specimens are large skulls, and that the size of the German brain has been somewhat overrated. The result of further investigation will probably be to correct these discrepancies.

In entering upon the decidedly brachycephalic races of Europe, it must be noted that we have for our examination the skulls of men only, and those in small numbers, which will prevent any accurate comparison with the rest of the series. The mean brain-weight of two male *Poles* is 47·14 oz., or 1336 grms. That deduced from Dr. A. Weisbach's gauging of 25 skulls of young Polish men, when subjected to our rule, is 47·21 oz., or 1338 grms., a result almost identical with that from our observations.

The *Gipsies* of Wallachia present a marked diminution of brain-weight when compared with the *Rumangos* and other races of that region. The mean of 6 male Rumango skulls is 45·97 oz., or 1303 grms.; that of two male Gipsies is 43·93 oz., or 1245 grms.

Although it has reference merely to the collection of skulls upon which these observations have been made, the order in which the different European races range themselves, beginning with the heaviest brain-weights and proceeding to the lightest, is given in the memoir. The general mean of the European series is 46·87 oz., or 1328 grms.

In entering upon the Asiatic Races, we are at once struck with the small brain-weights of the people at the commencement of the Table, the

Vedahs of Ceylon and the *Hindoos*. The mean obtained from 35 crania of males of Hindoo Tribes is 44·22 oz., or 1253 grms.; that from those of 31 females 39·99 oz., or 1133 grms.,—which yields a mean of the two sexes of 42·11 oz., or 1193 grms. A reference to Dr. Morton's observations shows that his series, when properly calculated by our rule, give a still lower mean, viz. 41·74 oz., or 1183 grms.

The skulls of *Mussulmen* afford a slightly increased average of brain-weight over those of Hindoos. This perhaps might have been anticipated.

The two skulls of male *Khonds*, one of the unquestioned aboriginal races of India, show a brain-weight of 37·87 oz., or 1073 grms.

As we ascend the Himalayan Slope we reach races who have a somewhat increased volume of brain. The *Lepchas, Bodos*, and *Bhotias* range about 46 oz.

And when we reach what have been called the Indo-Chinese races, the brain-weight becomes again more considerable. The mean of the *Siamese* is 47·14 oz., or 1336 grms., that of the *Chinese* 47·00 oz., or 1332 grms., and that of the *Burmese* 47·87 oz., or 1357 grms.

Of the races of *Japan* only two crania of true Japanese have been obtained; but of the aboriginal *Ainos*, of the Island of Yesso, the brain-weight, as deduced from four skulls, is 45·83 oz., or 1299 grms.

The general average of the Asiatic Table shows a diminution, when compared with that of the European. The numbers are, for the latter, 46·87 oz., or 1328 grms., and for the former, 44·62 oz., or 1266 grms., which is a diminution of more than two ounces, or 62 grms.

At the commencement of the African Races we encounter the *Berbers* and the *Guanches*, the former inhabitants of Teneriffe. These are people with rather small brains. The general mean amounts to 43·49 oz., or 1233 grms.

Of the Continental people, the *Negroes of Tribes unknown* give a slightly increased brain-weight of 44·08 oz., or 1249 grms. The *Dahomans* rise to a mean of 46·34 oz., or 1313 grms., and the *Bakeles*, a warlike tribe on the Equator, to an equally high brain-weight. A brain of an adult Negro, weighed by Dr. Peacock, was found to weigh 45·50 oz., or 1289 grms. Another, examined by Dr. Edmond Simon, was found to weigh, *with the membranes*, 1226 grms.

In passing to the more Southern portions of the African continent we find the races much contrasted in respect of their brain-weights. The Kafir skulls, seven-eighths of which are those of men, present a mean of 48·16 oz., or 1365 grms., whilst that of the small Bushmans reaches only to 39·70 oz., or 1125 grms.

In Prof. Marshall's valuable and elaborate account of the Brain of a Bushwoman in the Philosophical Transactions, there is a careful calculation of the original weight of the brain, which, by means of experiment upon human brains subjected to the action of spirits of wine, he was able to restore to its original weight; and he decided this to have been 30·75 oz.,

or 875 grms. Prof. Marshall afterwards gauged the capacity of the skull by filling it with water, which he has given. He found it to be 60·64 cubic inches. With this capacity given, it is easy to determine that the weight of the brain would have been 31·01 oz. according to our rule. Prof. Marshall's calculation very closely approximates to this, viz. 30·75 oz., or only a quarter of an ounce different. This case, in which the brain-weight was restored and the skull was also gauged, seems to be a good instance for testing the accuracy of our method; and the result appears to prove that it may be relied upon with much confidence.

The general mean of our African Races is less than that of European Races,—a result which is not in agreement with Tiedemann's conclusions. It is rather more than two and a half ounces less than our European mean.

In passing to the AMERICAN RACES we have placed first the *Esquimaux* of the whole Arctic Circle. They present the large general mean brain-weight of 46·56 oz., or 1319 grms.

A series of mostly individual skulls belonging to different *American Tribes* affords a general mean of 46·23 oz., or 1310 grms. With these may be compared the 164 skulls of the "Barbarous Tribes" of Morton's American Family. When the mean cubic contents of these are reduced to our terms, with the observance of our rule, they produce a brain-weight of 42·84 oz., or 1214 grms., which is less than our mean by something more than three ozs.

When we arrive at the Caribs, former inhabitants of the Antilles, there is a considerable falling off; they descend to 42·32 oz., or 1199 grms.

Among South American Tribes, the *Amizcas*, or ancient inhabitants of the plain of Bogotá, afford a mean brain-weight of 44·20 oz., or 1253 grms. This is about the average until we reach the warlike *Araucanians*. Of these, six skulls, five of which are those of men, and one of them also a megalocephalic skull, ascend to a mean brain-weight of 48·02 oz., or 1361 grms.

The average of the whole of the American races reaches 44·73 oz., or 1268 grms., which is 2·14 oz., or 60 grms. less than that of the European races. It also comes so near to the general mean of the Asiatic and African races as to produce the impression that the whole must be regarded as pretty nearly equal.

The AUSTRALIAN RACES belong to two families—the *Australians* proper, and the *Tasmanians*; and they are remarkable among human races as possessing the smallest brains. The mean brain-weight among the former is 41·38 oz., or 1173 grms., and among the more robust Tasmanians 42·25 oz., or 1197 grms. The mean of the two families when combined reaches to 41·81 oz., or 1185 grms. This is a brain-weight one-ninth less than that of the general average of Europeans.

The last great section into which we have divided human races is that of OCEANIC RACES. It includes the aboriginal inhabitants of all the Islands, both of the North and the South Pacific Oceans. When we arrive at this section we seem as if we were returning in some measure to the large brain-weights of Europeans.

The eight skulls of *Malays* (six of men and two of women) afford the highest mean of any of the Oceanic Races, viz. 47·07 oz., or 1334 grms. For such a bold and enterprising race, who have pushed their migrations, chiefly for commercial purposes, over almost the whole Ocean, such a rich cerebral endowment might have been in some measure expected.

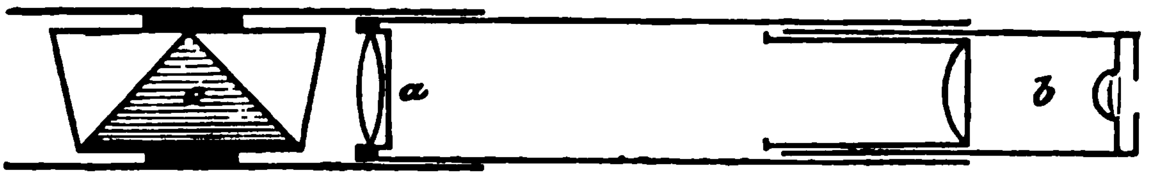
The collection which has afforded the materials for this Memoir is rich in crania from the Dutch dominions in the East-Indian Archipelago. These are distinguished for a tolerably high average of brain-weight. And this is not much diminished when we reach the aboriginal inhabitants of the Polynesian Islands and Western Pacific.

In conclusion, it is believed that this investigation has contributed much more than any former one to define and to discriminate the brain-weights of different human races. Hence it is hoped that it will be accepted as a valid contribution to a most important subject.

II. "Description of a Hand Spectrum-Telescope." By William Huggins, F.R.S. Received December 19, 1867.

The instrument described in this paper was contrived in the summer of 1866, for the purpose of observing the spectra of meteors and their trains. The special suitability of this apparatus, as *a hand-spectroscope*, for the examination of the spectra of the lights which may be seen about the sun during the total solar eclipse of next year, induces me to offer a description of it to the Royal Society.

The apparatus consists essentially of a direct-vision prism placed in front of a small achromatic telescope.



The achromatic object-glass marked *a* is 1·2 inch in diameter, and has a focal length of about 10 inches. The eyepiece (*b*) consists of two plano-convex lenses. As a large field of view is of great importance, especially for its use as a meteor-spectroscope, the field-lens is made of nearly the same diameter as the object-glass. The imperfect definition at the margin of the field is not of much practical importance, as the spectra can be brought for examination into the centre of the field. The field-lens is fixed in a sliding tube, which permits the distance between the two lenses of the eyepiece to be altered; in this way the magnifying-power of the instrument may be varied within certain limits at pleasure. Before the object-glass is fixed a direct-vision prism (*c*), consisting of one prism of dense flint glass, and two prisms of crown glass.

The field of view of my apparatus embraces an area of sky of about 7° in diameter. The spectrum of a bright star has an apparent length of nearly 3°. The spectrum of the Great Nebula in Orion appears as two

bright lines, one of them broad, crossed by a faint continuous spectrum. The magnifying-power of the telescope is insufficient to show the three distinct lines of which the spectrum of the nebula consists. The continuous spectrum is due to the stars of the trapezium, and the other fainter stars scattered over the nebula.

For the purpose of testing the efficiency of this instrument as a meteor-spectroscope, I observed the spectra of fireworks seen from a distance of about three miles. The bright lines of the metals contained in the fireworks were seen with great distinctness. I was able to recognize sodium, magnesium, strontium, copper, and some other metals.

Unfortunately I was prevented from making the use of the instrument which I had intended at the display of meteors in November 1866. I have, however, great confidence in the suitability of the apparatus for the prismatic observation of meteors and their trains.

As the instrument is not provided with a slit, it is applicable only to bright objects of small size, or to objects so distant as to subtend but a very small angle. It is obvious that if the object has a diameter smaller in one direction than in any other, as would usually be the case with the trains of meteors, the instrument should be rotated to take advantage of the form of the object. The most favourable position will be when the smallest diameter of the object is perpendicular to the height of the prisms. In this way I have seen the lines of Fraunhofer in the spectrum of the moon when a very narrow crescent.

In the case of objects which appear as points, a small breadth may be given to the spectrum by a cylindrical lens fitted in a little cap which slips over the eye-lens, and is placed next to the eye.

As some of the advantages which this instrument possesses over an ordinary spectroscope, or over a prism held before the eye, may be stated the comparatively large amount of light which the object-glass collects, the great facility for instantly pointing the instrument to the object desired, which the large field of view affords, and in some cases the magnifying-power of the instrument.

It may perhaps be mentioned that secret signals might be conveyed at night by means of the temporary introduction of certain suitable substances, as preparations of lithium, copper, strontium, &c., into the flame of a lamp giving a continuous spectrum; the presence of the bright lines due to these substances would not be perceived except by an observer provided with a spectrum-telescope, to whom they might convey information in accordance with a previous arrangement.

This little instrument, held in the hand and directed to the place of the sun during its eclipse in 1868, might enable an observer, who was not provided with larger apparatus, to give an answer to the important question whether the bright prominences are self-luminous or reflect solar light. At least it would be possible for him to determine the general character of the spectrum of a bright prominence so far as to learn whether

it is continuous or consists of bright lines. On account of the low magnifying-power of the instrument, the red prominence would appear sufficiently small to permit of bright lines being distinguished on its spectrum, if such should exist.

The instrument should be previously focused by the observer on the moon, or some distant object.

Should a portion of the sun's limb be visible, the instrument must be rotated until the spectrum of the little projecting prominence appears in a direction parallel to that of the spectrum of the sun's limb, and is not overlapped by it. Perhaps a diaphragm across the field of view and cutting off about one-third of it would be an advantage, as the spectrum of the sun's limb might be concealed behind it. The eye, relieved in this way from the bright solar spectrum, would be in a more favourable state to examine the fainter spectrum of the red prominence.

Four of these instruments, made by Mr. Browning, have been sent out by the Royal Society to India, to be placed in the hands of observers stationed at different places along the central line of the eclipse. This instrument would be specially suitable for use at sea.

Postscript.—Mr. Browning has recently suggested a method of diminishing the apparent velocity of meteors by the use of a concave cylindrical lens placed with its axis perpendicular to the direction of their motion. This mode of observing may be applied to the spectrum-telescope by substituting, when required, a plano-convex cylindrical lens for the eye-lens of the eyepiece. If this lens be placed with its axis parallel to the height of the compound prism before the object-glass, and if the telescope be held in a position such that the direction in which the light of the meteor is dispersed is perpendicular to that of its motion, the spectrum of the meteor will be magnified, as when the ordinary eye-lens is employed, but the apparent velocity of the meteor will be less by an amount equal to the magnifying-power of the eye-lens.

January 30, 1868.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "Remarks upon *Archæopteryx lithographica.*" By Prof. T. H. Huxley, LL.D., F.R.S. Received January 1, 1868.

The unique specimen of *Archæopteryx lithographica* (von Meyer) which at present adorns the collection of fossils in the British Museum, is undoubtedly one of the most interesting relics of the extinct fauna of long-past ages; and the correct interpretation of the fossil is of proportional importance. Hence I do not hesitate to trouble the Royal Society with

the following remarks, which are, in part, intended to rectify certain errors which appear to me to be contained in the description of the fossil in the Philosophical Transactions for 1863 *.

It is obviously impossible to compare the bones of one animal satisfactorily with those of another, unless it is clearly settled that such is the dorsal and such the ventral aspect of a vertebra, and that such a bone of the limb-arches, or limbs, belongs to the left, and such another to the right side.

Identical animals may seem to be quite different, if the bones of the same limbs are compared under the impression that they belong to opposite sides; and very different bones may appear to be similar, if those of opposite sides are placed in juxtaposition.

The following citations, and the remarks with which I accompany them, however, will show that these indispensable conditions of comparison have not been complied with in the memoir to which I refer.

1. "The moiety (Plate I.) containing the greater number of the petrified bones exhibits such proportion of the skeleton from the inferior or ventral aspect" (*l. c.* p. 34).

I propose to show, on the contrary, that the fossilized animal presents, in general, its dorsal aspect to the eye, though one of the most conspicuous bones may have been so twisted round as to exhibit its ventral face.

2. The demonstration that the bones of the *Archæopteryx* are thus wrongly interpreted, may be best commenced by showing that what is called "right femur (65), tibia (66), and bones of the foot (68, *i, ii, iii, iv*)," *l. c.* p. 35, are respectively the left femur, left tibia, and bones of the left foot.

That such is the case is very easily proved by the circumstance that (as is very properly pointed out in the memoir) the second toe of the foot in question is that which lies uppermost, while the plantar surface of the foot is turned outwards, and its dorsal aspect towards the vertebral column.

If the limb in question were, as the describer of the fossil supposes, the right leg, it would obviously be impossible to place the foot in its present position, unless the numbers of the phalanges in its toes were the reverse of what is observed in Birds; that is to say, the uppermost toe, that which has three phalanges, must also be the outermost. Nevertheless the describer of the fossil justly lays great stress upon the fact that the toes have the same number of phalanges as in birds. As a matter of fact, this is quite true; but it would not be true if we were to assume with him that the limb in question is the right leg.

3. Certain parts of the fossil which lie upon the opposite side of the spine to the so-called "right leg" are named, at p. 34 of the memoir cited, "Portion of the left os innominatum, showing part of the ilium (62) and ischium (63), with the acetabulum (*a*)."

* "On the *Archæopteryx* of Von Meyer, with a description of the Fossil Remains of a Long-tailed Species, from the lithographic stone of Solenhofen." By Professor Owen, F.R.S. &c.

A full description of this mass of bone as "the left os innominatum, including the anterior two-thirds of the ilium, and the anterior half, or more, of the coalesced ischium," is given at p. 39; and at p. 40 I find, "The inferior or central* face [of the sacrum], *as in the case of the slightly dislocated left innominatum*, is towards the observer."

There is no doubt on any side, that the end of the bone in question which at present is directed forwards is its true anterior end, and that the edge which is turned towards the spinal column is the true dorsal edge. The question is, whether the face of the bone which is exposed is its outer (or dorsal) or its inner (or ventral) face. In the former case it must needs be a right ilium, in the latter a left ilium.

That it is the outer face of the bone which lies uppermost appears to me to be demonstrated—

(*a*) By the fact that the iliac margin of the acetabulum is prominent, and that the adjacent surface of this ilium rises to this margin. I am not aware that any vertebrate animal exists in which the acetabulum lies at the bottom of a funnel-shaped depression, such as would be the case in *Archæopteryx* if the bone in dispute were the left os innominatum seen from the inner side.

(*b*) By the fact that a small portion of what appears to be an innominate bone can be descried in close relation with the proximal end of what has just been shown to be the left femur; while the right femur (called left in the memoir), though dislocated, is not very far from the bone under discussion.

(*c*) By the further consideration, that if this were not the right os innominatum, it would be as curiously unlike the corresponding bone of a bird in the form of its surface as it resembles it in all other respects.

4. The bone marked 51′ is named "left scapula" (*l. c.* p. 34), and that marked 51 "right scapula" (*l. c.* p. 35); and a full description of these bones, as such, is given at pp. 36 and 37 of the memoir cited.

Nevertheless I venture to affirm that 51′ is the right scapula and not the left; for it will not be denied that the anterior or glenoidal end of the bone, as it now lies, is directed forwards, its posterior or vertebral end backwards, and its glenoidal articular surface outwards and forwards: it would be quite impossible to put a left scapula of similar construction into this position.

Further, the glenoidal end of this scapula remains in connexion with what is obviously the glenoidal (or humeral) end of the right coracoid (marked *c* in Plate I.). The author of the memoir, indeed, gives a different interpretation of the osseous projection thus marked (*l. c.* p. 37):—

"The prominence beyond the left scapula (Plate I. 51′) suggested at first view the humeral end of the coracoid, but I believe it to be part of the humerus corresponding with the tuberosity on the ulnar side of the sessile semioval head, overarching the pneumatic foramen in the bird."

* "Central" in the original. The word appears to have been substituted by an error of the press for "ventral."

And this view is pictorially embodied in the restoration of the humerus of *Archæopteryx* given in Plate II. fig. 1.

But a most distinct line of matrix separates the humerus from the prominence in question, in which may be seen, with great clearness, the glenoidal facet of the coracoid, as well as the excavation of the exterior surface of the bone which is characteristic of the glenoidal, or humeral, end of the coracoid in birds and pterodactyles.

I think, then, there can be no question that the parts marked 51′ and *c* in Plate I. of the memoir cited are the right scapula and the glenoidal end of the right coracoid, and not, as the author affirms, the left scapula and a tuberosity of the humerus.

5. Even apart from the fact that the humerus marked 53′ lies in almost undisturbed relation with the right pectoral arch, it is obviously a right humerus. On no other supposition can the relative position of the deltoid ridge and of the various contours of the bone be accounted for. Nevertheless this is called "proximal half of left humerus (53′), entire, and part of the distal half" at p. 34 of the memoir cited.

It is probably needless to pursue this part of the inquiry any further. As the so-called right leg turns out to be the left, the so-called left os innominatum the right, and the so-called left scapula and wing-bones to be those of the opposite side of the body, the necessity of a corresponding rectification for the other limb-bones needs no evidence.

6. As both the hind limbs and one-half of the pelvis have just such positions as they would readily assume if the hinder part of the animal's body lay upon its ventral face, it is highly improbable (to say the least) that the caudal and posterior trunk-vertebræ should have turned round so as to present their ventral aspect to the eye, as they do according to the memoir (*l. c.* p. 44).

But I apprehend that evidence can be found in the vertebræ themselves sufficient to prove that their dorsal and not their ventral faces are turned towards the eye. In several of the best-preserved of these vertebræ, in fact, (and Plate I. imperfectly shows this,) the remains of two small articular poocesses are distinctly visible at each end of the vertebra. The superior surface of each articular process is raised into a low longitudinal ridge; and the posterior pair of processes lie at the sides of a narrow, parallel-sided plate of bone, which projects beyond the posterior edge of the vertebra, and is received between the anterior articular processes of the vertebra which succeeds it. A low linear longitudinal elevation occupies the place of spinous process.

If my interpretation of these appearances is correct, it is clear that the caudal vertebræ (as was to be expected) turn their dorsal faces to the eye.

7. One important and extremely conspicuous bone, the furculum (if it be such), undoubtedly turns its ventral surface to the eye; and I cannot but suspect that it is the *bouleversement* of this bone which has led to that reversal of the proper nomenclature of the other bones which, could it be

sustained, would leave *Archæopteryx* without a parallel in the vertebrate subkingdom.

When the specimen of *Archæopteryx* is once put into its right position, many points of its structure acquire an intelligibility which they lose to those who accept the interpretations given in the memoir. The so-called right foot, for example, which, as a right foot, is like nothing in nature, becomes strikingly ornithic as a left foot, from the backward direction of the hallux and the apparent anchylosis of the metatarsal bones. The distal ends of the second and third metatarsals appear to me, however, to be separated for a much greater distance, proportionately to the length of the metatarsus, than in any existing birds, except the Penguins.

The femur is more slender and more curved in proportion to its length than in any recent bird with which I am acquainted. The representation of the bone in fig. 1 of Plate III. is inaccurate, as may be seen by comparing it with that given in Plate I.

The small size of the cnemial crest of the tibia is also very remarkable.

The right innominate bone is imperfectly represented in Plate I. of the memoir cited. Its anterior end is not, as it there appears to be, abruptly truncated: there is an elevation in the region which would be occupied by the prominence against which the base of the great trochanter works, and which is so characteristic of birds. The greater part of the ischium is not represented; and the sacrosciatic space certainly has not the form which it is represented to have. The references *o* to the "obturator foramen," and 63, to the "ischium" (*l. c.* p. 40), are unintelligible to me.

The ischium can be traced back for $\frac{3}{4}$ of an inch from the acetabulum; and so much of it as is preserved remains narrow throughout this extent, and is convex upwards, but concave downwards or towards the matrix.

The ventral edge of the ischium appears to be entire throughout this extent; but the posterior moiety of its dorsal edge is somewhat rough and angular. It is therefore very probable that the ischium expanded behind the sacrosciatic notch and united with the ilium, as it very generally does in carinate birds. It is very desirable that this part of the skeleton of *Archæopteryx* should be figured again.

The scapula has a distinct clavicular process, as in carinate birds; and it seems to be pretty clear that the scapula had that twofold angulation upon the coracoid which is characteristic of the *Carinatæ*.

The glenoidal end of the coracoid is unlike the corresponding part of that bone in any of the *Ratitæ*; but it is more like that of a Pterodactyle than that of any carinate bird which I have met with. It is less prominent (and the counterpart shows that this shortness is not the result of fracture) than in any recent bird, provided with a strong furculum, with which I am acquainted. In fact, in its form, and strength relatively to the shoulder-girdle, the so-called "furculum" appears to me to be the greatest osteological difficulty presented by *Archæopteryx*. I prefer waiting for the

light which will be afforded by another specimen to the indulgence of any speculation regarding this bone; in the meanwhile, I by no means wish to deny that appearances are strongly in favour of the interpretation which has been put upon it.

In conclusion, I may remark that I am unaware of the existence of any "law of correlation" which will enable us to infer that the mouth of this animal was devoid of lips, and was a toothless beak. The soft tortoises (*Trionyx*) have fleshy lips as well as horny beaks; the *Chelonia* in general have horny beaks, though they possess no feathers to preen; and *Rhamphorhynchus* combined both beak and teeth, though it was equally devoid of feathers. If, when the head of *Archæopteryx* is discovered, its jaws contain teeth, it will not the more, to my mind, cease to be a bird, than turtles cease to be reptiles because they have beaks.

All birds have a tarso-metatarsus, a pelvis, and feathers, such, in principle, as those possessed by *Archæopteryx*. No known reptile, recent or fossil, combines these three characters, or presents feathers, or possesses a completely ornithic tarsometatarsus, or pelvis. *Compsognathus* comes nearest in the tarsal region, *Megalosaurus* and *Iguanodon* in the pelvis. But, so far as the specimen enables me to judge, I am disposed to think that, in many respects, *Archæopteryx* is more remote from the boundary-line between birds and reptiles than some living *Ratitæ* are.

II. "Account of Experiments on Torsion and Flexure for the Determination of Rigidities." By JOSEPH D. EVERETT, D.C.L., Professor of Natural Philosophy in Queen's College, Belfast. Communicated by Sir WILLIAM THOMSON. Received January 13, 1868.

(Abstract.)

This paper describes a continuation of experiments related in two former papers, read February 22, 1866, and February 7, 1867,—the substances operated on in the new series being wrought iron, cast iron, and copper, and the mode of procedure being the same as in the latter of the two preceding series. The results obtained, along with those published in the former papers, are given below, the figures I., II., III. indicating the paper in which the results are deduced. The values of M, n, and k are in millions of grammes weight per square centimetre.

	M, Young's modulus.	n, Rigidity.	k, Resistance to compression.	σ, Poisson's ratio.	Specific gravity.
Glass, flint, I.	614·3	244·2	423·0	·258	2·942
Do. II. ...	585·1	239·0	353·3	·229	2·935
Brass, drawn, II....	1094·8	372·9	5701 (?)	·469	8·471
Steel, cast, II.......	2179·3	834·1	1875·6	·310	7·849
Iron, wrought, III.	1999·4	783·8	1484·1	·275	7·677
Iron, cast. III.......	1374·1	542·3	982·2	·267	7·235
Copper, drawn, III.	1255·8	455·6	1716·4	·378	8·843

February 6, 1868.

Lieut.-General SABINE, President, in the Chair.

The following communication was read :—

"Comparison of Magnetic Disturbances recorded by the Self-registering Magnetometers at the Royal Observatory, Greenwich, with Magnetic Disturbances deduced from the corresponding Terrestrial Galvanic Currents recorded by the Self-registering Galvanometers of the Royal Observatory." By GEORGE BIDDELL AIRY, Astronomer Royal. Received December 20, 1867.

(Abstract.)

The author, after adverting to the origin of this branch of science, as commencing (with himself) in communications with Messrs. Edwin and Latimer Clark, but more particularly with Mr. Charles V. Walker, and alluding to the important labours of Mr. W. H. Barlow, Mr. Walker, and Dr. Lamont, proceeds to give the official history of the establishment of the wires and other apparatus necessary for its prosecution at the Royal Observatory. In 1860 and 1861, the author submitted to the Board of Visitors of the Royal Observatory proposals for extending wires from the Royal Observatory in two directions nearly at right angles,—on the second occasion, specifying Croydon and Dartford as terminal points. The Board in 1861 recommended this to the Admiralty, who immediately gave their sanction. The author then applied to the Directors of the South-Eastern Railway for permission to place his wires on their poles, which was granted, at a merely nominal rent. All the wires and labour in mounting them were provided by the Railway Company at cost price, and the insulators were furnished by Messrs. Silver without profit. The wires communicate with the earth at both ends of each by solder-attachment to water-pipes.

The author then describes the apparatus made by Mr. Simms for the record of the currents. For each wire the current acts on a galvanometer whose needle-carrier also supports a small plane mirror; and, by proper adjustment of cylindrical lenses, neat spots of light are formed upon a rotating ebonite cylinder, covered with photographic paper and made to revolve (by clockwork) in twenty-four hours. With angular motion of the galvanometer, the spot of light moves. The zero of measure is obtained by interrupting the wire-circuit. The zero of time is obtained by interrupting the light and observing the corresponding clock-time. Other adjustments have received great attention.

Many delays occurred in the establishment of the apparatus, and finally from the discovery that the earth-currents were very much stronger than

had been anticipated. From 1865, April 18, it has been continuously in use, in the same form as at present.

The author then gives the theory, algebraical and numerical, for inferring, from the magnitude of the galvanic currents observed in two known directions, the magnitude of the galvanic currents in the north and west directions. And, proceeding from these by the known law, that when a current from the graphite or copper pole of a battery passes under a needle, it forces the austral element to the right (as referred to the current-course), he infers the magnitude of the magnetic force in the north and west directions.

The numerical expression contains four unequal constant factors, by which the photographic ordinates must be multiplied. The author explains that, for his own preliminary examination, he used four proportional compasses, constructed expressly for this purpose by Mr. Simms, and thus formed the ordinates of the new Magnetic-Force Curves without any use of numbers whatever. But for the more detailed work to be done by young assistants, he judged it better to measure the ordinates by scales with graduations of different value, and to add the results, thus forming numerical values of the magnetic ordinates.

The resulting scale being arbitrary, it was so adapted that the largest ordinates were not very different from the largest ordinates of the curves given by the Horizontal-Force Magnetometer. The curves given by the Declination Magnetometer were adapted to the scale of the Horizontal-Force Magnetometer.

In the large diagrams exhibited to the Society, the curves representing the North Force as shown by the Horizontal-Force Magnetometer, and the North Force as inferred from the Galvanometers, are brought into juxtaposition, and the curves representing the West Force as shown by the Declination Magnetometer, and as inferred from the Galvanometers, are brought into juxtaposition, for seventeen days in 1865, 1866, and 1867. And the general agreement between the curves of the two classes, especially for the North Force, is so remarkable that the author expresses his undoubting belief that the irregularities of magnetic force are caused by the galvanic currents. At the same time he indicates some discordances which require further examination. One of these is, that the disturbance inferred from the galvanic currents usually (but not always) precedes that recorded by the magnetometers. Another is, that the North Force appears, from the galvanic currents, to be increased (whereas, in magnetic storms, it is usually found to be diminished). There are other points of smaller importance.

The author suggests as possible that these discordances may arise from the circumstance that the Observatory is at the end of each of the wires; and therefore the galvanic current which is recorded, being that which covers a space whose centre is several miles from the Observatory, may not correspond to the magnetic forces which are observed at the Observatory.

And he submits for consideration whether it may not be desirable to try two shorter wires, the two ends of each wire making connexion with the earth on opposite sides of the Observatory, and the register of each being made, at the Observatory, near the middle of its length.

February 13, 1868.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "On the Mysteries of Numbers alluded to by Fermat."—Second Communication. By the Right Hon. Sir FREDERICK POLLOCK, Bart., F.R.S. Received January 14, 1868.

(Abstract.)

This paper is not adapted to be read *in extenso*; so much of it is connected with mere calculation, so much more of it requires continual reference to diagrams, that no adequate knowledge of its contents would be acquired by merely hearing it read aloud; but a statement has been prepared of what it contains which will give a general view of the result.

The properties ascribed to all odd numbers, in addition to those contained in Fermat's theorem, are these:—1st. The algebraic sum of the roots in some form of the 4 squares which compose the number will equal 1, 3, 5, 7, &c. (every odd number which it is large enough to produce); 2ndly, the difference between some 2 of the roots will be any odd or even number whatever, subject to the same limitation.

1 3 5 7 9
The series 1, 3, 7, 13, &c. ($n, n, n, n+l$) will give 1, 3, 5, &c. as the sum
2 4 6 8
of the roots of its terms; and each term is the smallest that will give that
1 3 5 7
amount. So 1, 5, 13, 25, &c. is the series whose terms are the smallest that
4 8 12 16
give the odd numbers as a difference of the roots, and 1, 3, 9, 19, &c. that
2 6 10
give the even numbers. And these are the three series that compose *The Square* (the subject of the *last* paper) when the 1st term is 1; and they are the cause of its properties. A portion of the paper is devoted to an investigation of the change effected in the sum of the squares, by a change in the roots. If 2 roots differ by n, they may be represented by a and $a+n$; and if the smaller be diminished by 1, and the larger increased by 1, the sum of the squares is increased by $2n+2$; if $n=0$, the difference is 2; and it becomes 4, 6, 8, &c. as n becomes 1, 2, 3, 4, &c. On the other hand, if the smaller root be increased and the larger diminished by 1, the sum of the squares becomes less by $2n-2$.

A similar property belongs to all polygonal numbers; in the trigonal
the increase is.............. $n+1$,
in the square it is .... $2n+2$,
in the pentagonal .... $3n+3$,
and in the m-gonal.......... $mn+m$.

When the reverse operation takes place and the sum of the squares is diminished, the + (plus) in the above expressions becomes — (minus). There are some other modes mentioned also in dealing with the roots so as to increase the sum of the squares by 2, although there be not two of the roots which are equal. A proof is offered, by means of a supplemental square decreasing as the other increases, that if every number up to $2n+1$ has the properties of odd numbers above enumerated, then the number $2n+3$ will also possess them; and if this be so, then every subsequent odd number will likewise possess them. This is a mode of proof not unfrequent in mathematical investigations: it cannot be abbreviated; but it may be useful to state that the proof chiefly arises from this, that if one term of a series corresponds with the law of it, then every term will do so, and in all the series but two there will be one term obedient to the law which renders all the rest so; the other two series are treated differently.

It is shown that if a term, in the series 1, 3, 7, &c., whose terms (represented in the roots of the 4 squares of which they are the sum) will be $n, n, n\ (n\pm1)$ be increased by 2, the roots being altered in the manner described above, the operation may be carried on till one of the terms becomes zero (0); but the next term in the series will be reached before that occurs. Then the next term may be taken as the beginning of another similar operation, and may go on till *another* term is reached, and so on without end. In this way the 4 squares into which any odd numbers may be divided will be obtained; and if every odd number is divisible into 4 squares, every even number will be so likewise.

The next subject is considered the most material and important in the paper, because it connects Lagrange's proof of the square numbers with *The Square* (the subject of the last paper). Euler thought that no assistance could be derived from the proof of Lagrange as to the other branches of the theorem (see Euler, 'Opuscula Analytica,' vol. ii. p. 4). But if every odd number is composed of 4 squares or less, then a number of the form $4n+2$ must be composed of 2, 3, or 4 squares, and in any of these cases n (any number) will be equal to 4 trigonal numbers, *which is shown in the paper.* The expression a^2+a+b^2 has been proved in a former paper of the author to be a general expression for any 2 trigonal numbers; and if any number is composed of 4 trigonal numbers or less, $a^2+a+b^2+m^2+m+n^2$ will represent any number whatever, odd or even, and $2a^2+2a+2b^2+2m^2+2m+2n^2$ will represent any even number. This connects Lagrange's proof of the squares with *The Square,* which is the subject of the last paper; and if a series be composed of squares and double trigonal numbers beginning with nothing, and having differences 2, 2, 4, 4, 6, 6,

8, 8, &c., the series will be 2, 4, 8, 12, 18, &c., and any even number will be made with some 4 terms of the series. Now *The Square,* the subject of the last paper, has a property not noticed in the former paper, viz. that the first term of *The Square,* supposing it to be of the form $\underline{4n+3}$, will be increased in descending down the principal diagonal into the sum of the squares of the roots $n, n+1, n+1, n+1$, into which the number itself may be divided; and if the form of the number be $4n+1$, a term which is the sum of the roots $n, n, n, n+1$ (into which $\underline{4n+1}$ may be divided) would appear in the diagonal next below the principal diagonal; and as every odd number is of the form of either $4n+3$, or $4n+1$, this applies to every possible odd number, and each of these numbers is a term in the series already mentioned, 1, 3, 7, 13, &c., and which may be increased by any even number by means of the series $2b^2$, $2a^2+2a$, and so on. This, it is shown in the paper, may be so altered as to correspond with the index of some number in the principal diagonal of the square, or the one below it, and will therefore ascend to the first term in *The Square,* and give the sum of its roots equal to 1; and therefore $(4n+3-1)$ divided by 2 will be composed of 3 trigonal numbers, and in the other case $(4n+1-1)$ is divided by 2; that is, every odd and every even number is composed of 3 trigonal numbers. If this be so, Fermat's theorem of the trigonal numbers is proved from the case of the squares, which (it is believed) has not been done before; but this leads to other conclusions, which are shown in the paper. If 1 be the first term of *The Square,* every term in it will have its roots of the squares that compose it of the form $+1a, a, b, b$, and the term itself will be composed of two trigonal numbers; but if each of these be made the first term of a square, every odd number will be found in some of the resulting squares; and it is shown that every odd number not only is of the form $1+2a^2+2a+2b^2+2m^2+2m+2n^2$, but also of the form $1+2a^2+2a+2b^2+2m^2+2m$, or $1+2a^2+2a+2b^2+2n^2$; so that, with respect to every odd number, two of the squares that compose it may be equal, and also two may have their roots differing by 1.

There remains one other matter to be mentioned, viz. a certain remarkable relation which all the polygonal numbers bear to each other, and which forms a connexion that runs through them all, from which it would seem to follow that a solution of the theorem as to one would be a solution as to all the rest (except the first).

This relation arises in the square numbers by a property of the gradation series, already in part alluded to, viz., as to the odd numbers, by which the interval between any two terms can be filled up, all the terms having, as to the odd numbers, the sum of the roots of the squares that compose them equal to the sum of the roots of the first term; but the intervals, as to the *even* numbers, may be also filled up by making the sum of the roots one less than that of the roots of the odd numbers (see the Table in Diagram No. 3, which is thus constructed). A term in the gradation series is assumed (in this case 73); its roots are 4, 4, 4, 5; and the roots of all the odd numbers

between that and the next term are found by the processes mentioned in the former part of this paper. The roots of the even numbers are then obtained by an analogous process; and these are used as bases or roots of the polygonal numbers, which are placed in columns, with their sums, as appears in the Table (see Diagram No. 4 for the mode in which the polygonal numbers are formed).

It will be observed that the sum of the roots or bases is 17; but if they be used to form trigonal numbers, the increment of the sum of the resulting trigonal numbers above the sum of the roots or bases is 28, and so on of the rest, each successive column increasing by the same number, viz. 28. If the roots or bases be $n, n, n, n \pm 1$ (that is, a term in the gradation series), the increment of the sums of the successive columns will be $2n^2 \mp n$, a trigonal number.

Again, in the trigonal numbers the difference between the sums of the first and second term is 0; in the square numbers it is 1; in the pentagonal numbers 2; in the hexagonal numbers 3; in the heptagonal numbers 4; but in all of them the difference between the second and third terms is 1, and this continues throughout. The difference between the third and fourth, the fifth and sixth, the seventh and eighth, &c., increases by 1 in each column; but the difference between the second and third, the fourth and fifth, the sixth and seventh, &c., is always 1 in each column; and the result is that, by adding 1 in the pentagonal column, by adding 1, or 1.1 in the hexagonal, by adding 1, or 1.1, or 1.1.1 in the heptagonal, every number, odd or even, can be made by not exceeding four square numbers, or five pentagonal numbers, or, &c., as clearly appears by the Table.

This corresponds with what was discovered by Cauchy, published at the end of Legendre's 'Théorie des Nombres,' viz. that four only of each class of numbers is necessary; the rest may be supplied by 1 repeated as often as necessary. But I must not omit to say that, although all the odd numbers are sufficiently obedient, there is one class of even numbers quite refractory, viz. the powers of 2. They may be easily expressed in squares, pentagonal numbers, &c., but they cannot be brought within the rule that otherwise prevails.

II. "Compounds Isomeric with the Sulphocyanic Ethers.—I. On the Mustard Oil of the Ethyl Series." By A. W. Hofmann, LL.D., F.R.S.

The results of my researches on the chloroform-derivatives of the primary monamines, which, as I have shown, are isomeric with the nitriles, could not fail to direct my attention to allied groups of bodies, with the view of discovering similar isomerisms.

In a note communicated to the Royal Society some months ago, I expressed the expectations which even then appeared to be justified in the following manner:—"In conclusion, I may be permitted to announce as

very probable the existence of a series of bodies isomeric with the sulphocyanides. Already M. Cloëz has shown that the action of chloride of cyanogen on ethylate of potassium gives rise to the formation of an ethylic cyanate possessing properties absolutely different from those belonging to the cyanate discovered by M. Wurtz. On comparing, on the other hand, the properties of the methylic and ethylic sulphocyanides with those of the sulphocyanides of allyl and phenyl, it can scarcely be doubted that we have here the representatives of two groups entirely different, and that the terms of the methyl- and ethyl-series which correspond to oil of mustard, and to the sulphocyanide of phenyl, still remain to be discovered. Experiments with which I am now engaged will show whether these bodies cannot be obtained by the action of the iodides of methyl and ethyl on sulphocyanide of silver."

These experiments I have since concluded, but the hopes which I expressed have not been realized. Dry sulphocyanide of silver is much less easily attacked by the alcohol-iodides than the cyanide. The mixture, in consequence of the formation of iodide of silver, rapidly turns yellow, but the reaction is not completed without protracted digestion in the water-bath; on submitting the product of the reaction to distillation, the well-known sulphocyanic ethers, discovered by M. Cahours, are obtained. The experiment was performed both in the ethyl- and the amyl-series; the ethers thus produced, when compared with the compounds prepared by distilling sulphocyanide of potassium with sulphethylates or sulphamylates, exhibited the same odour, the same boiling-point, and in general the same chemical deportment.

The failure of these experiments could not, however, shake my belief in the existence of two series of bodies of the composition of the sulphocyanic ethers. It was only necessary to find the method of producing the new isomers.

I was fortunate enough almost at the very outset to trace the right track, and I beg leave to submit to the Royal Society some of the facts established even now by my experiments. These experiments are intimately connected with some of my older observations.

More than twenty years ago, when studying the action of bisulphide of carbon on aniline, I discovered a finely crystallized body, which in succession has been designated as *Sulphocarbanilide, Diphenylsulphocarbamide,* and *sulphuretted diphenylurea.* About ten years later*, this compound again passed through my hands. I then found that, when submitted to the action of anhydrous phosphoric acid, sulphocarbanilide was converted by the loss of 1 mol. of aniline into sulphocyanide of phenyl. The phenyl-compound has the peculiar pungent odour of the ethereal oil of black mustard; like the latter it possesses the faculty of fixing the ammonias—so much so, indeed, that I did not hesitate to describe the new compound as the mustard oil of the phenyl series.

* Proc. of the Roy. Soc. vol. ix. p. 274.

I almost wonder now that the experiments performed in the phenyl series, and soon afterwards also in the naphthyl series, were not even then extended to the ethylic compounds and their homologues—the more so since the study of the action of bisulphide of carbon upon amylamine and ethylamine had, so to say, supplied me with the material for the inquiry. When contemplated in the light of the recent observations, these experiments acquired an increased interest; for I could no longer doubt that the reaction, which had yielded me the sulphocyanide of phenyl, when appropriately applied to the derivatives of methylic, ethylic, and amylic alcohols, would put me into possession of the compounds isomeric with the sulphocyanic ethers which I was anxious to procure. Experiment has not failed to confirm my anticipations.

Experiments in the Ethyl series.

The general character of the action of bisulphide of carbon upon ethylamine I had examined when more minutely investigating the behaviour of amylamine under the influence of the bisulphide*. I have resumed the inquiry, which has furnished me the following results:—

On adding bisulphide of carbon to an alcoholic solution of ethylamine, the liquid becomes more or less heated, according to the concentration of the solution. The liquid turns neutral, and yields on evaporation an oily compound, which after some time solidifies into a mass of splendid tabular crystals. This compound fuses at 103°, and then retains the liquid condition even when cooled to the ordinary temperature. When gently heated, the salt is volatilized, partly, at all events, without decomposition. These crystals are the *ethylamine-salt of ethyl-sulphocarbamic acid.*

$$2\left[\left.\begin{matrix}C_2H_5\\H_2\end{matrix}\right\}N\right]+CS_2=\left.\begin{matrix}[(CS)''\ (C_2H_5)\ HN]\\ [C_2H_5)\ H_2N]\ H\end{matrix}\right\}S.$$

The salt is readily soluble both in water and in alcohol. Addition of soda disengages ethylamine, giving rise to the formation of ethylsulphocarbamate of sodium. Hydrochloric acid decomposes the salt with separation of the acid, which collects in oily drops on the surface of the liquid, gradually solidifying to a mass of fatty crystals. Excess of hydrochloric acid dissolves these crystals, bisulphide of carbon being evolved, and a salt of ethylamine remaining behind.

Under the protracted influence of heat ethylsulphocarbamate of ethylamine is thoroughly decomposed.

Even at the temperature of boiling water, torrents of sulphuretted hydrogen are disengaged; the transformation is rapidly accomplished when the alcoholic solution is heated under pressure to 110° or 120°. On evaporating the alcoholic liquid after the evolution of sulphuretted hydrogen has ceased, an oily liquid remains behind, which also crystallizes after some time. These crystals fuse at 77°; they are likewise soluble in alcohol,

* Proc. of the Roy. Soc. vol. ix. p. 591 (1860).

but differ from the ethylsulphocarbamate by their insolubility in water. Hydrochloric acid dissolves them; the solution yields, with perchloride of platinum, a light-yellow precipitate. The new substance is *diethylsulphocarbamide* or *sulphuretted diethylurea*, the formation of which is represented by the following equation:—

$$\left.\begin{matrix}[(CS)''\,(C_2H_5)\,HN]\\ [(C_2H_5)\,H_2N],\,H\end{matrix}\right\}S=\left.\begin{matrix}H\\ H\end{matrix}\right\}S+\left.\begin{matrix}(CS)''\\ (C_2H_5)_2\\ H_2\end{matrix}\right\}N_2.$$

On gently heating a mixture of diethylsulphocarbamide with anhydrous phosphoric acid, pungent vapours are evolved, which are condensed to a yellowish liquid possessing in a remarkable manner the odour of mustard oil. When rectified, this liquid becomes colourless; it boils constantly at 134°, and has the same composition as the sulphocyanide of ethyl which is formed by the action of a metallic sulphocyanide upon sulphethylate of potassium. The new substance is formed by the separation from the diethylsulphocarbamide of one molecule of ethylamine, which unites with the phosphoric acid.

$$\left.\begin{matrix}(CS)''\\ (C_2H_5)_2\\ H_2\end{matrix}\right\}N_2=\left.\begin{matrix}(C_2H_5)\\ H_2\end{matrix}\right\}N+\left.\begin{matrix}(CS)''\\ (C_2H_5)\end{matrix}\right\}N.$$

In its properties, the new compound essentially differs from the known sulphocyanic ethylether. The boiling-point of the latter is 147°; the new substance therefore boils 13° lower than the old one. The powerfully irritating odour of the new ether is absolutely different from that of the ordinary sulphocyanic ether, which, though by no means agreeable, exerts no marked action either upon nose or eyes.

By far the most characteristic feature of the new compound, however, is the facility with which it acts upon ammonia and its derivatives. Dissolved in alcoholic ammonia and digested for a few hours at 100°, the ether is converted into *ethylsulphocarbamide* or *sulphuretted ethyl-urea.*

$$\left.\begin{matrix}(CS)''\\ (C_2H_5)\end{matrix}\right\}N+H_3N=\left.\begin{matrix}(CS)''\\ (C_2H_5)H\\ H_2\end{matrix}\right\}N_2;$$

with methylamine a mixed urea is formed,

$$\left.\begin{matrix}(CS)''\\ (C_2H_5)\end{matrix}\right\}N+\left.\begin{matrix}(CH_3)\\ H\\ H\end{matrix}\right\}N=\left.\begin{matrix}(CH_3)\,(CS)''\\ (C_2H_5)\\ H_2\end{matrix}\right\}N_2.$$

Ethylamine produces the diethylated compound which has served in the preparation of the ether; aniline, lastly, gives rise to the formation of a mixed urea of the fatty and aromatic series,

$$\left.\begin{matrix}(CS)''\\ (C_2H_5)\end{matrix}\right\}N+\left.\begin{matrix}C_6H_5\\ H\\ H\end{matrix}\right\}N=\left.\begin{matrix}(C_6H_5)\,(CS)''\\ (C_2H_5)\\ H_2\end{matrix}\right\}N_2.$$

All these diamines are very crystalline; they are weak bases which dissolve in acids, and furnish, with perchloride of platinum, yellow crystalline precipitates.

The faculty of combining with the ammonias, it will be remembered, is altogether deficient in the ordinary sulphocyanic ethers. On the other hand, it belongs to sulphocyanide of allyl, or mustard oil. In fact the new compound is in the ethyl-series what mustard oil is in the allyl-series. I have on this occasion again perused the beautiful memoir of Professor Will on mustard oil, the indications of which have served me as a guide in my experiments. So far as these experiments go, the parallelism of the ethyl- and allyl-body is complete.

For the present I must be satisfied to have indicated the formation and the principal properties of the new compound isomeric with sulphocyanide of ethyl.

In a subsequent paper I propose to communicate to the Royal Society the results of a comparative study to which I have submitted the old and the new sulphocyanide, together with the conclusions elicited by these researches as to the different atomic construction of the two substances.

In conclusion, I may be permitted to state that methylamine and amylamine, when subjected to the same treatment, have furnished me the analogous mustard terms of the methyl- and amyl-group; the properties of these substances I have not yet more minutely investigated.

February 20, 1868.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

Mr. Baldwin Francis Duppa was admitted into the Society.

The following communications were read:—

I. "Sur l'Origine de l'Électrotone des Nerfs." Par M. Ch. Matteucci. Communicated by Dr. Beale, F.R.S.

Depuis mes premiers travaux sur la fonction électrique de la torpille, et sur le courant musculaire de la grenouille et des animaux à sang chaud, qui datent depuis 1837, et que la Société Royale voulut quelque temps après encourager avec sa plus haute distinction, je n'ai jamais cessé de m'acquitter de la dette que j'avais ainsi contracté envers cet illustre corps scientifique, en lui communiquant les recherches successives tentées sur les mêmes sujets, et dont le but constant a été de démontrer les analogies qui existent entre des lois physiques et chimiques connues et les phénomènes électro-physiologiques. Tel est aussi le but de cette communication.

Un fait remarquable d'électro-physiologie est la propriété dont parmi les

tissus organiques paraît presque exclusivement doué le nerf, d'être parcouru en dehors des électrodes d'une pile par un courant continu qui dure pendant que le circuit voltaïque est fermé, dans le même sens de celui-ci. Pour bien étudier les lois de ce phénomène, il fallait les reproduire, comme je l'ai fait, outre que sur les nerfs de la grenouille, sur ceux des animaux supérieurs et à sang chaud, tels que lapin, chiens, poulet, et brebis. C'est avec ces nerfs qu'on voit le courant appelé "de l'électrotone" acquérir une plus grande intensité, se produire à une plus grande distance des points touchés par les électrodes de la pile, et avoir lieu encore d'une manière très-sensible lorsque le nerf a perdu tout-à-fait ses propriétés vitales. De même sur ces gros nerfs on obtient facilement un phénomène très-important pour la théorie, c'est-à-dire, la persistance du courant de l'électrotone après qu'on a fait cesser le courant de la pile, pourvu que ce courant eût une certaine intensité, et le passage eût été plus prolongé.

Des phénomènes de cette nature faisaient déjà entrevoir que si le courant de l'électrotone est nécessairement lié à la structure du nerf, il ne l'est pas avec ses propriétés vitales, mais qu'il dépend plutôt de quelques effets, physiques ou chimiques, produits dans les nerfs par le passage du courant voltaïque.

Je crois en effet d'avoir bien démontré que ces effets sont les produits de l'électrolysation qui se recueillent sur les points du nerf touchés par les électrodes de la pile, et des courants ou polarités secondaires développés par les réactions successives de ces produits.

Je dois ici ajouter d'avoir répété encore tout dernièrement dans mes cours, l'expérience de la ligature et de la section du nerf pour reconnaître l'influence de ces actions sur le courant de l'électrotone. En opérant sur les nerfs sciatiques du lapin et du poulet on acquiert la certitude qu'un courant de l'électrotone, qui donne des déviations fixes de 36° à un galvanomètre de 24,000 tours, diminue à 13° et s'y fixe après la ligature.

Sur un autre nerf la diminution qui se produit après la section laisse l'aiguille déviée à 8°.

Il me restait à rechercher de reproduire le courant de l'électrotone sur des fils métalliques choisis et placés dans les conditions les plus favorables pour le développement des courants secondaires. Dans ce but j'ai pris deux fils de métal très-minces de platine et de zinc, et ce dernier, je l'ai amalgamé : ces deux fils ont été ensuite enveloppés d'une couche de fil de coton, couche qui a été également imbibée d'une solution neutre de sulphate de zinc. On sait que les polarités secondaires qui se développent très-fortes et très-rapidement sur le fil de platine, ne se développent pas sur le zinc. Il est facile de disposer l'expérience de l'électrotone de manière à opérer d'abord avec un de ces fils et puis avec l'autre. Le résultat de cette comparaison est net constant. Avec le fil de platine préparé comme je l'ai dit, on a le courant de l'électrotone très-fort même à 1 mètre de distance des électrodes de la pile, *tandis qu'avec le fil de zinc, le courant de l'électrotone on ne*

l'obtient pas, quelque petite que soit la distance entre la pile et le galvanomètre.

Il est donc bien prouvé que les polarités secondaires sont les causes de ces courants de l'électrotone, et il est d'ailleurs facile de s'assurer par les papiers réactifs, de l'existence et de la diffusion très-rapide des produits de l'électrolysation sur le fil de platine de manière à expliquer ces courants.

Il est donc logique de voir entre ces courants de l'électrotone du fil de platine et celui des nerfs, une analogie fondée et qui s'accorde avec les causes qui détruisent cette propriété dans les nerfs, et qui sont celles qui altèrent sa structure, telles que la compression, la coagulation, et la ligature. Et à ce propos je puis ajouter que la ligature et la section sur le fil de platine préparé comme je l'ai dit, agissent dans le même sens sur le courant de l'électrotone comme sur le nerf; et on voit manifestement sur le fil de platine préparé, que l'altération consiste principalement dans la solution de continuité qu'on crée ainsi dans la couche humide externe qui enveloppe ce fil métallique.

Je demande la permission à la S. R. d'ajouter encore quelques résultats d'une recherche qui m'occupe maintenant, et qui est tentée dans la même direction que mes recherches précédentes, c'est-à-dire, dans le but de déterminer quels sont les changements chimiques dans les nerfs et dans les muscles de la grenouille, soumis au passage continu du courant électrique.

On conçoit facilement la manière d'opérer; c'est de former en quelque sorte des électrodes avec des grenouilles préparées et dont les extrémités plongent dans l'eau de puits contenue dans deux cylindres poreux qui plongent également dans le même liquide. On fait passer le courant de 8 à 10 couples de Daniell pendant plusieurs heures à travers ces grenouilles et l'eau, et puis on analyse les deux liquides des porcelaines. Un résultat constant est, que les muscles des grenouilles ainsi électrolysés donnent une réaction alcaline beaucoup plus intense que les mêmes muscles laissés à l'air, et les muscles qui communiquent au pôle positif aussi bien que le liquide où ces muscles plongent sont généralement plus chargés d'alcali que les muscles et le liquide du pôle negatif.

Un autre fait aussi constant et remarquable c'est la grande différence dans la quantité d'albumine qu'on trouve dissoute dans les deux liquides. Dans cinq expériences faites dans des circonstances égales, tandis que les liquides où plongeaient les muscles en communication avec le pôle positif, montraient à peine quelques traces d'albumine; l'autre liquide dont les grenouilles communiquaient avec le pôle negatif, contenait une quantité abondante d'albumine. Dans d'autres séries d'expériences qui sont à peine commencées, j'analyse l'air resté en contact avec des grenouilles, les unes ayant les nerfs parcourus par le courant direct, les autres les nerfs parcourus par le courant inverse. Dans ces cas encore, on trouve des différences constantes, et qui correspondent à ces effets électro-physiologiques différents. Je me suis aussi beaucoup occupé de la relation qu'il y a entre

le pouvoir électro-moteur des muscles et des actes chimiques de la respiration musculaire. Cette relation est mise hors de doute par des expériences rigoureuses et très-variées.

J'espère pouvoir plus tard communiquer à la S. R. la suite de ces trauaux.

II. "On the Resistance of the Air to the Motion of Elongated Projectiles having variously formed Heads." By the Rev. F. Bashforth, B.D., Professor of Applied Mathematics to the Advanced Class of Artillery Officers, Woolwich. Communicated by Professor Stokes, Sec. R.S. Received January 30, 1868.

(Abstract.)

These experiments were undertaken with a view to determine the resistance of the air to some forms of heads of elongated shot which were likely to be of practical use. The chronograph used was the one described in the Proceedings of the Royal Artillery Institution for August 1866*, which was constructed on the plan of the Greenwich instrument. Ten screens were placed in a line at intervals of 150 feet, the first being 75 feet from the gun. The following were the forms of the heads, and ten shot of each kind were prepared:—

(1)	Hemispherical	solid.
(2)	Hemispheroidal (axes as 1 : 2)	solid.
(3)	Ogival (struck with a radius = 1 diameter)..	solid.
(4)	Ogival (struck with a radius = 2 diameters)..	solid.
(5)	Ogival (1 diameter)	hollow.
(6)	Ogival (2 diameters)	hollow.

(3) and (5) as well as (4) and (6) had respectively the same external forms, but the solid were nearly double the weight of the hollow shot. The gun used was a 40-pounder M.L., and the diameter of the shot was 4·7 inches.

It was found, as in the trial experiments of 1865, that, if s be the space described in time t after passing the first screen, then, approximately,

$$t=as+bs^2,$$

from which it follows that, if v be the velocity at time t,

$$v=\frac{1}{a+2bs},$$

and the retarding force

$$=-2bv^3.$$

If V denote the velocity when $s=0$, then

$$V=\frac{1}{a},$$

* Published separately by Bell and Daldy, 1866.

and
$$v=\frac{1}{\frac{1}{\mathrm{V}}+2bs}.$$

All the hollow shot were fired, giving eighteen out of twenty successful shots. Only a part of the solid shot prepared were fired; and they did not give nearly such good results as the hollow shot, probably in consequence of the superior angular velocity of the hollow shot—because, as a 5-lb. charge was used throughout, the lighter shot had a higher initial velocity, and consequently a higher corresponding angular velocity.

Tables are given showing for every round:—(1) the experimental determination of the time of passing each screen, supposing the first screen to be passed when $t=0$; (2) the velocities at the middle points between successive screens; (3) the weights of the shot; and (4) the numerical values of bl^2, where $l=150$ feet, the distance between the screens. And *assuming* that, for a given form of head, the resistance of the air varies as the square of the diameter, the mean values of $2b$ have been adopted for shot weighing W lbs., and having a diameter of d inches, or 2R feet.

When a body is moving in a straight line under the action of a force which varies as the cube of the velocity, it appears that the actual velocity v' at the middle of any space $2s'$ is such that, if another body moved over the same space $2s'$ with a *uniform* velocity v', it would describe it in the same time as the first-named body. For the time t' would

$$=\frac{1}{\mathrm{V}}\,2s'+b(2s')^2,$$

uniform velocity

$$=\frac{2s'}{t'}=\frac{2s'}{\frac{1}{\mathrm{V}}2s'+b(2s')^2}=\frac{1}{\frac{1}{\mathrm{V}}+2bs'}=v',$$

the actual velocity at the distance s'.

M. Hélie, in his 'Traité de Balistique' (1865), adopted, for elongated projectiles, a law for the resistance of the air which varied as the velocity cubed. The law was deduced from some experiments made at Gâvre, when a great number of velocities (v', v'') of shot fired with various charges were measured at two points x metres apart. The mean values of v' and v'' were taken and substituted in the formula $\dfrac{v''-v'}{v''v'x}$; and it was found that this was approximately constant, and consequently that the resistance varied as the (velocity)^3. The French measures and weights have been converted into English measures for M. Hélie's best experiment, in order to facilitate comparisons with my own experiments. The contents of M. Hélie's work were quite unknown to me for several months after my report on the above experiments had been given in. For an ogival-headed shot struck with a radius of two diameters M. Hélie's value of $2b$ is

$$2b=\cdot000036\frac{\mathrm{R}^2}{\mathrm{W}}=\cdot000000062\frac{d^2}{\mathrm{W}},$$

while my experiments for the same form of head, but with much higher velocities, give

$$2b = \cdot 000060 \frac{R^2}{W} = \cdot 000000104 \frac{d^2}{W}.$$

There is reason to expect that my value of b will require a small reduction for the low velocities used in M. Hélie's experiments; but it is extremely improbable that it can be reduced to M. Hélie's value. It will thus appear that M. Hélie and I agree in adopting a law of the resistance of the air, but that we have followed quite independent methods in experimenting, and have arrived at different numerical results.

February 27, 1868.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "On the Resistance of the Air to Rifled Projectiles." By J. A. Longridge, C.E. Communicated by C. Manby, Esq. Received February 13, 1868.

(Abstract.)

The introduction of elongated rifled projectiles having rendered it necessary to reconsider the laws of resistance which had been deduced by Robins, Hutton, and more recent authors, such an investigation is the object of this paper.

It is first shown that Hutton's law,

$$R = av + bv^2,$$

if applied to the results obtained by the Special Armstrong and Whitworth Committee, 1866, leads to the following equation,

$$x = 1620 \log_{10} \left\{ \frac{V - 1015 \cdot 4}{v - 1015 \cdot 4} \right\},$$

where V is the initial velocity,

v the residual velocity at the distance x from the gun.

In like manner it is shown that the law adopted by Piobert,

$$R = Av^2 + Bv^3,$$

leads to the equation

$$x = 2197 \log_{10} \left\{ \frac{V - 994}{v - 994} \cdot \frac{v}{V} \right\},$$

and the law

$$R = Av^2 + Bv^4$$

to the equation

$$x = 2668 \log_{10} \left\{ \frac{V^2 - 958850}{v^2 - 958850} \cdot \frac{v^2}{V^2} \right\}.$$

These equations all fail by x becoming infinite when $v=1015$, 994, and 979 respectively.

It is, however, observed that, in the assumption of the law of the resistance, the higher the power of velocity the longer does the corresponding equation give rational results; and by assuming $R=av^p$ with the same data, the following equation was obtained,

$$x=\frac{\log^{-1} 23{\cdot}618}{V^{8\cdot747}}\left\{\left(\frac{V}{v}\right)^{8\cdot747}-1\right\},$$

which gives consistent results for all values of v.

The value of p here is 8·747, which would give the resistance varying nearly as the ninth power of the velocity.

This result led the author of the paper to doubt the accuracy of the experiments, and to seek for further and more correct data, which were obtained from a minute (No. 23,351) of the Ordnance Select Committee, dated 21st September 1867, containing the results of experiments showing the loss of velocity of two projectiles, one of 8·818 lbs., and the other of 251 lbs., in passing through certain given distances with given initial velocities, varying from about 1500 feet to 600 feet per second.

From these results a diagram was constructed, and for each projectile an equation was found which agreed tolerably well with the experimental results.

The form of the equation assumed was

$$(x+a)\,v^n=C\,;$$

and the resulting equation was for the small shot

$$(x+665)\,v^{2\cdot4}=\log^{-1} 10{\cdot}1473853,$$

and for the large shot

$$(x+2032)\,v^3=\log^{-1} 12{\cdot}6696158,$$

the maximum error being about $1\frac{1}{2}$ per cent. of the velocity.

Introducing into these equations the diameter and weight of the respective projectiles, and taking the index $n=2{\cdot}5$, the values of C were found to be,

$$\text{small shot,}\quad C=\log^{-1} 10{\cdot}7295585\,\frac{W}{d^2},$$

$$\text{large shot,}\quad C=\log^{-1} 10{\cdot}7454405\,\frac{W}{d^2},$$

$$\text{the mean being } C=\log^{-1} 10{\cdot}7375745\,\frac{W}{d^2},$$

and the resulting general equation

$$\left(x+\frac{\log^{-1} 10{\cdot}7375745\,.\,W}{d^2\,V^{2\cdot5}}\right)\frac{d^2\,.\,v^{2\cdot5}}{W}=\log^{-1} 10{\cdot}7375745.$$

The maximum error in velocity, as calculated by this formula, was for the small shot $1\frac{1}{2}$ per cent., and for the large shot $2\frac{1}{3}$ per cent.

From the above equation the resistance per square inch of sectional area is found,

$$R=\frac{v^{4\cdot5}}{\log^{-1} 13\cdot0154756},$$

from which the following Table is constructed, the third column showing the resistance, as calculated by Hutton's formula :—

Table of Resistances to a Rifled Projectile.

Velocity, feet per second.	Resistance, in lbs., per square inch.	Hutton, p. 218.	Velocity, feet per second.	Resistance, in lbs., per square inch.	Hutton, p. 218.
1500	18·89	18·94	700	0·613	3.12
1400	13·87	16·23	600	0·306	2·20
1300	9·94	13·67	500	0·135	1·49
1200	6·92	11·29	400	0·0494	0·93
1100	3·722	9·14	300	0·01354	0·52
1000	3·052	7·24	200	0·00218	0·23
900	1·900	5·61	100	0·0000965	0·556
800	1·118	4·24			

It is next shown that the hypothesis of the great increase of resistance at velocities exceeding 1100 feet per second being due to the vacuum behind the projectile is untenable, because the actual resistance at 1300 feet per second is only 9·94 lbs. per square inch, whilst, according to that hypothesis, the back resistance alone would be 15 lbs. per square inch.

It is suggested that the true reason of the great increase of resistance may be found in the fact that a wave-impulse cannot be propagated at a greater velocity than 1100 feet per second, and that consequently a great condensation of air must take place in front of the projectile at all velocities exceeding this, and the resisting force of such condensed air will increase at a greater rate than indicated by Mariotte's law, owing to the evolution of heat due to the condensation.

A comparison is then instituted between the resistances as ascertained by the above law and those given by Hutton's formula.

It is stated that in experiments made on May 17th, 1867, the small shot weighing 8·8 lbs., moving with a mean velocity of 986 feet per second, lost 58½ feet of velocity in a distance of 900 feet.

The time of flight being ·96 of a second, the resisting force must have been nearly twice the weight of the shot, or more accurately 17·2 lbs.

Now, according to the formula given in this paper, the resistance is found to be 17·75 lbs., whilst Hutton's formula gives a resistance of 46½ lbs.

Having thus obtained a law which gives, with considerable accuracy, the residual velocity at any point of the flight, the corresponding equation to the trajectory is deduced for low degrees of elevation when the length of

the arc differs very slightly from the horizontal distance, or $ds=dx$ nearly; and the following is the resulting equation :—

$$y=x\tan\phi+\mathrm{A}\left\{\frac{n}{2(n+1)}a^{\frac{2(n+1)}{n}}+a^{\frac{n+2}{n}}x-\frac{n}{2(n+1)}(x+a)^{\frac{2(n+1)}{n}}\right\},$$

where $\mathrm{A}=\frac{g}{2\mathrm{C}^{\frac{2}{n}}}\cdot\frac{n}{n+2}$, and c and a are the constants, and n the index in the general equation

$$(x+a)\,v^{n}=\mathrm{C}.$$

Examples of the application of this are given, showing the calculated elevation for the 12-pounder muzzle-loading Armstrong gun for ranges of 2855 yards and 4719 yards, the gun being 17 feet above the planes.

The calculated elevations were 6° 56′ and 14° 6′, the actual elevations being 7° and 15° respectively.

It is not intended to claim more than approximate accuracy for the formulæ in this paper. The general formula has been shown to be derived by taking mean values of n and c, whereas the actual results would indicate that the value of n increases with the diameter of the projectile; and it is shown in a note that the values of n which agree best with experiment are,

for the small shot $n=2{\cdot}4$,
for the large shot $n=4$,

corresponding to the following resistances,

small shot $\mathrm{R}=v^{4\cdot4}$,
large shot $\mathrm{R}=v^{6}$.

Whether in reality the index does increase with the diameter of the shot must be left to be determined by more extended experiments; meantime it may be assumed that the general formula in this paper represents with tolerable accuracy the law of resistance and the loss of velocity of projectiles varying from 8·8 lbs. to 251 lbs. in weight, from 3 inches to 9 inches in diameter, and from 1500 to 600 feet per second in velocity.

II. "On the Theory of Probability, applied to Random Straight Lines." By M. W. Crofton, B.A., of the Royal Military Academy, Woolwich, late Professor of Natural Philosophy in the Queen's University, Ireland. Communicated by Prof. Sylvester. Received February 5, 1868.

(Abstract.)

This paper relates to the Theory of Local Probability—that is, the application of Probability to geometrical magnitude. This inquiry seems to have been originated by the great naturalist Buffon, in a celebrated problem proposed and solved by him. Though the subject has been more than once touched upon by Laplace, yet the remarkable depth and beauty of this new Calculus seem to have been little suspected till within the last

few years, when the attention of several English mathematicians has been directed to it, and results of a most singular character have been obtained.

The problems on Local Probability which have been hitherto treated relate almost exclusively to *points* taken at random. The object of the present paper is to show how the Theory of Probability is to be applied to *straight lines* whose position is unknown, or, in other words, which are taken at random.

The author commences by showing that when a straight line is drawn at random in an indefinite plane, or, in other words, when we take one out of an infinite assemblage of lines all drawn at random in the plane, the true mathematical conception of this assemblage is as follows:—

Conceive the plane ruled with an infinity of parallels at a constant infinitesimal distance (δp) asunder; then imagine this system of parallels turned through an infinitesimal angle ($\delta\theta$); then through a second equal angle, and so on, till the parallels return to their original direction; the plane will thus be covered with an infinite number of systems of parallels, running in every possible direction.

If an infinite plane be covered in this manner with straight lines, and we draw any closed convex contour on the plane, and then imagine all the lines effaced from the plane, except those which meet this contour, we shall have a clear conception of the system of random lines which meet the given contour.

By applying mathematical calculation to this system, the following important principle is proved:—

The measure of the number of random lines which meet a given closed convex contour is L, *the length of the contour.*

If the contour be non-convex, or be not closed, the measure will be *the length of an endless string passing round it and tightly enveloping it.*

Hence, given any closed convex contour of length L, and any other of length l, lying wholly within the former, the probability that a line drawn at random to meet L shall also meet l, is

$$P=\frac{l}{L}.$$

The following propositions are then established:—

If the contour l lie wholly *outside* L, then, if X be the length of an endless band tightly enveloping the two contours and crossing between them, and Y the length of another endless band also enveloping both, but not crossing between them, the probability that a random line meeting L shall also meet l, is

$$p=\frac{X-Y}{L}.$$

Again, if the contour l should intersect L (whether in two or more points), then, if Y be an endless band tightly enveloping both,

$$p=\frac{L+l-Y}{L}.$$

A closed convex boundary of any form, of length L, encloses an area Ω: if two random straight lines intersect it, the probability of their intersection lying within it is

$$p=\frac{2\pi\Omega}{L^2}.$$

The probability of their intersection lying within any given area, ω, which is enclosed within Ω, is

$$p=\frac{2\pi\omega}{L^2}.$$

A more difficult question would be to determine the probability in the case where ω is external to Ω.

These fundamental results, it will be observed, are of great generality. The author proceeds to apply them to the solution of various problems relating to random straight lines; in fact any such problem of probability may be reduced by the principles above laid down to a question of pure mathematical calculation.

What will probably be considered among the most curious results contained in this paper are the collateral applications of the theory to the integral calculus. Several integrals of a singular character are obtained, some of which it seems very difficult to prove by any known method. One or two of these are subjoined, with indications of the methods used in establishing their truth.

If a given convex boundary be intersected by a system of random lines, as above described, every pair of lines will meet in a point; and these points of intersection will be scattered all over the plane, some within the boundary, some without. Those within will evidently be distributed with uniform density over the area; but it becomes a question for those outside, to determine the law according to which their density varies; and it is proved in this paper that *the density of the intersections of a system of random lines crossing a given area, for any external point* P, *is proportional to* $\theta-\sin\theta$, *where* θ *is the apparent angular magnitude of the area from* P.

Hence the number of external intersections is represented by

$$\iint(\theta-\sin\theta)\,dS;$$

now the number of internal will be $\pi\Omega$, and the whole number $\frac{1}{2}L^2$. Hence

If Ω *be any plane area, enclosed by a convex boundary of length* L, *and* θ *the angle which it subtends at any external point* (x, y), then

$$\iint(\theta-\sin\theta)\,dxdy=\tfrac{1}{2}L^2-\pi\Omega,$$

the integral extending over the whole external surface of the plane.*

By conceiving an infinite system of random lines covering an infinite

* This theorem has appeared in the 'Comptes Rendus' of the French Academy of Sciences (Dec. 1867).

plane, and a second system, all of which meet a given boundary in that plane, and then fixing our attention on the intersections of the former system with the latter, we find the density here proportional to θ; and the following theorem is deduced from this consideration:—

Given any convex boundary (whose apparent magnitude is called θ), let there be an external boundary surrounding it, such that any tangent to the inner cuts off a constant area from the outer, then

$$\iint \theta dx dy = \pi D,$$

the integral extending over the whole annulus between them, D *being the difference of the areas of the parts into which the annulus is divided by any tangent to the inner.*

For instance, we may take two similar concentric ellipses. If both the inner and outer boundaries are of any convex forms whatever, the above expression is still true, provided D mean *the average value of the difference of areas as the tangent revolves by uniform angular displacements.*

If we consider a plane covered with random lines, and then divide them into two systems, one crossing a given boundary, the other all outside it, the density of the points in which the former system cut the latter will be proportional to $\sin \theta$; and this leads to the next theorem.

If an endless string (of length Y*) be passed round a given convex boundary (of length* L*), and the string be kept stretched by the point of a pencil, which thus traces out an external boundary, then if θ be the apparent magnitude of the given boundary at any point* (x, y), we shall have

$$\iint \sin \theta dx dy = L(Y - L),$$

the integral extending over the annular space between the boundaries.

A remarkable instance of this is an ellipse, the outer curve being, as is well known, a confocal ellipse.

Some other applications of the theory to integration are then given. It is important to notice that these applications, though having arisen from researches on probability, rest on a basis wholly independent of that theory. The apparatus of equidistant parallels revolving by infinitesimal angular displacements, which has been here employed, is a purely geometrical conception; and the proofs of these integrals can be presented in a strict mathematical form. A reticulation composed of two systems of parallels crossing at a finite angle has already been employed by Cauchy, Liouville, and Eisenstein as a method in the theory of numbers and elliptic functions. The reticulation used above is a more delicate and complicated one, consisting, not of two, but of an infinite number of systems of parallels.

There remains a more difficult but deeply interesting inquiry, scarcely touched upon in this paper—namely, the extension of the above results to the cases of straight lines, and of planes, taken at random in space.

March 5, 1868.

JOHN PETER GASSIOT, Esq., V.P., in the Chair.

In accordance with the Statutes, the names of the Candidates for election into the Society were read as follows:—

Alexander Armstrong, M.D.
John Baily, Esq., Q.C.
John Ball, Esq., M.A.
Henry Charlton Bastian, M.D.
Samuel Brown, Esq.
Lieut.-Colonel John Cameron, R.E.
Charles Chambers, Esq.
Frederic Le Gros Clark, Esq.
Robert Bellamy Clifton, Esq., M.A.
George Critchett, Esq.
Morgan William Crofton, Esq., B.A.
Herbert Davies, M.D.
Joseph Barnard Davis, M.D.
Henry Dircks, Esq.
P. Martin Duncan, M.D.
William Esson, Esq., M.A.
Alexander Fleming, M.D.
George Carey Foster, Esq., B.A.
Peter Le Neve Foster, Esq., M.A.
Sir Charles Fox.
Edward Headlam Greenhow, M.D.
Peter Griess, Esq.
Augustus George Vernon Harcourt, Esq.
Edmund Thomas Higgins, Esq.
William Charles Hood, M.D.
George Johnson, M.D.
Rear-Admiral Astley Cooper Key, C.B.
David Macloughlin, M.D.
George Matthey, Esq.
St. George Mivart, Esq.
Edward Chambers Nicholson, Esq.
Thomas Nunneley, Esq.
Rear-Admiral Erasmus Ommaney, C.B.
Captain Sherard Osborn, R.N., C.B.
Rev. Stephen Parkinson, B.D.
James Bell Pettigrew, M.D.
Charles Bland Radcliffe, M.D.
John Russell Reynolds, M.D.
Vice-Admiral Robert Spencer Robinson.
Edward Henry Sieveking, M.D.
Edward James Stone, Esq., M.A.
Colonel Henry Edward Landor Thuillier, R.A.
Rev. Henry Baker Tristram, M.A.
Edward Burnet Tylor, Esq.
William Sandys Wright Vaux, Esq., M.A.
Augustus Voelcker, Esq., Ph.D.
Edward Walker, Esq., M.A.
George Charles Wallich, M.D.
J. Alfred Wanklyn, Esq.
Edward John Waring, M.D.
Henry Wilde, Esq.
Samuel Wilks, M.D.
Henry Worms, Esq.

The following communications were read:—

I. "On Governors." By J. Clerk Maxwell, M.A., F.R.SS.L. & E. Received Feb. 20, 1868.

A Governor is a part of a machine by means of which the velocity of the machine is kept nearly uniform, notwithstanding variations in the driving-power or the resistance.

Most governors depend on the centrifugal force of a piece connected with a shaft of the machine. When the velocity increases, this force increases, and either increases the pressure of the piece against a surface or moves the piece, and so acts on a break or a valve.

In one class of regulators of machinery, which we may call *moderators* *, the resistance is increased by a quantity depending on the velocity. Thus in some pieces of clockwork the moderator consists of a conical pendulum revolving within a circular case. When the velocity increases, the ball of the pendulum presses against the inside of the case, and the friction checks the increase of velocity.

In Watt's governor for steam-engines the arms open outwards, and so contract the aperture of the steam-valve.

In a water-break invented by Professor J. Thomson, when the velocity is increased, water is centrifugally pumped up, and overflows with a great velocity, and the work is spent in lifting and communicating this velocity to the water.

In all these contrivances an increase of driving-power produces an increase of velocity, though a much smaller increase than would be produced without the moderator.

But if the part acted on by centrifugal force, instead of acting directly on the machine, sets in motion a contrivance which continually increases the resistance as long as the velocity is above its normal value, and reverses its action when the velocity is below that value, the governor will bring the velocity to the same normal value whatever variation (within the working limits of the machine) be made in the driving-power or the resistance.

I propose at present, without entering into any details of mechanism, to direct the attention of engineers and mathematicians to the dynamical theory of such governors.

It will be seen that the motion of a machine with its governor consists in general of a uniform motion, combined with a disturbance which may be expressed as the sum of several component motions. These components may be of four different kinds :—

1. The disturbance may continually increase.
2. It may continually diminish.
3. It may be an oscillation of continually increasing amplitude.
4. It may be an oscillation of continually decreasing amplitude.

The first and third cases are evidently inconsistent with the stability of the motion ; and the second and fourth alone are admissible in a good governor. This condition is mathematically equivalent to the condition that all the possible roots, and all the possible parts of the impossible roots, of a certain equation shall be negative.

I have not been able completely to determine these conditions for equa-

* See Mr. C. W. Siemens "On Uniform Rotation," Phil. Trans. 1866, p. 657.

tions of a higher degree than the third; but I hope that the subject will obtain the attention of mathematicians.

The actual motions corresponding to these impossible roots are not generally taken notice of by the inventors of such machines, who naturally confine their attention to the way in which it is *designed* to act; and this is generally expressed by the real root of the equation. If, by altering the adjustments of the machine, its governing power is continually increased, there is generally a limit at which the disturbance, instead of subsiding more rapidly, becomes an oscillating and jerking motion, increasing in violence till it reaches the limit of action of the governor. This takes place when the possible part of one of the impossible roots becomes positive. The mathematical investigation of the motion may be rendered practically useful by pointing out the remedy for these disturbances.

This has been actually done in the case of a governor constructed by Mr. Fleeming Jenkin, with adjustments, by which the regulating power of the governor could be altered. By altering these adjustments the regulation could be made more and more rapid, till at last a dancing motion of the governor, accompanied with a jerking motion of the main shaft, showed that an alteration had taken place among the impossible roots of the equation.

I shall consider three kinds of governors, corresponding to the three kinds of moderators already referred to.

In the first kind, the centrifugal piece has a constant distance from the axis of motion, but its pressure on a surface on which it rubs varies when the velocity varies. In the *moderator* this friction is itself the retarding force. In the *governor* this surface is made moveable about the axis, and the friction tends to move it; and this motion is made to act on a break to retard the machine. A constant force acts on the moveable wheel in the opposite direction to that of the friction, which takes off the break when the friction is less than a given quantity.

Mr. Jenkin's governor is on this principle. It has the advantage that the centrifugal piece does not change its position, and that its pressure is always the same function of the velocity. It has the disadvantage that the normal velocity depends in some degree on the coefficient of sliding friction between two surfaces which cannot be kept always in the same condition.

In the second kind of governor, the centrifugal piece is free to move further from the axis, but is restrained by a force the intensity of which varies with the position of the centrifugal piece in such a way that, if the velocity of rotation has the normal value, the centrifugal piece will be in equilibrium in every position. If the velocity is greater or less than the normal velocity, the centrifugal piece will fly out or fall in without any limit except the limits of motion of the piece. But a break is arranged so that it is made more or less powerful according to the distance of the centrifugal piece from the axis, and thus the oscillations of the centrifugal piece are restrained within narrow limits.

Governors have been constructed on this principle by Sir W. Thomson and by M. Foucault. In the first, the force restraining the centrifugal piece is that of a spring acting between a point of the centrifugal piece and a fixed point at a considerable distance, and the break is a friction-break worked by the reaction of the spring on the fixed point.

In M. Foucault's arrangement, the force acting on the centrifugal piece is the weight of the balls acting downward, and an upward force produced by weights acting on a combination of levers and tending to raise the balls. The resultant vertical force on the balls is proportional to their depth below the centre of motion, which ensures a constant normal velocity. The break is:—in the first place, the variable friction between the combination of levers and the ring on the shaft on which the force is made to act; and, in the second place, a centrifugal air-fan through which more or less air is allowed to pass, according to the position of the levers. Both these causes tend to regulate the velocity according to the same law.

The governors designed by the Astronomer Royal on Mr. Siemens's principle for the chronograph and equatorial of Greenwich Observatory depend on nearly similar conditions. The centrifugal piece is here a long conical pendulum, not far removed from the vertical, and it is prevented from deviating much from a fixed angle by the driving-force being rendered nearly constant by means of a differential system. The break of the pendulum consists of a fan which dips into a liquid more or less, according to the angle of the pendulum with the vertical. The break of the principal shaft is worked by the differential apparatus; and the smoothness of motion of the principal shaft is ensured by connecting it with a fly-wheel.

In the third kind of governor a liquid is pumped up and thrown out over the sides of a revolving cup. In the governor on this principle, described by Mr. C. W. Siemens, the cup is connected with its axis by a screw and a spring, in such a way that if the axis gets ahead of the cup the cup is lowered and more liquid is pumped up. If this adjustment can be made perfect, the normal velocity of the cup will remain the same through a considerable range of driving-power.

It appears from the investigations that the oscillations in the motion must be checked by some force resisting the motion of oscillation. This may be done in some cases by connecting the oscillating body with a body hanging in a viscous liquid, so that the oscillations cause the body to rise and fall in the liquid.

To check the variations of motion in a revolving shaft, a vessel filled with viscous liquid may be attached to the shaft. It will have no effect on uniform rotation, but will check periodic alterations of speed.

Similar effects are produced by the viscosity of the lubricating matter in the sliding parts of the machine, and by other unavoidable resistances; so that it is not always necessary to introduce special contrivances to check oscillations.

I shall call all such resistances, if approximately proportional to the velocity, by the name of "viscosity," whatever be their true origin.

In several contrivances a differential system of wheelwork is introduced between the machine and the governor, so that the driving-power acting on the governor is nearly constant.

I have pointed out that, under certain conditions, the sudden disturbances of the machine do not act through the differential system on the governor, or *vice versâ.* When these conditions are fulfilled, the equations of motion are not only simple, but the motion itself is not liable to disturbances depending on the mutual action of the machine and the governor.

Distinction between Moderators and Governors.

In regulators of the first kind, let P be the driving-power and R the resistance, both estimated as if applied to a given axis of the machine. Let V be the normal velocity, estimated for the same axis, and $\frac{dx}{dt}$ the actual velocity, and let M be the moment of inertia of the whole machine reduced to the given axis.

Let the governor be so arranged as to increase the resistance or diminish the driving-power by a quantity $F\left(\frac{dx}{dt}-V\right)$, then the equation of motion will be

$$\frac{d}{dt}\left(M\frac{dx}{dt}\right)=P-R-F\left(\frac{dx}{dt}-V\right). \quad . \quad . \quad . \quad . \quad . \quad (1)$$

When the machine has obtained its final rate the first term vanishes, and

$$\frac{dx}{dt}=V+\frac{P-R}{F}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Hence, if P is increased or R diminished, the velocity will be permanently increased. Regulators of this kind, as Mr. Siemens * has observed, should be called moderators rather than governors.

In the second kind of regulator, the force $F\left(\frac{dx}{dt}-V\right)$, instead of being applied directly to the machine, is applied to an independent moving piece, B, which continually increases the resistance, or diminishes the driving-power, by a quantity depending on the whole motion of B.

If y represents the whole motion of B, the equation of motion of B is

$$\frac{d}{dt}\left(B\frac{dy}{dt}\right)=F\left(\frac{dx}{dt}-V\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and that of M

$$\frac{d}{dt}\left(M\frac{dx}{dt}\right)=P-R-F\left(\frac{dx}{dt}-V\right)+Gy, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where G is the resistance applied by B when B moves through one unit of space.

* "On Uniform Rotation," Phil. Trans. 1866. p. 657.

We can integrate the first of these equations at once, and we find

$$B\frac{dy}{dt}=F\,(x-Vt)\,; \quad \ldots\ldots\ldots\ldots \quad (5)$$

so that if the governor B has come to rest $x=Vt$, and not only is the velocity of the machine equal to the normal velocity, but the position of the machine is the same as if no disturbance of the driving-power or resistance had taken place.

Jenkin's Governor.—In a governor of this kind, invented by Mr. Fleeming Jenkin, and used in electrical experiments, a centrifugal piece revolves on the principal axis, and is kept always at a constant angle by an appendage which slides on the edge of a loose wheel, B, which works on the same axis. The pressure on the edge of this wheel would be proportional to the square of the velocity; but a constant portion of this pressure is taken off by a spring which acts on the centrifugal piece. The force acting on B to turn it round is therefore

$$F^1\left|\frac{dx}{dt}\right|^2-C^1\,;$$

and if we remember that the velocity varies within very narrow limits, we may write the expression

$$F\left(\frac{dx}{dt}-V_1\right),$$

where F is a new constant, and V_1 is the lowest limit of velocity within which the governor will act.

Since this force necessarily acts on B in the positive direction, and since it is necessary that the break should be taken off as well as put on, a weight W is applied to B, tending to turn it in the negative direction; and, for a reason to be afterwards explained, this weight is made to hang in a viscous liquid, so as to bring it to rest quickly.

The equation of motion of B may then be written

$$B\frac{d^2y}{dt^2}=F\left(\frac{dx}{dt}-V_1\right)-Y\frac{dy}{dt}-W, \quad \ldots\ldots \quad (6)$$

where Y is a coefficient depending on the viscosity of the liquid and on other resistances varying with the velocity, and W is the constant weight.

Integrating this equation with respect to t, we find

$$B\frac{dy}{dt}=F\,(x-V_1t)-Yy-Wt. \quad \ldots\ldots \quad (7)$$

If B has come to rest, we have

$$x=\left(V_1+\frac{W}{F}\right)t+\frac{Y}{F}y, \quad \ldots\ldots\ldots \quad (8)$$

or the position of the machine is affected by that of the governor, but the final velocity is constant, and

$$V_1+\frac{W}{F}=V, \quad \ldots\ldots\ldots\ldots \quad (9)$$

where V is the normal velocity.

The equation of motion of the machine itself is

$$M\frac{d^2x}{dt^2}=P-R-F\left(\frac{dx}{dt}-V_1\right)-Gy. \quad \ldots \ldots \quad (10)$$

This must be combined with equation (7) to determine the motion of the whole apparatus. The solution is of the form

$$x=A_1e^{n_1t}+A_2e^{n_2t}+A_3e^{n_3t}+Vt, \quad \ldots \ldots \quad (11)$$

where n_1, n_2, n_3 are the roots of the cubic equation

$$MBn^3+(MY+FB)n^2+FYn+FG=0. \quad \ldots \quad (12)$$

If n be a pair of roots of this equation of the form $a\pm\sqrt{-1}b$, then the part of x corresponding to these roots will be of the form

$$e^{at}\cos(bt+\beta).$$

If a is a negative quantity, this will indicate an oscillation the amplitude of which continually decreases. If a is zero, the amplitude will remain constant, and if a is positive, the amplitude will continually increase.

One root of the equation (12) is evidently a real negative quantity. The condition that the real part of the other roots should be negative is

$$\left(\frac{F}{M}+\frac{Y}{B}\right)\frac{Y}{B}-\frac{G}{B}=\text{a positive quantity.}$$

This is the condition of stability of the motion. If it is not fulfilled there will be a dancing motion of the governor, which will increase till it is as great as the limits of motion of the governor. To ensure this stability, the value of Y must be made sufficiently great, as compared with G, by placing the weight W in a viscous liquid if the viscosity of the lubricating materials at the axle is not sufficient.

To determine the value of F, put the break out of gear, and fix the moveable wheel; then, if V and V′ be the velocities when the driving-power is P and P′,

$$F=\frac{P-P'}{V-V'}.$$

To determine G, let the governor act, and let y and y' be the positions of the break when the driving-power is P and P′, then

$$G=\frac{P-P'}{y-y'}.$$

General Theory of Chronometric Centrifugal Pieces.

Sir W. Thomson's and M. Foucault's Governors.—Let A be the moment of inertia of a revolving apparatus, and θ the angle of revolution. The equation of motion is

$$\frac{d}{dt}\left(A\frac{d\theta}{dt}\right)=L, \quad \ldots \ldots \quad (1)$$

where L is the moment of the applied force round the axis.

Now, let A be a function of another variable ϕ (the divergence of the centrifugal piece), and let the kinetic energy of the whole be

$$\frac{1}{2}\mathrm{A}\left.\overline{\frac{d\theta}{dt}}\right|^2+\frac{1}{2}\mathrm{B}\left.\overline{\frac{d\phi}{dt}}\right|^2,$$

where B may also be a function of ϕ, if the centrifugal piece is complex.

If we also assume that P, the potential energy of the apparatus, is a function of ϕ, then the force tending to *diminish* ϕ, arising from the action of gravity, springs, &c., will be $\frac{d\mathrm{P}}{d\phi}$.

The whole energy, kinetic and potential, is

$$\mathrm{E}=\frac{1}{2}\mathrm{A}\left.\overline{\frac{d\theta}{dt}}\right|^2+\frac{1}{2}\mathrm{B}\left.\overline{\frac{d\phi}{dt}}\right|^2+\mathrm{P}=\int \mathrm{L}\,d\theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Differentiating with respect to t, we find

$$\left.\begin{aligned}\frac{d\phi}{dt}\left(\frac{1}{2}\frac{d\mathrm{A}}{d\phi}\left.\overline{\frac{d\theta}{dt}}\right|^2+\frac{1}{2}\frac{d\mathrm{B}}{d\phi}\left.\overline{\frac{d\phi}{dt}}\right|^2+\frac{d\mathrm{P}}{d\phi}\right)+\mathrm{A}\frac{d\theta}{dt}\frac{d^2\theta}{dt^2}+\mathrm{B}\frac{d\phi}{dt}\frac{d^2\phi}{dt^2}\\ =\mathrm{L}\frac{d\theta}{dt}=\frac{d\theta}{dt}\left(\frac{d\mathrm{A}}{d\phi}\frac{d\theta}{dt}\frac{d\phi}{dt}+\mathrm{A}\frac{d^2\theta}{dt^2}\right),\end{aligned}\right\} \quad . \quad . \quad (3)$$

whence we have, by eliminating L,

$$\frac{d}{dt}\left(\mathrm{B}\frac{d\phi}{dt}\right)=\frac{1}{2}\frac{d\mathrm{A}}{d\phi}\overline{\frac{d\theta}{dt}}^2+\frac{1}{2}\frac{d\mathrm{B}}{d\phi}\left.\overline{\frac{d\phi}{dt}}\right|^2-\frac{d\mathrm{P}}{d\phi}. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The first two terms on the right-hand side indicate a force tending to *increase* ϕ, depending on the squares of the velocities of the main shaft and of the centrifugal piece. The force indicated by these terms may be called the centrifugal force.

If the apparatus is so arranged that

$$\mathrm{P}=\tfrac{1}{2}\mathrm{A}\omega^2+\text{const.}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where ω is a constant velocity, the equation becomes

$$\frac{d}{dt}\left(\mathrm{B}\frac{d\phi}{dt}\right)=\frac{1}{2}\frac{d\mathrm{A}}{d\phi}\left(\left.\overline{\frac{d\theta}{dt}}\right|^2-\omega^2\right)+\frac{1}{2}\frac{d\mathrm{B}}{d\phi}\left.\overline{\frac{d\phi}{dt}}\right|^2. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

In this case the value of ϕ cannot remain constant unless the angular velocity is equal to ω.

A shaft with a centrifugal piece arranged on this principle has only one velocity of rotation without disturbance. If there be a small disturbance, the equations for the disturbances θ and ϕ may be written

$$\mathrm{A}\frac{d^2\theta}{dt^2}+\frac{d\mathrm{A}}{d\phi}\omega\frac{d\phi}{dt}=\mathrm{L}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$\mathrm{B}\frac{d^2\phi}{dt^2}-\frac{d\mathrm{A}}{d\phi}\omega\frac{d\theta}{dt}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The period of such small disturbances is $\frac{d\mathrm{A}}{d\phi}(\mathrm{AB})^{-\frac{1}{2}}$ revolutions of the

shaft. They will neither increase nor diminish if there are no other terms in the equations.

To convert this apparatus into a governor, let us assume viscosities X and Y in the motions of the main shaft and the centrifugal piece, and a resistance $G\phi$ applied to the main shaft. Putting $\frac{dA}{d\phi}\omega=K$, the equations become

$$A\frac{d^2\theta}{dt^2}+X\frac{d\theta}{dt}+K\frac{d\phi}{dt}+G\phi=L, \quad \ldots \ldots \quad (9)$$

$$B\frac{d^2\phi}{dt^2}+Y\frac{d\phi}{dt}-K\frac{d\theta}{dt} \qquad =0. \quad \ldots \ldots \quad (10)$$

The condition of stability of the motion indicated by these equations is that all the possible roots, or parts of roots, of the cubic equation

$$ABn^3+(AY+BX)n^2+(XY+K^2)n+GK=0 \quad \ldots \quad (11)$$

shall be negative; and this condition is

$$\left(\frac{X}{A}+\frac{Y}{B}\right)(XY+K^2)>GK. \quad \ldots \ldots \quad (12)$$

Combination of Governors.—If the break of Thomson's governor is applied to a moveable wheel, as in Jenkin's governor, and if this wheel works a steam-valve, or a more powerful break, we have to consider the motion of three pieces. Without entering into the calculation of the general equations of motion of these pieces, we may confine ourselves to the case of small disturbances, and write the equations

$$\left.\begin{aligned} &A\frac{d^2\theta}{dt^2}+X\frac{d\theta}{dt}+K\frac{d\phi}{dt}+T\phi+J\psi=P-R,\\ &B\frac{d^2\phi}{dt^2}+Y\frac{d\phi}{dt}-K\frac{d\theta}{dt} \qquad =0,\\ &C\frac{d^2\psi}{dt^2}+Z\frac{d\psi}{dt}-T\phi \qquad =0, \end{aligned}\right\} \quad \ldots \quad (13)$$

where θ, ϕ, ψ are the angles of disturbance of the main shaft, the centrifugal arm, and the moveable wheel respectively, A, B, C their moments of inertia, X, Y, Z the viscosity of their connexions, K is what was formerly denoted by $\frac{dA}{d\phi}\omega$, and T and J are the powers of Thomson's and Jenkin's breaks respectively.

The resulting equation in n is of the form

$$\begin{vmatrix} An^2+Xn & Kn+T & J \\ -K & Bn+Y & 0 \\ 0 & -T & Cn^2+Zn \end{vmatrix}=0, \quad \ldots \quad (14)$$

or

$$\left.\begin{aligned} &n^5+n^4\left(\frac{X}{A}+\frac{Y}{B}+\frac{Z}{C}\right)+n^3\left[\frac{XYZ}{ABC}\left(\frac{A}{X}+\frac{B}{Y}+\frac{C}{Z}\right)+\frac{K^2}{AB}\right]\\ &+n^2\left(\frac{XYZ+KTC+K^2Z}{ABC}\right)+n\frac{KTZ}{ABC}+\frac{KTJ}{ABC}=0. \end{aligned}\right\} \quad \ldots \quad (15)$$

I have not succeeded in determining completely the conditions of stability of the motion from this equation; but I have found two necessary conditions, which are in fact the conditions of stability of the two governors taken separately. If we write the equation

$$n^5+pn^4+qn^3+rn^2+sn+t, \quad \ldots \quad (16)$$

then, in order that the possible parts of all the roots shall be negative, it is necessary that

$$pq>r \text{ and } ps>t. \quad \ldots \quad (17)$$

I am not able to show that these conditions are sufficient. This compound governor has been constructed and used.

On the Motion of a Liquid in a Tube revolving about a Vertical Axis.

Mr. C. W. Siemens's Liquid Governor.—Let ρ be the density of the fluid, k the section of the tube at a point whose distance from the origin measured along the tube is s, r, θ, z the coordinates of this point referred to axes fixed with respect to the tube, Q the volume of liquid which passes through any section in unit of time. Also let the following integrals, taken over the whole tube, be

$$\int \rho k r^2 ds = \mathrm{A}, \quad \int \rho r^2 d\theta = \mathrm{B}, \quad \int \rho \frac{1}{u} ds = \mathrm{C}, \quad \ldots \quad (1)$$

the lower end of the tube being in the axis of motion.

Let ϕ be the angle of position of the tube about the vertical axis, then the moment of momentum of the liquid in the tube is

$$\mathrm{H}=\mathrm{A}\frac{d\phi}{dt}+\mathrm{BQ}. \quad \ldots \quad (2)$$

The moment of momentum of the liquid thrown out of the tube in unit of time is

$$\frac{d\mathrm{H}'}{dt} = \rho r^2 \mathrm{Q}\frac{d\phi}{dt} + \rho\frac{r}{k}\mathrm{Q}^2\cos\alpha, \quad \ldots \quad (3)$$

where r is the radius at the orifice, k its section, and α the angle between the direction of the tube there and the direction of motion.

The energy of motion of the fluid in the tube is

$$\mathrm{W}=\tfrac{1}{2}\mathrm{A}\left(\frac{d\phi}{dt}\right)^2 + \mathrm{BQ}\frac{d\phi}{dt}+\tfrac{1}{2}\mathrm{CQ}^2. \quad \ldots \quad (4)$$

The energy of the fluid which escapes in unit of time is

$$\frac{d\mathrm{W}'}{dt}=\rho g\mathrm{Q}(h+z)+\tfrac{1}{2}\rho r^2\mathrm{Q}\left(\frac{d\phi}{dt}\right)^2+\rho\frac{r}{k}\cos\alpha\,\mathrm{Q}^2\frac{d\phi}{dt}+\tfrac{1}{2}\frac{\rho}{k^2}\mathrm{Q}^3. \quad \ldots \quad (5)$$

The work done by the prime mover in turning the shaft in unit of time is

$$\mathrm{L}\frac{d\phi}{dt}=\frac{d\phi}{dt}\left(\frac{d\mathrm{H}}{dt}+\frac{d\mathrm{H}'}{dt}\right). \quad \ldots \quad (6)$$

The work spent on the liquid in unit of time is

$$\frac{d\mathrm{W}}{dt}+\frac{d\mathrm{W}'}{dt}.$$

Equating this to the work done, we obtain the equations of motion

$$A\frac{d^2\phi}{dt^2}+B\frac{dQ}{dt}+\rho r^2 Q\frac{d\phi}{dt}+\rho\frac{r}{k}\cos\alpha Q^2=L, \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$B\frac{d^2\phi}{dt^2}+C\frac{dQ}{dt}+\tfrac{1}{2}\frac{\rho}{k^2}Q^2+\rho g(h+z)-\tfrac{1}{2}\rho r^2\overline{\frac{d\phi}{dt}}\Big|^2=0. \quad . \quad . \quad . \quad . \quad (8)$$

These equations apply to a tube of given section throughout. If the fluid is in open channels, the values of A and C will depend on the depth to which the channels are filled at each point, and that of k will depend on the depth at the overflow.

In the governor described by Mr. C. W. Siemens in the paper already referred to, the discharge is practically limited by the depth of the fluid at the brim of the cup.

The resultant force at the brim is $f=\sqrt{g^2+\omega^4 r^2}$.

If the brim is perfectly horizontal, the overflow will be proportional to $x^{\frac{3}{2}}$ (where x is the depth at the brim), and the mean square of the velocity relative to the brim will be proportional to x, or to $Q^{\frac{2}{3}}$.

If the breadth of overflow at the surface is proportional to x^n, where x is the height above the lowest point of overflow, then Q will vary as $x^{n+\frac{3}{2}}$, and the mean square of the velocity of overflow relative to the cup as x or as $\frac{1}{Q^{n+\frac{3}{2}}}$.

If $n=-\frac{1}{2}$, then the overflow and the mean square of the velocity are both proportional to x.

From the second equation we find for the mean square of velocity

$$\frac{Q^2}{k^2}=-\frac{2}{\rho}\left(B\frac{d^2\phi}{dt^2}+C\frac{dQ}{dt}\right)+r^2\overline{\frac{d\phi}{dt}}\Big|^2-2g(h+r). \quad . \quad . \quad . \quad (9)$$

If the velocity of rotation and of overflow is constant, this becomes

$$\frac{Q^2}{k^2}=r^2\overline{\frac{d\phi}{dt}}\Big|^2-2g(h+r). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

From the first equation, supposing, as in Mr. Siemens's construction, that $\cos\alpha=0$ and $B=0$, we find

$$L=\rho r^2 Q\frac{d\phi}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

In Mr. Siemens's governor there is an arrangement by which a fixed relation is established between L and z,

$$L=-Sz, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

whence

$$\frac{Q^2}{k^2}=r^2\overline{\frac{d\phi}{dt}}\Big|^2-2gh+\frac{2g\rho}{S}r^2Q\frac{d\phi}{dt}. \quad . \quad . \quad . \quad . \quad . \quad (13)$$

If the conditions of overflow can be so arranged that the mean square of the velocity, represented by $\frac{Q^2}{k^2}$, is proportional to Q, and if the strength of

the spring which determines S is also arranged so that

$$\frac{Q^2}{k^2}=\frac{2g\rho}{S}r^2\omega Q, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

the equation will become, if $2gh=\omega^2r^2$,

$$0=r^2\left(\left|\frac{d\phi}{dt}\right|^2-\omega^2\right)+\frac{2g\rho}{S}r^2Q\left(\frac{d\phi}{dt}-\omega\right), \quad . \quad . \quad . \quad . \quad . \quad (15)$$

which shows that the velocity of rotation and of overflow cannot be constant unless the velocity of rotation is ω.

The condition about the overflow is probably difficult to obtain accurately in practice; but very good results have been obtained within a considerable range of driving-power by a proper adjustment of the spring. If the rim is uniform, there will be a *maximum* velocity for a certain driving-power. This seems to be verified by the results given at p. 667 of Mr. Siemens's paper.

If the flow of the fluid were limited by a hole, there would be a *minimum* velocity instead of a maximum.

The differential equation which determines the nature of small disturbances is in general of the fourth order, but may be reduced to the third by a proper choice of the value of the mean overflow.

Theory of Differential Gearing.

In some contrivances the main shaft is connected with the governor by a wheel or system of wheels which are capable of rotation round an axis, which is itself also capable of rotation about the axis of the main shaft. These two axes may be at right angles, as in the ordinary system of differential bevel wheels; or they may be parallel, as in several contrivances adapted to clockwork.

Let ξ and η represent the angular position about each of these axes respectively, θ that of the main shaft, and ϕ that of the governor; then θ and ϕ are linear functions of ξ and η, and the motion of any point of the system can be expressed in terms either of ξ and η or of θ and ϕ.

Let the velocity of a particle whose mass is m resolved in the direction of x be

$$\frac{dx}{dt}=p_1\frac{d\xi}{dt}+q_1\frac{d\eta}{dt}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

with similar expressions for the other coordinate directions, putting suffixes 2 and 3 to denote the values of p and q for these directions. Then Lagrange's equation of motion becomes

$$\Xi\delta\xi+\mathrm{H}\delta\eta-\Sigma m\left(\frac{d^2x}{dt^2}\delta x+\frac{d^2y}{dt^2}\delta y+\frac{d^2z}{dt^2}\delta z\right)=0, \quad . \quad . \quad (2)$$

where Ξ and H are the forces tending to increase ξ and η respectively, no force being supposed to be applied at any other point.

Now putting

$$\delta x=p_1\,\delta\xi+q_1\,\delta\eta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and

$$\frac{d^2x}{dt^2}=p_1\frac{d^2\xi}{dt^2}+q_1\frac{d^2\eta}{dt^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

the equation becomes

$$\left(\Xi-\Sigma mp^2\frac{d^2\xi}{dt^2}-\Sigma mpq\frac{d^2\eta}{dt^2}\right)\delta\xi+\left(\mathrm{H}-\Sigma mpq\frac{d^2\xi}{dt^2}-\Sigma mq^2\frac{d^2\eta}{dt^2}\right)\delta\eta=0\,; \quad (5)$$

and since $\delta\xi$ and $\delta\eta$ are independent, the coefficient of each must be zero.

If we now put

$$\Sigma(mp^2)=\mathrm{L},\quad \Sigma(mpq)=\mathrm{M},\quad \Sigma(mq^2)=\mathrm{N},\quad .\ .\ .\ .\ . \quad (6)$$

where

$$p^2=p_1^2+p_2^2+p_3^2,\ pq=p_1q_1+p_2q_2+p_3q_3,\ \text{and}\ q^2=q_1^2+q_2^2+q_3^2,$$

the equations of motion will be

$$\Xi=\mathrm{L}\frac{d^2\xi}{dt^2}+\mathrm{M}\frac{d^2\eta}{dt^2},\ .\ .\ .\ .\ .\ .\ .\ .\ . \quad (7)$$

$$\mathrm{H}=\mathrm{M}\frac{d^2\xi}{dt^2}+\mathrm{N}\frac{d^2\eta}{dt^2}.\ .\ .\ .\ .\ .\ .\ .\ . \quad (8)$$

If the apparatus is so arranged that $\mathrm{M}=0$, then the two motions will be independent of each other; and the motions indicated by ξ and η will be about conjugate axes—that is, about axes such that the rotation round one of them does not tend to produce a force about the other.

Now let Θ be the driving-power of the shaft on the differential system, and Φ that of the differential system on the governor; then the equation of motion becomes

$$\Theta\delta\theta+\Phi\delta\phi+\left(\Xi-\mathrm{L}\frac{d^2\xi}{dt^2}-\mathrm{M}\frac{d^2\eta}{dt^2}\right)\delta\xi+\left(\mathrm{H}-\mathrm{M}\frac{d^2\xi}{dt^2}-\mathrm{N}\frac{d^2\eta}{dt^2}\right)\delta\eta=0\,; \quad (9)$$

and if

$$\left.\begin{aligned}\delta\xi&=\mathrm{P}\delta\theta+\mathrm{Q}\delta\phi,\\ \delta\eta&=\mathrm{R}\delta\theta+\mathrm{S}\delta\phi,\end{aligned}\right\}\ .\ .\ .\ .\ .\ .\ .\ .\ . \quad (10)$$

and if we put

$$\left.\begin{aligned}\mathrm{L}'&=\mathrm{LP}^2+2\mathrm{MPR}+\mathrm{NR}^2,\\ \mathrm{M}'&=\mathrm{LPQ}+\mathrm{M}(\mathrm{PS}+\mathrm{QR})+\mathrm{NRS},\\ \mathrm{N}'&=\mathrm{LQ}^2+2\mathrm{MQS}+\mathrm{NS}^2,\end{aligned}\right\}\ .\ .\ . \quad (11)$$

the equations of motion in θ and ϕ will be

$$\left.\begin{aligned}\Theta+\mathrm{P}\,\Xi+\mathrm{QH}&=\mathrm{L}'\frac{d^2\theta}{dt^2}+\mathrm{M}'\frac{d^2\phi}{dt^2},\\ \Phi+\mathrm{R}\,\Xi+\mathrm{SH}&=\mathrm{M}'\frac{d^2\theta}{dt^2}+\mathrm{N}'\frac{d^2\phi}{dt^2}.\end{aligned}\right\}\ .\ .\ . \quad (12)$$

If $\mathrm{M}'=0$, then the motions in θ and ϕ will be independent of each other. If M is also 0, then we have the relation

$$\mathrm{LPQ}+\mathrm{NRS}=0\,;\ .\ .\ .\ .\ .\ .\ . \quad (13)$$

and if this is fulfilled, the disturbances of the motion in θ will have no effect on the motion in ϕ. The teeth of the differential system in gear with the main shaft and the governor respectively will then correspond to the centres of percussion and rotation of a simple body, and this relation will be mutual.

In such differential systems a constant force, H, sufficient to keep the governor in a proper state of efficiency, is applied to the axis η, and the motion of this axis is made to work a valve or a break on the main shaft of the machine. Ξ in this case is merely the friction about the axis of ξ. If the moments of inertia of the different parts of the system are so arranged that $M'=0$, then the disturbance produced by a blow or a jerk on the machine will act instantaneously on the valve, but will not communicate any impulse to the governor.

II. "Proceedings of the Council of the Royal Society with reference to the undertaking of certain Physical Observations in India." Communicated to the Society by direction of the President.

On the 13th of February 1866, J. S. N. Hennessey, Esq., First Assistant on the Trigonometrical Survey of India, addressed a letter to the President, in which, after explaining the nature of the calls upon his time occasioned by his professional duties, he offered to devote any portion of his leisure to such scientific experiments as the President might direct. He stated that he was resident at Mussoorie from May to October, residing at Dehra during the remainder of the year, from which place, however, he would be able to visit Mussoorie occasionally, such as once a week. Mussoorie is a hill-station or town, at an altitude above the sea-level of about 6700 feet, in lat. 30° 28′ N., and long. 78° 10′ E. With reference to the climate of this place he observes, "In September the skies begin to clear, and from September 15 to about December 15 there prevails at Mussoorie a clearness of atmosphere such as I have never known in my wanderings. I mention this period in particular; but at all times, when the sky is clear of clouds, the intensity of the heavenly bodies is something exquisite. I have seen Venus distinctly at midday with my unaided eye. It is this wonderful transparency of atmosphere to which I would draw particular attention."

This letter was read to the Council on the 28th of June, and was ordered to be printed, and the President was requested to communicate it to such men of science as he might see fit, with the view of obtaining suggestions with reference thereto.

The President accordingly communicated the letter to several scientific men, accompanied by a request that they would favour him with suggestions as to observations which it might seem to them desirable that Mr. Hennessey should be requested to take up. Answers were received during the recess; and on the 11th of October a Committee was appointed to draw up a report upon Mr. Hennessey's letter, giving specific indications as to the observations in meteorology and with the spectroscope which it might be desirable to make.

Before the Committee presented their report another subject of great scientific interest presented itself in connexion with India. On the 17th of

August in the present year will occur a total solar eclipse of nearly the greatest possible duration. This eclipse will be visible in India, but elsewhere only in parts of the earth which are comparatively uncivilized or difficult of access. The subject was accordingly taken into consideration by the Committee; and both objects are embraced in their report, which is here appended.

"*Report of the Committee on Mr. Hennessey's Letter concerning Astronomical Observations in India.*

"In making a selection of subjects to which they suggest that Mr. Hennessey's attention might advantageously be directed, the Committee have borne in mind that that gentleman's professional duties necessarily occupy the greater part of his time and attention. They have therefore abstained from recommending researches which, though desirable in themselves, would require for their successful prosecution that the observer should devote his main attention to them, or which, though less laborious, are of such a nature as to admit of being equally well carried on elsewhere.

"§ 1. The determination of the order of brightness of the fixed stars by the method of sequences, as described by Sir John Herschel in his Cape Observations, is one for which the extraordinary clearness of atmosphere described by Mr. Hennessey offers peculiar advantages, and which, losing absolutely nothing in value by being taken up in a disconnected manner, is eminently suitable for leisure hours. It requires the use of a good star map, but no apparatus. The peculiar clearness of atmosphere at Mussoorie does not, it is true, extend over a very large part of the year; but by making observations sometimes in the early part of the night, sometimes towards morning, a large part of the heavens might be brought under scrutiny without including stars too near the horizon to give trustworthy results—more especially as favourable nights might occasionally occur at other times of the year. The time at which the comparison of two stars was made should always be noted, in order to render it possible subsequently to make a correction for zenith-distance.

"When numerous observations taken on different occasions have been compared, should any particular star be found to exhibit unusual discrepancies, giving rise to the suspicion that it might be a periodic star, such star would naturally claim special attention, and should be frequently observed in comparison with a few selected stars near it in brightness, with a view to test its periodicity—and in case of confirmation to determine the period, and maximum and minimum brightness.

"§ 2. It would be well if Mr. Hennessey would also watch the zodiacal light, after sunset about the months of April and May, and before sunrise at the opposite time of year, with a view to defining its form and extent, and also noticing anything which may be held to indicate whether it be *lenticular* or *annular*.

"A rough spectroscopic observation of the object, if the light be not

too feeble to allow of it, might be valuable, and an examination of the light for polarization with a double-image prism.

"§ 3. The climate of Mussoorie would make actinometric observations at that place especially interesting. On account of the fragility of the well-known instrument invented by Sir John Herschel, it is proposed to employ the actinometer of Mr. Hodgkinson. *At least* two instruments should be provided, which would allow of *simultaneous* observations at different altitudes being taken when the services of a second observer could be obtained. It seems desirable, as suggested by Mr. Hodgkinson, that an actinometer should be preserved at Kew, to furnish a fixed though arbitrary scale for reference, and that the instruments sent out should be compared before departure with the Kew standard, and their coefficients of reduction to the Kew scale determined.

"§ 4. Much attention has recently been bestowed on the atmospheric lines of the solar spectrum; and it appears to be pretty well established that they are, partly at least, due to absorption by watery vapour. The extreme clearness of atmosphere at Mussoorie would seem to render this a desirable subject of investigation at that place, provided a clear view of the sun can be obtained, either at rising or at setting, down to the horizon, or at least to a very low altitude; and such the Committee are informed is the case.

"For this purpose the observer should be provided with a spectroscope of similar power to that with which Fraunhofer's or Kirchhoff's excellent map of the spectrum was constructed. It is proposed to take the latter as a basis on which to work, but that the observer should confine his attention, at least in the first instance, to the region extending from the extreme red to the line E. The spectrum should first be observed when the sun is high, and compared with Kirchhoff's map. Should any lines represented in the map not be found, and should the want not be due to inferior spectroscopic power, (which may be judged of by comparing neighbouring lines of similar strength with their representation in the map,) such missing lines should be noted as lines probably atmospheric included in the map. Then the spectrum should be observed at low altitudes of the sun, and the lines seen in addition to those in the map measured and drawn. Differential measures referring the additional lines to those represented in the map would be sufficient. Any remarkable increase of breadth of a line, too great to be accounted for merely as a result of decreasing illumination, and proved by direct observation, by weakening the light when the sun is high, not to be referable to that cause, should also be noted. The lines in the map which were missed when the sun was high should now be sought for; for if really atmospheric they would be sure now to come out strongly, unless indeed they were produced by exceptional atmospheric conditions. The time at which the drawings were made should also be noted, from which the sun's altitude could afterwards be calculated if desired; and the hygrometric condition of the air at the

time, the direction of the wind, the character of the clouds, and any electric discharges which might occur should be mentioned.

"In this way we should gradually obtain from independent observation a map of the purely terrestrial lines. In the construction of the map much must be left to the discretion of the observer, as the appearance changes rapidly with the varying altitude of the sun. When constructed, the map ought to be repeatedly compared with the object, with a view to determine whether the system of atmospheric lines be single, or consist of two or more systems superposed. Should two gases possessing the power of definite absorption be present, as it is not likely that they would occur in the same proportion on different occasions, and under different conditions of weather, the complex character of the atmospheric system, if it be complex, would thus probably be in time revealed. The map should also be compared with the map drawn by Sir David Brewster and Dr. Gladstone, from observations made in England and Scotland (Philosophical Transactions for 1860).

"§ 5. In connexion with the offer made by Mr. Hennessey, the Committee have had under their consideration the subject of the total solar eclipse of next year, which will be visible in India. As it will be long before equal facilities for observing the phenomena of the totality recur, it seems desirable not to lose this opportunity of making some physical observations on the phenomenon, which at the present time are of great interest. Above all, the character of the spectrum of the red protuberances may be expected to throw great light on their nature; and it is therefore important to be prepared with an instrument for observing their spectrum, and that of the corona. It is also important that fresh observations should be made on the polarization of the light of these objects.

"In order that a sufficient apparent breadth may be given to the red protuberances to permit of the convenient observation of any lines by which their spectrum may be crossed, it is necessary to use a tolerably high magnifying-power; and in order that this may be done with the least possible loss of light, a corresponding aperture is required in the telescope to be employed. Messrs. Cooke and Sons have on hand a fine telescope of 5 inches aperture, mounted as a portable equatorial, furnished with clock movement, which may be obtained for a moderate sum; and the Committee recommend that this instrument be purchased, with such part of the mounting as is required for the observations contemplated, and be provided with a star-spectroscope. They understand that Lieutenant Herschel, who is now in this country, and is going out to India in the course of a few months, would be willing to undertake the observations, subject to the approval of Colonel Walker, the Director of the Great Trigonometrical Survey of India. But as clouds might interfere with the observations by the large instrument at the critical moment, the Committee think it desirable that three or four direct spectroscopes, by which the general character of the spectrum might be observed, should be entrusted

to Colonel Walker to be placed in the hands of observers at different stations.

"The observation of the polarization would also require the use of a telescope, which, however, may be of much smaller size, such as 2 inches aperture, and will therefore be comparatively inexpensive.

"The Committee estimate that the whole cost of the instruments they have recommended, for the use both of Mr. Hennessey and of those gentlemen who may undertake the observations of the eclipse, will be under £300."

This Report was presented to the Council on the 20th of June, 1867. The Council adopted the report and requested the Committee to take the requisite steps for carrying out their recommendations. They further resolved that application be made to the Government-grant Committee for a sum of not exceeding £300 to defray the expenses, and that the Treasurer be authorized to make such advances within that sum as might in the meantime be required to enable the Committee to proceed with the work.

Mr. Huggins kindly undertook to superintend the construction of the instruments, and to give instruction in the use of the star-spectroscope to the observer who might be selected to observe the spectrum of the red protuberances ; and it is mainly owing to Mr. Huggins's zeal in the cause that the instruments have all been got ready within the limited time allowed.

The following instructions were drawn up for the guidance of the observers who might be entrusted with the observations which it was intended to make on the occasion of the total solar eclipse :—

"*Instructions respecting certain observations to be made in India on the occasion of the Total Solar Eclipse of* 1868, *by means of Instruments sent out by the Royal Society.*

"§ I. *Special Objects of the Observations proposed to be made.*

"According to modern opinion, the photosphere of the sun consists of a shell of liquid or solid particles, constantly forming by condensation in the outer layers of the intensely heated gaseous matter of the sun. When nearly the whole of the light from the solar photosphere is screened off by the moon during a total solar eclipse, two other, feebler sources of light without the sun's photosphere usually show themselves. One of these is in the form of an irregularly bright halo surrounding the sun, and is known as the *Corona*. The other source of light usually presents itself as small tongue-shaped flames more or less coloured, which apparently issue forth from the solar photosphere.

"The special object of the observations suggested is to determine, as far as may be possible, the physical nature of the '*Corona*' and of the '*Red flames*.'

"To obtain this information it is proposed to analyze the light from these sources for polarization, and especially to observe its prismatic spectrum in both cases.

... circles for declination
... stand can be adjusted to any latitude; a...
into three boxes for convenience in travelling.

"The telescope has a very perfect driving
maintained uniform by means of a form of pen
vented by Mr. Cooke, by whom the whole in
constructed for these observations. A Barlow
scope for the purpose of increasing the size of
object-glass. The finder is wired in a special m
eyepiece for convenience in looking from it to
spectrum-apparatus attached to the equatorial te

"The spectroscope, constructed by Messrs. S
dense flint glass, with a reflecting angle of 60°.
nished with a micrometer-screw for measuring the
of any lines observed. There is also provided
illuminated by a small lamp, which can be seen
second surface of the prism. The positions of line
can be read by means of the reflected image of the

"The slit of the instrument is so constructe
equally. A cylindrical lens is provided for the
spectra. The instrument is furnished with a prism
of which the spectra of terrestrial flames can be
the spectra of the objects to which the telescope is

"Four portable hand spectrum-telescopes are
which it will be possible to determine the general
of the lights seen about the sun at the time of the

"For the purpose of analyzing the light of
flames' for polarization, a second tel...
inches aperture. Th...

in the object viewed, both images become strongly coloured with complementary tints.

"The second analyzer is formed of a Nicol's prism and a system of quartz cut obliquely, known as Savart's polariscope.

"It is almost certain that the light from the objects will be polarized in a plane passing through the radius of the sun, if at all; and arrangements may be made accordingly. Nevertheless, if time permits, a rough determination of the plane of polarization, in case the light should prove to be more or less polarized, would be desirable. The plane may be determined roughly by the first analyzer by noticing the azimuth of the analyzer when either image (specifying which) assumes the 'tint of passage' (a purple in which blue and red are equally balanced),—more accurately by the second, by observing the azimuth at which the bands disappear, and stating whether on turning in a given direction from this position the bands seen are black-centred or white-centred, or, which is better, when the polarization is but slight, by observing the azimuth at which the bands are most vivid, and stating the character (black or white) of the central band.

"§ III. *Instructions as to the general method of the observations to be made.*

"i. In order that the observer may acquire the necessary familiarity with the use of the spectroscope, it is recommended that he apply himself as far as his other engagements may permit, during the time after his arrival in India until the period of the eclipse, to a prismatic examination of the brightest of the Southern Nebulæ.

"The instruments sent out by the Royal Society are in every respect suitable and convenient for these observations. The determination of the character of the spectra of the more brilliant of the Southern Nebulæ would be a service for science of very great value.

"It is recommended that the observer provide himself with a list of, say, from fifty to a hundred of the brightest of the Nebulæ the distance of which from the north pole prevents an advantageous study of them in the latitude of England. From this list the observer would select each night the Nebulæ which, at the time of observation, were situated near the meridian. The equatorial mounting, with its finely divided circles, would make the finding of the Nebulæ a task of no difficulty.

"The observer should first make a diagram and general description of the nebula as it appears in the telescope. The object should next be examined with the spectroscope added to the telescope. The telescope should then be moved so as to bring in succession upon the slit the different parts of the nebula. The wire arranged for that purpose in the finder would enable the observer to determine with accuracy the part of the nebula under examination.

"At the commencement of the observation the slit should be widely open, and the observer should then make it as narrow as the light of the

object would permit. In this way the character of the spectrum of each portion of the nebula could be recorded in connexion with the description of its appearance which had been already made. If the spectrum should be discontinuous, the positions of the bright lines should be measured by the micrometer-screw, or by the reflected scale. It is recommended that the screw should be used in preference to the scale whenever it is found practicable to do so.

“For the purpose of obtaining the value of the micrometer-screw and of the scale, measures should be frequently taken of the principal lines of Fraunhofer. These measures should be taken as near the time when the observations are to be made as possible. As a precaution against any accidental displacement of any part of the instrument, at the time of observing the nebulæ, a reading should be taken of the sodium line (D of Fraunhofer) by means of a small alcohol lamp placed before the object-glass of the telescope.

“A series of observations of the principal Southern Nebulæ would be of very great value in the present state of our knowledge, and would certainly repay the entire cost of the instrument, should bad weather, or some unforeseen accident render the primary object, the investigation of the sun, impossible.

“The observer should also practise himself with observing the spectrum of the moon, and of a cloud brightly illuminated by the moon, in order to become familiar with the appearance of spectra which are continuous, except so far as they may be interrupted by dark lines, and which come from objects rather deficient in illumination.

“i. Instructions for the spectrum-observations of the eclipse.

“The equatorial telescope should be previously put up accurately in position, within a suitable temporary hut or observatory. The clockwork is to be adjusted to sun’s apparent motion. The Barlow lens is to be employed, and placed within the focus at a distance sufficient to double, or nearly so, the diameter of the sun’s image.

“Great care must be taken that the finder is in perfect adjustment, so that the spectrum of any object brought upon the point of the indicating wire may with certainty be visible when the eye is placed to the little telescope of the spectrum-apparatus.

“It will be well for the observer to decide previously whether he intends to make use of the micrometer-screw, or of the illuminated scale. Still he is requested to have the small lamp lighted, and both methods of measuring in proper order, so that either could be available instantly during the duration of the totality.

“During the progress of the eclipse, both before and after the totality, the observer is to take measures of the lines of Fraunhofer, with the micrometer, and also with the scale.

“The measures obtained of the eclipse, and which have been made in the interval between the two sets of observations of the solar lines, can

thus be referred to them, and the positions on the spectrum of the lines accurately determined relatively to the lines of Fraunhofer.

"The finder is provided with a diagonal eyepiece, over which a wedge of dark glass is made to slide.

"The observer is expected to watch the progress of the eclipse in the finder, moving the wedge of glass as the light of the sun diminishes.

"As soon as he perceives a 'red prominence,' by means of the very efficient arrangements provided in the instrument, he is to bring the red prominence upon the point of the indicating wire. The observer will then see the spectrum of the 'red flame' in the little telescope of the spectroscope.

"At the commencement of the observations the lowest eyepiece should be used, and the slit should not be too narrow.

"The observer is first to record the general character of the spectrum, whether continuous or discontinuous.

"Then the principal lines, whether dark or bright, are to be measured with as much care as the very limited time will permit.

"As the spectrum of the red prominence is compound, and contains the spectrum of the light of the portion of corona before it (possibly also to some extent of the corona behind it), sufficient time must be left to move the instrument so as to bring upon the slit a part of the corona where it is brightest.

"The character of the spectrum must be in a similar manner examined, and any lines present measured.

"Of course, if there should be found time to do so, it would be desirable that several red prominences should be examined, and also light from different parts of the corona. The observer, however, is strongly recommended to make as complete an analysis as possible of the 'red flame' first selected.

"The observer is requested to send in full all the details of the observations as they were taken down at the time.

"A Clerk will be required to write down the results at the moment from the dictation of the observer.

"iii. The use of the portable hand spectrum-telescopes will be obvious. The observer has only to direct the instrument to the sun at the moment of totality. The instrument should be previously focused to suit the observer, upon the moon or some distant object. The light of the corona and red flames will be dispersed into its component colours. It will be easily detected whether the spectra of the corona and of the red flames are continuous, or consist of bright lines. The four instruments should be placed in the hands of observers stationed at different places along, or nearly along, the central line of the eclipse.

"iv. Observations for polarized light in the corona and 'red flames.'

"A distinct observer is required for these observations. He should be familiar with the telescope and its motions.

"A small observing-hut, or temporary place of shelter, would be probably necessary.

"It is recommended that the eyepiece magnifying twenty-seven diameters be used.

"The observer should fix upon the eye-end of the telescope a disk of cardboard some 12 or 15 inches diameter, which, near the edge, may be roughly divided by a few large figures, which can be easily read in the feeble light which prevails during the totality. To the small tubes carrying the analyzing prisms a long index of card should be attached. In this way the plane of polarization may be read off notwithstanding the feebleness of the light.

"With a dark glass the observer is to watch the progress of the eclipse until the whole of the sun is obscured. The dark glass is then to be removed, and the corona and red flames observed for traces of polarized light.

"There are two analyzers provided.

"The observer is to use first the double-image prism and plate of quartz. A slight degree of polarization will show itself by a difference of colour in the two images.

"An attempt should then be made to determine approximately the plane in which the light is polarized.

"If polarization is detected in the 'corona,' or in any prominence of large extent, the second analyzer may probably be employed with advantage. This consists of a Nicol's prism and a compound plate of quartz, showing Savart's bands. By means of these bands, the plane of polarization of the light analyzed may be easily obtained."

The observation of the spectra of the corona and red protuberances being one of considerable delicacy, it was highly desirable that the observer should have some previous training in this country, while, on the other hand, to send such an observer on purpose would involve considerable expense. It fortunately happened that Lieut. J. Herschel was at the time in this country, and was about to return to India in November 1867, to resume his duties in connexion with the great Trigonometrical Survey of that country. Mr. Herschel took a lively interest in the subject, and at once, on being applied to, undertook the observations which were to be made with the large telescope furnished with the spectroscope, subject to the approval of Colonel Walker, Director of the Survey. This approval was readily given, as will appear from the following letter:—

"Colonel Walker to General Sabine, August 17th.

"Dehra Doon, viâ Bombay,
17th August, 1867.

"My dear Sir,—I have to acknowledge your letter of the 30th June, forwarding a copy of the Report of the Committee appointed by the President and Council of the Royal Society, to take into consideration Mr. Hennessey's letter of the 13th February, 1866.

" I learn with satisfaction that the President and Council have decided on purchasing a telescope equatorially mounted, and furnished with clock-movement and a star spectroscope, to be employed in a close examination of the physical phenomena which may be observable during the solar eclipse of next year—also that other instruments are to be provided, to enable Mr. Hennessey to undertake the researches which he is willing to devote himself to in his brief intervals of leisure from the professional duties which necessarily occupy the greater part of his time and attention.

" I am glad to find that my assistant, Lieutenant Herschel, has placed himself in communication with the Committee, and will receive the fullest instruction as to the employment of the instruments.

" It will be a pleasure to me to do all in my power to carry out your wishes. I expect little or no difficulty in the practical arrangements regarding the eclipse; for it will fortunately happen during the recess season, when our officers can be more easily spared from their professional duties than at any other time of the year.

" I remain, with sincere regard, faithfully yours,
(Signed) " J. C. WALKER."

The instruments, both those for Mr. Hennessey and those intended to be used on the occasion of the total solar eclipse, were ready in time to be sent out under the care of Mr. Herschel; and the following letter to the Secretary, recently received from him, announces their safe arrival:—

" Bangalore, Jan. 23rd, 1868.

" DEAR SIR,—I fear I have hardly done right in delaying so long to inform you of the safe arrival of the instruments entrusted to me by the Royal Society.

" My present occupations absolutely forbid my endeavouring to enter into details; and it is only from a strong feeling that at least so much should be said that I sit down to write at all, at a time when scarcely a moment is my own.

" I have made myself tolerably familiar with the equatorial and spectroscope, and with the appearance of solar, lunar, and stellar spectra, and on one or two occasions have attempted to obtain a view of a nebular spectrum—with tolerable success I believe; but hitherto, whatever time and opportunity, and, I may add, energy, have been available, have been devoted to preliminary examination and manipulation rather than actual employment of the instrumental means. In some respects this has been very necessary, as the parts, never having been employed together, were not at first capable of being connected, &c.

" The next two months will be fully occupied by the work in which I am now taking a part—the measurement of a Base-line; and I cannot devote more than an occasional half hour to the employment of the Society's instruments.

" After that, however, I shall have more leisure; and having full permis-

sion to regard the eclipse-observations as a principal subject of attention, I shall have no excuse for not prosecuting preliminary arrangements, and for not forwarding fuller information of my success or otherwise.

"I am, Sir, yours very truly,

"J. HERSCHEL."

March 12, 1868.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

The following communications were read:—

I. "Notes on the Chemical Geology of the Gold-fields of California." By J. ARTHUR PHILLIPS. Communicated by Prof. A. C. RAMSAY. Received February 22, 1868.

(Abstract.)

Rocks of the Gold-Regions of California.—The great sedimentary metallic belt of California lies on the western slope of the Sierra Nevada, beginning in the neighbourhood of the Tejou Pass, and extending through the state to its northern limit. In consequence, however, of various local circumstances, different portions of this band are of very unequal importance as gold-producing districts.

The slates of the auriferous belt have been shown by Professor Whitney to belong, for a great extent, to the Jurassic period, although the occurrence of numerous Triassic fossils in the gold-bearing rocks of Plumas County and elsewhere renders it more than probable that no inconsiderable portion of the slates in the heart of the gold region are of that age.

The rock constituting the principal mass of the Sierra Nevada is a granite containing only a small proportion of quartz, and in which but one species of felspar (oligoclase) is generally found.

Lying between the band of metamorphic slates and the great central mass of granite forming the more elevated portions of the chain, are found various crystalline rocks, such as syenites, diorites, and porphyries.

Quartz Veins.—The matrix or gangue of the auriferous veins of California is invariably quartz, which is generally crystalline in its structure, or partially vitreous and semitransparent. In the majority of cases the quartz constituting an auriferous veinstone is ribboned in such a way as to form a succession of layers parallel with the walls of the lode itself; and some one or more of these laminæ are not unfrequently far more productive than all the others. In some instances these parallel bands are separated from each other by a thin layer of quartz, slightly differing, either in colour or structure, from that forming the seams themselves; or they may be only distinguished by a difference of colour of two adjoining members of the series.

In many cases, however, laminæ of the enclosing slates divide the vein into distinct bands; and in such instances it will be observed that the thickness of the interposed fragments of slate is sometimes not greater than that of a sheet of the thinnest paper. Cavities or druses containing crystals of quartz occur in all the auriferous veins of the country; and a certain amount of crystallisation may also not unfrequently be remarked along the lines of junction of the several bands of which a vein is composed. In addition to ordinary quartz, in a more or less crystalline form, amorphous hydrated silica, or semiopal, and chalcedony are occasionally met with: in some instances the opal is interfoliated between layers of true quartz, and is sufficiently auriferous to repay the expenses of treatment.

The metallic minerals enclosed in the gangue of auriferous veins are ordinary iron pyrites, blende, and galena, and, less frequently, arsenical pyrites, magnetic and copper pyrites, and cinnabar. These sulphides invariably contain gold; and veins in which some one or more of them does not occur, in considerable amounts, are not regularly and lastingly productive.

Near the surface the iron pyrites and other sulphides become decomposed by the action of air and the percolation of meteoric water through the mass, staining the quartz of a red or brown colour, and leaving the gold in a free state. Under such circumstances numerous cubical moulds of iron pyrites are found in the veinstone; and although this mineral has been entirely removed by chemical action, the cavities left contain finely divided gold, obviously liberated by the decomposition of pyrites.

Beneath the line of natural drainage of the country the sulphides remain undecomposed; but if rock containing pyrites be placed in nitric acid the sulphide becomes dissolved, and finely divided, crystalline, or filiform gold will partially occupy the resulting cavities.

In one of the detrital beds in the vicinity of the village of Volcano in the County of Amador, and elsewhere, distinctly marked quartz veins may be observed cutting through the gravel, and evidently formed by the action of water holding silica in solution.

Attention has also been recently directed to bands of auriferous slate found in the copper-bearing belt west of the main gold-belt of the State, and in the foot hills of the Sierra. In this locality the gold, instead of being obtained from a well-defined vein, chiefly composed of ordinary quartz, is enclosed in a band of siliceous slaty rock, extending north-west and south-east, and dipping in conformity with the other strata of the district.

The number of fluid-cavities contained in the veinstones of the auriferous lodes of California is seen under the microscope to be exceedingly limited; and in order to obtain sections affording good examples, even of small size, it is necessary to select such bands as may be more than ordinarily crystalline, or to operate on thin fragments of crystals sometimes found lining the interior of drusy cavities. In the more opaque and generally

most auriferous portions of veins, the cavities are numerous but exceedingly small, and are often so opaque, apparently rendered so by being internally coated with a lining of clay, that no vacuities can be distinguished.

Out of more than fifty sections of veinstone examined, only some six or seven were found to contain fluid-cavities of sufficient size to admit of any attempt at accurate measurement by means of ordinary appliances; but in all cases there appeared to be considerable differences in the relative dimensions of the vacuities and the enclosing cavities, and the temperatures at which they severally became filled were consequently ascertained by direct experiment. In every instance they were found to require very different degrees of heat to become full, since in the same specimens some of the vacuities disappeared at 180° Fahr., others filled at temperatures slightly above that of boiling water, whilst many, though much reduced in size, remained perfectly visible at 365° Fahr.

Alluvial Deposits.—Although a very large amount of the gold annually obtained was no doubt originally derived from auriferous veins, not more than about one-third of the precious metal collected is procured directly from that source. The larger proportion of the gold now brought into the market is derived from alluvial diggings, in which it is separated by washing from the clay, sand, and gravel with which it is associated.

This gold-bearing drift belongs to at least two distinct geological epochs, both comparatively modern—although the latter period is distinctly separated from the earlier, its materials being chiefly derived from the disintegration and redistribution of the older deposits.

In California the more ancient deposits or "deep placers" are referable to a river-system different from that which now exists, flowing at a higher level, and frequently nearly at right angles to the direction of the main valleys of the present period.

The deep placers are in many localities covered by a thick capping of lava; and the eruptive matter covering them often occurs in the form of basaltic columns, beneath which are found the layers of sand, gravel, and boulders with which the gold is associated. The wood which occurs in these gravel-beds is either beautifully silicified, or is replaced by iron pyrites.

In the more clayey strata of these deposits leaf-beds and impressions of leaves are not unfrequently found; and an examination of these made by Dr. Newberry authorizes the conclusion that the auriferous deposits lying beneath the lava are of tertiary age, and that they probably belong to the later Pliocene epoch. Water-worn gold is disseminated throughout the whole mass of these deposits, not, however, with uniformity, but always in greater abundance near the bottom, and more particularly in direct contact with the "bed rock," which is invariably grooved and worn by the action of water.

The materials of which these deep placers are composed are frequently consolidated into a sort of hard concrete, by being firmly bound together

by crystalline iron pyrites; and sometimes this cementing material consists either of carbonate of lime or silica. The silica is rarely met with in a crystalline form; but near Kenebeck Hill a cavity, resulting from the junction of several pebbles, was found completely lined with well-defined crystals of quartz. These did not show, under the microscope, the usual fluid-cavities of quartz of the ordinary quartz veins of the country.

Where the cementing material of the conglomerate chiefly consists of pyrites, the enclosed trunks of trees are usually replaced by that mineral, although, of two pieces of wood lying in close proximity to each other, one may have become silicified, whilst the other is replaced by iron pyrites.

The assay of several specimens of the cementing pyrites showed that it invariably contained a certain but very variable amount of gold. In order to ascertain whether this exists in the form of water-worn grains mechanically enclosed within the sulphide, or in the form of spongy, crystalline, and filamentary particles, similar to those met with in the pyrites of auriferous veins, various samples were dissolved in nitric acid, and the residues afterwards subjected to microscopical examination. In this way granules of the precious metal, which had evidently been worn by the action of water, were detected, whilst others appeared not to have been subjected to such attrition. Mr. Ulrich states that in the gold-drifts of Australia pyrites is often found replacing roots and driftwood, and that samples have, on assay, yielded from a few pennyweights to several ounces of gold per ton.

Hot Springs.—Hot and boiling springs are exceedingly numerous throughout California; and considerable accumulations of sulphur, together with evidences of extensive solfatara action, are met with in different sections of the State.

The most remarkable instance on the Pacific coast of the actual growth, on a large scale, and at the present time, of mineral veins is probably that afforded by the boiling springs in Steamboat Valley, about seven miles north-west of the great Comstock silver vein in the State of Nevada.

These springs are situated at a height of about 5000 feet above the level of the sea, at the foot of the eastern declivity of the Sierra Nevada. The rock in this locality presents several straight and parallel fissures, either giving out heated water or simply ejecting steam. The first group of crevices comprises five longitudinal springs extending in a straight line, and parallel to each other, for a distance of above 3000 feet. These fissures are partially filled by a siliceous incrustation, which is being constantly deposited on the sides, whilst a longitudinal central crevice allows of the escape of boiling water or steam. On the most eastern of these lines of fracture are five active centres of eruption, from which boiling water is sometimes ejected by the force of steam to a height of from 8 to 10 feet. These waters are alkaline, and contain, in addition to carbonate of soda, the sulphate of that base, together with chloride of sodium.

There is also everywhere an escape of carbonic acid, whilst from some places sulphuretted hydrogen is also evolved. These products, on arriving at the surface, give rise to the deposition of sulphur, silica, and anhydrous oxide of iron. The silica and oxide of iron form semicrystalline bands parallel with the walls of the fissures; and spongy deposits accumulate around some of the points of most active emergence.

At a considerable distance to the west of those above described, a fissure having the same origin is observed; but this is no longer traversed by currents of hot water, although it still gives off steam and carbonic acid at various points throughout its extent. At its northern extremity a central fissure still remains open; but in other localities it is, for the most part, obstructed by siliceous concretious. This siliceous rock is metalliferous, and, in addition to oxide of iron and manganese, contains iron and copper pyrites. M. Laur states that he also discovered metallic gold in this deposit.

The rock enclosing the veins of Steamboat springs is granite, which in their vicinity is much decomposed, being often reduced to a cavernous skeleton of silica containing a few scales of mica.

Alkaline Lakes.—In that portion of California lying on the east of the Sierra Nevada are Mono Lake and Owen's Lake, both considerable sheets of water, highly impregnated with alkaline salts. Owen's Lake lies in lat. 36° 20″ south, long. 118° west from Greenwich, and is about twenty miles in length and eight in width.

The waters of this lake have a specific gravity of 1·076, and contain 7128·24 grs. of solid matter per gallon. The salts held in solution are chiefly carbonate and sulphate of soda, with chloride of sodium; but potash, silica, and phosphoric acid are also present.

The incrustations, which at certain seasons of the year are found to the extent of many hundreds of tons, consist of a white spongy efflorescence, and are, as will be seen from the results of the analysis given in the paper, chiefly composed of carbonate of soda, mixed with a little chloride of sodium and sulphate of soda.

General deductions.—The author remarks that, in the present state of our knowledge, the results of a careful examination of the gold-regions of the Pacific coast would appear to lead to the following conclusions:—

a. Quartz veins have generally been produced by the slow deposition from aqueous solutions of silica on the surfaces of the enclosing fissures.

b. From the general parallelism with its walls of the planes of any fragments of the enclosing rock which may have become imbedded in a vein, it is to be inferred that they were mechanically removed by the growth of the several layers to which they adhered, and that a subsequent deposition of quartz took place between them and the rock from which they had become detached. In this way were introduced the masses of rock known as "horses."

c. The formation of quartz veins is due to hydrothermal agencies, of

which evidences are still to be found in the hot springs and recent metalliferous veins met with in various parts of the Pacific coast.

d. From the variable temperatures at which the vacuities in their fluid-cavities become filled, it may be inferred that they are the result of an intermittent action, and that the fissures were sometimes traversed by currents of hot water, whilst at others they gave off aqueous vapour or gaseous exhalations. This is precisely what is now taking place at Steamboat springs, where the formation of a vein is in progress, and from which currents of boiling water are often poured forth, whilst at other times the fissures give off currents of steam and heated gases only.

e. That gold may be deposited from the same solutions which give rise to the formation of the enclosing quartz, appears evident from the presence of that metal in pyrites enclosed in siliceous incrustations, as well as from the fact of large quantities of gold having been found in the interior of the stems of trees, which in deep diggings are often converted into pyrites.

f. The constant presence of iron pyrites in auriferous veins, and when so occurring its invariably containing a certain amount of gold, suggests the probability of this sulphide being in some way necessarily connected with the solvent by which the precious metal was held in solution. It has been shown that finely divided gold is soluble in the sesquichloride of iron and, more sparingly, in the sesquisulphate of that metal. It is also well known that iron pyrites sometimes results from the action of reducing agents on the sulphates of that metal. If therefore sulphate of iron, in a solution containing gold, should become transformed by the action of a reducing agent into pyrites, the gold, at the same time being reduced to the metallic state, would probably be found enclosed in the resulting crystals of that mineral.

g. The silica and other substances forming the cementing material of the ancient auriferous river-beds have probably been slowly deposited at a low temperature.

The connexion existing between the decomposition of granite by the agency of boiling springs, the existence of alkaline plains, and the formation of lakes containing various salts of soda and potash, is too obvious to require comment.

II. "Third Supplementary Paper on the Calculation of the Numerical Value of Euler's Constant." By WILLIAM SHANKS. Communicated by the Rev. B. PRICE. Received February 29, 1868.

When $n=5000$, we have

$1+\frac{1}{2}+\frac{1}{3}+\dots\frac{1}{5000}=$

9·09450 88529 84436 96726 12455 33393 43939 17829
87811 30384 14506 16283 86638 30530 78016 46808
46902 09226 85495 77084+

E=·57721 56649 01532 86060 65120 90082 40243 10421 59335 93995 35988 05773 14949 71379 78029 07030 (last term is $+\frac{B_{11}}{22 \cdot 5000^{22}}$).

Comparing the values of E obtained from taking $n=500$, 1000, 2000 (given in former papers), and 5000 (given in this), and *assuming* that the *increase* in the several values of E obtainable from taking n higher numbers will be *nearly constant*, we may conjecture that the value of the 60th decimal last found in E will be increased 1 by taking $n=5000 \cdot 4$; the 59th place will be increased 1 by taking $n=5000 \cdot 4^{10}$; in like manner the 58th decimal will be increased 1 by taking $n=5000 \cdot 4^{100}$, and the 57th also 1 when $n=5000 \cdot 4^{1000}$.

It is certain, however, that when n is very large we may, numerically speaking, express E pretty nearly by $Sn-\log_e n$; and indeed when n becomes infinite, the formula

$$E=Sn-\log_e n-\frac{1}{2n}+\frac{B_1}{2 \cdot n^2}-\&c.$$

becomes $E=Sn-\log_e n$, as given by Professor Price in his 'Infinitesimal Calculus.'

In the value of E last found, then, we deem it probable that *at least* 56 *decimals will remain unchanged, whatever high values be given to* n.

March 19, 1868.

JOHN PETER GASSIOT, Esq., Vice-President, in the Chair.

Prof. Theodor Ludwig Wilhelm Bischoff of Munich, Rudolph Julius Emmanuel Clausius of Würzburg, Hugo von Mohl of Tübingen, and Samuel Heinrich Schwabe of Dessau, were proposed for election as Foreign Members; and notice was given from the Chair that these gentlemen would be ballotted for at the next meeting.

The following communications were read:—

I. "Transformation of the Aromatic Monamines into Acids richer in Carbon.—II. On Menaphthoxylic Acid, the Naphthaline-term corresponding to Benzoic Acid." By A. W. Hofmann, LL.D., F.R.S. Received March 4, 1868.

In a paper communicated to the Royal Society* about a year ago, I pointed out the existence of an acid holding to naphthaline the same relations which obtain between benzoic acid and the hydrocarbon benzole. I have since prepared this compound on a somewhat larger scale, and I beg now to submit to the Royal Society some of the results which I have obtained in its examination.

The material used in preparing the new acid is *naphthylamine*, the mon-

* Proceedings of the Royal Society, vol. xv. p. 335.

amine of the naphthyl series. This substance, presenting an exclusively scientific interest but a few years ago, is now produced on an industrial scale. It is more especially a beautifully crystallized yellow colouring-matter (Manchestery ellow), the *dinitronaphthylic acid*, discovered and first employed as a dye by Dr. Martius, which is largely manufactured from naphthylamine. The base occurring in commerce is far from being pure. It is generally met with as a brown fused mass, containing more or less resinous matter and, more particularly, a considerable amount of naphthaline. The purification of the commercial product presents some difficulties; still tolerable crystals may be obtained by crystallization from petroleum.

For the object I had in view it was unnecessary to purify the naphthylamine of commerce. The base was mixed with powdered oxalic acid in such proportions as to produce the primary oxalate with an excess of free oxalic acid. Four parts of naphthylamine and five parts of crystallized oxalic acid were found to yield very satisfactory results. The operation had to be performed upon rather a large scale. After some trials, a cyanide-of-potassium pot, provided with a cover and bent tube, proved to be the most convenient apparatus for distilling the mixture. At the commencement of the operation water and naphthaline were evolved; soon, however, an oily liquid appeared, solidifying on cooling, and consisting of a mixture of *naphthylformamide, naphthyloxamide, oxalate of naphthylamine, naphthylamine*, and *naphthaline*. This distillate was transferred to one of the large two-necked stone-ware bottles which are used for condensing nitric acid, mixed with concentrated hydrochloric acid, and submitted to the action of a powerful current of steam, care being taken to condense the steam by a spiral surrounded with water. Together with the water large quantities of a dark-brown, almost opaque oil, heavier than water, were thus obtained. For this oil, which a more minute examination proved to be the compound I was endeavouring to prepare, I propose, in accordance with its composition, the name of *cyanide of naphthyl*. It still contained appreciable quantities of naphthaline. In the main, however, the reaction had taken place exactly in the same manner as in the case of aniline and toluidine, the deportment of which I have described in my previous paper*.

In the first place, the primary oxalate of naphthylamine had been converted into naphtylformamide,

$$\left.\begin{matrix}(C_2O_2)''\\ H_2\end{matrix}\right\}O_2 + \left.\begin{matrix}C_{10}H_7\\ H\\ H\end{matrix}\right\}N = \left.\begin{matrix}C\ H\ O\\ C_{10}H_7\\ H\end{matrix}\right\}N + H_2O + CO_2,$$

which, losing one molecule of water, had in the second stage of the operation furnished cyanide of naphthyl,

$$\left.\begin{matrix}C\ H\ O\\ C_{10}H_7\\ H\end{matrix}\right\}N = C_{10}H_7CN + H_2O,$$

* *Loc. cit.* p. 336–338.

The purification of the crude product presented no difficulty. The oil was separated from the water by ether, and, after the ether had been evaporated, submitted to distillation. Between 218° and 220° the thermometer became stationary; the fraction distilling at that temperature solidified on cooling, and exhibited the boiling-point, fusing-point, and the other properties of naphthaline. The thermometer then rose rapidly to 290°, and the whole liquid had distilled before the thermometer reached 300°. The liquid distilling between 290° and 300° was of a light-yellow colour, and of a peculiar aromatic odour; after standing for twenty-four hours in a cold room, it likewise solidified to a crystalline mass. On immersion in a frigorific mixture, solidification immediately took place; once solidified, the compound no longer liquefied at the ordinary temperature. The new compound thus obtained is easily soluble in alcohol; the crystals which on evaporation separate from this solution are absolutely pure. On adding water to the alcoholic solution the body separates as an oil, which after a few moments solidifies to a confused mass of crystals. These crystals fuse at 33°·5; fused and resolidified, the substance is heavier than water; its boiling-point is 296°·5 (corr.).

The new compound represents in the naphthyl-series the benzonitrile in the benzoic series. Its composition, as has already been pointed out in the equation exhibiting its formation, is represented by the formula

$$C_{11}H_7N.$$

On dissolving the nitrile in an alcoholic solution of soda, comparatively little ammonia is disengaged; but addition of water to the solution shows at once that the nitrile has undergone transformation. The crystals which are precipitated are far less fusible and soluble than the nitrile. By one or two recrystallizations from boiling alcohol they are obtained in a state of perfect purity. Thus prepared they are fine white needles, fusing at 244° (corr.), and subliming at a high temperature. Analysis has proved these crystals to have the composition

$$C_{11}H_9NO = C_{11}H_7N + H_2O,$$

thus showing that they are the *amide* corresponding to the *nitrile*, from which they are derived by the accession of 1 molecule of water.

I have mentioned that the transformation of the nitrile into the amide is attended by an evolution of ammonia. This evolution is obviously due to the further change of the amide. Assimilating a second molecule of water, this substance gives rise to the formation of an ammonium-salt,

$$C_{11}H_9NO + H_2O = C_{11}H_{11}NO_2 = C_{11}H_7(H_4N)O_2,$$

which in its turn is converted into the sodium compound with evolution of ammonia. Indeed, on adding hydrochloric acid to the alkaline solution, an abundant precipitate of a beautifully crystalline acid is obtained, the properties of which strongly resemble those of benzoic acid. It is scarcely

necessary to state that the difficultly soluble amide, when submitted to the protracted action of boiling soda, is ultimately entirely converted into the new acid.

Of the three substances mentioned, the acid is by far the most interesting, forming as it does the starting-point of a new group of compounds, the number and variety of which can scarcely be inferior to that of benzoic acid. It thus became desirable to prepare the acid on a larger scale. For this purpose it was not necessary to employ the nitrile or the amide in a state of perfect purity. The crude nitrile had simply to be boiled for some time with alcoholic soda, care being taken to condense the vapours, so as not to lose any nitrile by evaporation. When ammonia was no longer evolved the alcohol was boiled off, and the alkaline liquid filtered after cooling, whereby small quantities of naphthylamine were separated. The brown liquid, when decomposed by hydrochloric acid, furnished an abundant curdy precipitate, consisting of the new acid. It was washed with cold water and recrystallized partly from boiling water (in which it is very sparingly soluble), partly from hot alcohol (by which it is copiously dissolved). Thus purified, the new acid presents itself in the form of white needles, which fuse at 160° C. Fused and resolidified, the new compound is heavier than water. When heated beyond its fusing-point the new acid sublimes. The boiling-point is considerably above 300°. The acid is nearly inodorous and tasteless; when heated it feebly evolves the odour of naphthaline; its vapour, like that of benzoic acid, irritates the respiratory organs. The solutions of this acid have a distinct action upon litmus-paper, and decompose with facility (more especially on heating) the alkaline carbonates.

In accordance with a principle of nomenclature which I proposed some time ago, I will designate the new acid by the name *menaphthoxylic* or *naphthaline-carboxylic acid.* In this case the amide receives the name *menaphthoxylamide,* the nitrile that of *menaphthenylnitrile.*

I may be permitted briefly to mention some of the observations which, in studying the new compound, I have already been able to collect.

It has been stated that the nitrile fixes water with facility. It could not be doubtful that in like manner it would also combine with sulphuretted hydrogen. Indeed, when dissolved in alcoholic sulphide of ammonium, and exposed for some hours to a temperature of 100°, the nitrile absorbs one molecule of hydrosulphuric acid, being converted into a beautifully crystalline body, easily soluble in alcohol, which fuses at 126°, and has the composition

$$C_{11}H_9NS = C_{11}H_7N + H_2S.$$

This substance is *menaphthosulphylamide,* corresponding to thiobenzamide, the sulphuretted derivative of benzonitrile discovered by M. Cahours.

I have examined menaphthoxylic acid somewhat in detail. Like benzoic acid, to which it has a strongly marked family resemblance, it is a monobasic acid. The *silver-salt* is a white, scarcely crystalline precipitate,

nearly insoluble in water, which is obtained when the ammonium-salt is decomposed by nitrate of silver. Its composition is

$$C_{11}H_7AgO_2.$$

The *barium-* and *calcium-salts* are crystalline compounds, difficultly soluble in water, easily obtained by double decomposition, and purified by crystallization from boiling water.

The barium-salt forms white needles which, when dried *in vacuo*, exhibit the composition

$$\left.\begin{matrix} C_{11}H_7 \\ C_{11}H_7 \end{matrix} Ba'' \begin{matrix} O_2 \\ O_2 \end{matrix}\right\} + 4H_2O.$$

At 110° the salt loses its water.

The calcium-salt also crystallizes in needles. The analysis of the vacuum-dry salt led to the formula

$$\left.\begin{matrix} C_{11}H_7 \\ C_{11}H_7 \end{matrix} Ca'' \begin{matrix} O_2 \\ O_2 \end{matrix}\right\} + 2H_2O.$$

At 110° the salt becomes anhydrous.

The *copper-salt* and the *lead-salt* are respectively green and white precipitates.

Highly characteristic is the deportment of the acid when it is submitted to the action of caustic baryta. Faithful to the traditions of benzoic acid, menaphthoxylic acid splits into carbonic acid and naphthaline,

$$C_{11}H_8O_2 = C_{10}H_8 + CO_2.$$

The naphthaline thus obtained possesses the fusing-point, and the properties in general of the hydrocarbon formed in the distillation of coal.

Menaphthoxalate of calcium, when submitted to the action of heat, yields an aromatic distillate which gradually solidifies to a crystalline mass, probably the ketone of the series.

Nitric acid gives rise to the formation of a beautiful nitro-acid; when boiled with very concentrated acid, menaphthoxylic acid is transformed into a difficultly soluble crystalline compound which is no longer acid.

Some experiments on the chloride corresponding to the acid, and some of the derivatives of the chloride, may still be briefly here recorded.

On mixing four parts of menaphthoxylic acid (fused and powdered after solidification) with five parts of pentachloride of phosphorus, the two compounds begin to act upon each other at the common temperature. The mixture becomes liquid, and disengages, when gently heated, abundant quantities of hydrochloric acid and oxychloride of phosphorus. The boiling-point of the liquid rapidly rises to 300° What distils between 296° and 298° is the pure *chloride of menaphthoxylic acid*, the boiling-point of which is pretty accurately 297°·5. Menaphthoxylic chloride is a liquid at the common, a solid at low temperature; it has the composition

$$C_{11}H_7OCl,$$

and exhibits the deportment of the aromatic chlorides in general. When exposed to the atmosphere it absorbs moisture, being gradually transformed

into menaphthoxylic and hydrochloric acids. Addition of water produces this effect instantaneously.

In contact with ammonia the chloride is converted into *menaphthoxylamide*, with all the properties of the compound generated by the action of alcoholic soda upon the nitrile.

The action of aniline upon the chloride gives rise to the formation of *menaphthoxylphenylamide*,

$$C_{17}H_{13}NO = \left.\begin{matrix} C_{11}H_7O \\ C_6H_5 \\ H \end{matrix}\right\} N,$$

white crystals, insoluble in water, readily soluble in alcohol, easily purified by crystallization. Their fusing-point is 160°.

When aniline is replaced by naphthylamine, the corresponding naphthylated compound, *menaphtkoxylnaphthylamide*, is produced,

$$C_{21}H_{15}NO = \left.\begin{matrix} C_{11}H_7O \\ C_{10}H_7 \\ H \end{matrix}\right\} N,$$

crystalline powder, insoluble in water and benzole, difficultly soluble in alcohol. It fuses at 244° (corr.).

On treating menaphthoxylic chloride with absolute alcohol, the *ethylether of menaphthoxylic acid* is formed,

$$C_{13}H_{12}O_2 = \left.\begin{matrix} C_{11}H_7O \\ C_2H_5 \end{matrix}\right\} O,$$

aromatic liquid, insoluble in water, boiling at 309° (corr.).

I have also prepared the *anhydride of menaphthoxylic acid* by submitting, according to Gerhardt's method, the chloride to the action of a menaphthoxylate. For this purpose the calcium-salt, dried at 110°, was mixed with an equivalent quantity of the chloride, and maintained for some time at 140°; it is insoluble in water, difficultly soluble in alcohol, and easily soluble in ether and benzol.

In conclusion, I beg to express my thanks to Mr. Cornelius O'Sullivan for the zealous assistance which he has given me in performing these experiments.

Since in my first communication to the Royal Society I pointed out the existence of menaphthoxylic acid, this substance has been produced by another reaction which appears to be more advantageous than the process described in the preceding paper. By distilling a sulphonaphthylate with cyanide of potassium, M. V. Merz* has obtained an oil possessing the composition and the properties of the cyanide of naphthyl as obtained by treating naphthylamine with oxalic acid,

$$C_{10}H_7KSO_3 + KCN = K_2SO_3 + C_{10}H_7CN.$$

As far as I can judge from the statements published by M. Merz, I con-

* Zeitschrift für Chemie, 1868, p. 133.

sider the two substances identical. Treated with hydrate of potassium, this nitrile is converted into an acid which M. Merz describes under the name of *naphthaline-carboxylic acid.* The opinion expressed by this chemist, that his acid might be identical with the one observed by myself, I am inclined to adopt, although there are still some few discrepancies in our observations to be elucidated. M. Merz states that the fusing-point of his acid is at 140°, whilst the acid examined by myself fuses at 160°. In order to remove, if possible, this discrepancy, I have, since I saw M. Merz's paper, again and repeatedly taken the fusing-point of menaphthoxylic acid, but always with the same result. Possibly the fusing-point of the acid prepared by means of a sulphonaphthylate may be found somewhat higher when the compound is carefully purified by repeated crystallization from alcohol.

II. "On the Relation of Form and Dimensions to Weight of Material in the Construction of Iron-clad Ships." By E. J. Reed, Chief Constructor of the Navy. Communicated by Prof. G. G. Stokes, Sec. R.S. Received March 3, 1868.

(Abstract.)

The object of the Paper is to show that the proportion of length to breadth in a ship, and the form of her water-lines, should be made in a very great degree dependent upon the weight of the material of which her hull is to be constructed—that an armour-plated ship, for example, should be made of very different proportions and form from those of a ship without armour, and that as the extent and thickness of the armour to be carried by a ship are increased the proportions of length to breadth should be diminished, and the water-lines increased in fulness.

It is highly desirable that this subject should receive the attention of men of science, not only because it bears most directly upon both the cost and the efficiency of future iron-clad fleets, but also because it opens up a theoretical question which has hitherto, I believe, received absolutely no consideration from scientific writers upon the forms and resistances of ships, viz. the manner in which the weight of the material composing the hull should influence the form. Prior to the design of the 'Bellerophon,' the forms of ships were determined in complete disregard of this consideration; and even the most recent works upon the subject incite the naval architect to aim always at approaching the form of least resistance. The investigations given in the Paper show, however, that the adoption of a form of least resistance, or of small comparative resistance, may, in fact, lead to a lavish outlay upon our ships, and to a great sacrifice of efficiency; while, on the other hand, the adoption of a form of greater resistance would contribute in certain classes of ships to greater economy and to superior efficiency.

In order to indicate clearly, but approximately only, the purpose in view, the author first considers the hypothetical cases of a long and a shorter ship, both of which are prismatic in a vertical sense. The length of the long ship is seven times its breadth, and its horizontal sections consist of two triangles set base to base; the length of the short ship is five times its breath, the middle portion being parallel for two-fifths of the length, and the ends being wedge-shaped. It is assumed also that at a speed of 14 knots the long ship will give a constant of 600, and the short ship a constant of 500 in the Admiralty formula,

$$\frac{\text{speed}^3 \times \text{mid. section}}{\text{indicated horse-power}}.$$

The draught of water is in each case 25 feet, and the total depth 50 feet.

It is taken for granted that the form of the long ship has been found satisfactory for a ship of such scantlings that we may consider her built of iron of a uniform thickness of 6 inches, the top and bottom being weightless.

Now, let it be required to design a ship of equal speed, draught of water, and depth, but of such increased scantlings (whether of hull proper or of armour) that the weight shall be equivalent to a uniform thickness of 12 inches of iron, the top and bottom being weightless as before. First, the new ship has the proportions of the long ship given to her; and secondly, those of the shorter ship. In each case the engines are supposed to develope seven times their nominal horse-power, and to weigh (with boilers, water, &c.) 1 ton per nominal H.P. The coal-supply in each case equals the weight of the engines, so that both ships will steam the same distance at the same speed. But as the equipment of the smaller ship will be less weighty than that of the larger ship, we will require the larger ship to carry 2000 tons, and the smaller 1500 tons additional weights.

Assuming the breadth extreme in each case to be the unknown, we can, from the Admiralty formula given above, deduce an expression for the Indicated Horse-Power; thence, under the assumed conditions, the weights of engines and coals can be found; and these being added to the weights of hull (calculated on the assumption that the sides are of 12-inch iron), and to the weights carried, give an expression for the total displacement, in tons, of each ship. Another expression is found for this displacement by finding the weight of water displaced. The two expressions are equated, and a quadratic equation is formed, from which the breadth extreme is determined; and from it all the other values can be found.

The accompanying Table shows the results obtained by this method for the two classes of ships:—

	Long ship.	Shorter ship.
Length extreme	581 feet.	342 feet.
Breadth „	83 „	68½ „
Nominal horse-power	1350 H.P.	1337 H.P.
Indicated „	9450 „	9359 „
Weight of hull	12570 tons.	7576 tons.
„ engines	1350 „	1337 „
„ coals	1350 „	1337 „
„ carried	2000 „	1500 „
Total displacement	17270 „	11750 „

It will therefore be seen that, by adopting the proportions and form of the shorter ship, a ship of the required scantlings and speed will be obtained on a length of 342 feet and a breadth of 68½ feet; whereas if the proportions of the long ship are adopted, the ship, although of the same scantlings and speed only, will require to be 581 feet long and 83 feet broad, the steam-power in both cases being as nearly as possible the same.

Considerations of this character, worked out more fully, led the designer of the 'Bellerophon' to depart so considerably from the form and proportions of the 'Warrior.'

The next part of the investigation is based upon the official reports of the measured mile trials of the 'Minotaur' and 'Bellerophon' when fully rigged, and upon calculations made from the drawings of those ships. It is assumed that a prismatic vessel having the same mean draught as each of these ships, and having the same form and dimensions as the mean horizontal section (which equals the mean displacement in cubic feet, divided by the mean draught of water), will give the same constant as the ship herself, at the assumed speed of 14 knots, which, as nearly as possible, equals the speed obtained by both the 'Minotaur' and 'Bellerophon' on the measured mile. For each ship the weight of the armour and backing is supposed to be uniformly distributed over vertical prismatic sides of the dimensions of the armoured sides; and the weight of hull is similarly distributed over vertical prismatic sides of the dimensions below water of the mean horizontal section, and above water of the armoured side. The actual weights carried by the ships are thus transferred to what may be termed representative prismatic vessels, having the same constant of performance as the ships. The detailed calculations in the Paper show that the weight per square foot of the material in the hulls of the two ships, when distributed over the sides of the representative prismatic vessels, is very nearly the same for both; and the same holds with respect to the weight per square foot of armour and backing. The 'Minotaur' is rather heavier in both respects; but, for the reasons given in the Paper, the means of the values found for the two ships are taken, and are found to be

Weight per square foot of hull $= \cdot 152$ ton.
„ „ „ armour and backing $= \cdot 11$ ton.

The questions next considered are these: presuming it to be necessar

to build another ship, which shall also steam 14 knots, carry the same proportionate supply of coal to engine-power and proportionate quantities of stores, but shall have her armour and backing of double the weight of armour and backing of the 'Bellerophon' and 'Minotaur,' then (1) what will be the size, engine-power, and cost of the new ship of the 'Minotaur' type, and having the same mean draught and depth of armour? and (2) what will be the size, engine-power, &c., if built on the 'Bellerophon' type, and having her mean draught and depth of armour?—this condition implying, of course, that the same constants of performance as before will be realized in each case. On account of the great disproportion in size between the two types of ship, it is obvious that the smaller one will require much less weight of equipment. It is assumed, therefore, that the additional weights of the smaller ship (exclusive of engines, boilers, and coals) amount to 700 tons, and those of the larger ship to 1000 tons. The developed power of the engines, proportionate supply of coal, and the weight of engines &c. are taken exactly the same as in the hypothetical case first given.

By proceeding with the investigation for each case in a way similar to that sketched for the hypothetical ships, only treating the breadth extreme of the mean horizontal sections of the new ships as the unknown, the following results are obtained. The new ship of the 'Minotaur' type which fulfils the required conditions will be nearly 490 feet long, 72½ feet breadth extreme, and have a total displacement of 14,253 tons; while the new ship of the 'Bellerophon' type will be 380 feet long, 71 feet breadth extreme, and have a total displacement of 10,950 tons. It thus becomes obvious that a correction is needed in the weight per square foot of hull in the new ship of the 'Minotaur' type, as her length has been so greatly increased: it is considered that an increase of at least 10 per cent. is required; and this is the allowance made. On the other hand, the new ship of the 'Bellerophon' type is still shorter than the 'Minotaur' herself, and the displacement is not much greater than the actual displacement of the 'Minotaur;' so that no correction is needed in her weight per square foot of hull. When the correction has been made for the new ship of the 'Minotaur' type, the final results in round numbers are as follows for the two classes of ship:—

	New ship of 'Minotaur' type.	New ship of 'Bellerophon' type.
Length	510 feet.	380 feet.
Breadth	75 "	71 "
Tonnage	13770 tons.	8620 tons.
Nominal horse-power	1080 H.P.	1080 H.P.
Indicated "	7560 "	7560 "
Weight of hull	7100 tons.	4460 tons.
" armour and backing	5190 "	3630 "
" engines and coals	2160 "	2160 "
" stores carried	1000 "	700 "
Displacement	15450 "	10950 "

Taking the cost per ton at £55 (which is the average cost per ton of tonnage for the hulls of armour-clad ships), the saving made by adopting the new ship of the 'Bellerophon' type would amount to £283,250, or considerably more than a quarter of a million sterling. It must also be considered that the ship of the 'Bellerophon' type would cost less for maintenance and repair, and be much handier in action.

The last investigation in the Paper is purely theoretical, and consists of a determination of the dimensions which would be required in two ships of which the horizontal sections are curves of sines, and which are prismatic vertically, if they were built with the same weight per square foot of hull (say 1 ton) as the 'Bellerophon,' but carried twice the weight of armour per square foot (say $\frac{6}{28}$ ton). In these cases the bottom is taken to have weight as well as the sides; the speed for both is 14 knots, the draught of water is 25 feet, and the depth of the armoured side 24 feet. One of the ships is seven times her breadth in length, and the other is five times. Professor Rankine's rule for the calculation of horse-power and speed is employed; and the same conditions of engines &c. are assumed as have been indicated previously. The larger ship carries 1350 tons additional weights, and the smaller 900 tons.

The results obtained for these ships are as follows, when expressed in round numbers:—

	Larger ship.	Smaller ship.
Length	585 feet.	425 feet.
Breadth	84 „	85 „
Nominal horse-power	1267 H.P.	980 H.P.
Indicated „	8890 „	6860 „
Weight of hull	7586 tons.	5540 tons.
„ armour and backing	6124 „	4470 „
„ engines and coals	2540 „	1960 „
„ carried	1350 „	900 „
Displacement	17600 „	12870 „

These results are very different in detail from those obtained in the cases based on the actual trials of the 'Bellerophon' and 'Minotaur.' The 2000 H.P. which is needed by the larger ship above the power required by the smaller ship, is principally due to the difference between the immersed surfaces of the two ships, and is spent in overcoming friction. The immersed midship sections, it will be remarked, only differ by a very small amount.

This last investigation serves to show that, the theoretical best form of ship being taken, and the most recent rule being applied in the calculations, the speed of 14 knots can be obtained in the short type of ship at a surprisingly less cost and size than the long type requires; and this result agrees with that of the preceding investigation based on actual trials.

March 26, 1868.

Lieut.-General SABINE, President, in the Chair.

Pursuant to notice given at the last Meeting, Theodor Ludwig Wilhelm Bischoff of Munich, Rudolph J. E. Clausius of Würzburg, Samuel Heinrich Schwabe of Dessau, and Hugo von Mohl of Tübingen, were balloted for, and elected Foreign Members of the Society.

The following communication was read:—

"On the Amount and Changes of the Polar Magnetism at certain positions in Her Majesty's Iron-built and Armour-plated Ship 'Northumberland.'" By FREDERICK JOHN EVANS, F.R.S., Staff-Captain R.N., in charge of the Admiralty Magnetic Department. Communicated with the sanction of the Lords Commissioners of the Admiralty. Received March 5, 1868.

(Abstract.)

The 'Northumberland' is a ship of 6621 tons, built at Millwall, River Thames; head N. 39½° E. magnetic, and completed with the armour-plates in the same direction: the launch was effected on 17th April 1866; she then lay for eight months in the Victoria Docks, head S. 22° W. magnetic, or in a direction nearly opposite to that occupied in building.

From January to March 1867 she lay at Sheerness swinging to wind and tide: the ship was then removed to Devonport and placed in dry dock, head S. 84° E. magnetic, where she has remained till the present time. Observations of deviation and horizontal and vertical force were made at the standard compass (elevated 8¾ feet from the iron deck, and 172 feet distant from the stern), the poop-, and two steering-compasses (starboard and port), the latter being on the quarter-deck below the poop-compass,—the group being placed near the steering-wheel, 52 feet from the stern, and each compass 4 feet above its own deck.

The polar force at each compass was originally directed to the part of the ship which was south in building; it diminished in the Victoria Docks, showed a tendency to return to its original value at Sheerness; and finally a large force to starboard was developed by the position of the ship in dock for the lengthened period at Devonport.

Tables are appended giving the magnetic coefficients for each compass from time to time, extending from 17th April 1866 to 10th December 1867.

The results show that the greater part of the polar magnetism was caused by the subpermanent magnetism of the *whole mass of the ship*, due to her position in building and afterwards: this polar force was so great as to require correction by magnets in addition to the usual tabular corrections.

The 'Northumberland' was the subject of a singular attempt to "depolarize" her, by the Patentee of a process "for an improved method of correcting the deviation of compasses in iron ships"*.

The attempt was made—first, on the 4th August 1866, in Victoria Docks, by moving electromagnets over the external plates of the ship, but without effect; next at Sheerness, in January 1867—a similar process without effect; then by applying electromagnets to the beams of the poop-deck, in immediate proximity to the poop- and two steering-compasses, and with considerable effect, as a powerful north pole of a subpermanent quality was developed in the centre of those beams (about 5 feet abaft the compasses), producing a repulsive force on the poop- and steering-compasses amounting nearly to two-thirds of the earth's force.

By this the semicircular deviation of the poop and port steering-compasses was reduced to $\frac{1}{4}$ of its original amount, returning, however, in the course of a year (eleven months), as determined by observations made in June, August, and December 1867, to $\frac{3}{4}$ of its original amount. The deviation of the starboard steering-compass was altered slightly in amount, and largely in direction; but is now, in common with that of the two compasses just named, gradually returning to its original state.

The deviation of the standard compass was not affected by the operations in the slightest degree.

The "heeling" deviation of the poop-compass was affected in nearly the same way as its semicircular deviation: the "heeling" deviation of the starboard steering-compass was *increased*; but the *increase*, like the decrease of the poop-compass, was fast disappearing in December 1867.

The correction by the "depolarizing" or "demagnetizing" process was therefore both imperfect and transient, and productive of more injury than benefit; in consequence of which the author has submitted to the Admiralty that no so-called "depolarization" should be allowed within 20 feet of any compass placed for the navigation of the ship.

* A full account of this "depolarizing" process, with the general views of the patentee, will be found embodied in two papers read before the Royal United Service Institution, and the discussions thereon, as published in the Journal of the Institution:—the first paper, on "Terrestrial Magnetism with reference to the Compasses of Iron Ships; their deviation and remedies," read January 29th, 1866; the second paper, on "The Demagnetization of Iron Ships, and of the iron beams &c. of wooden vessels, to prevent the deviation of the compasses, &c.," read May 6th, 1867,—both papers by Evan Hopkins, Esq., C.E., F.G.S.

April 2, 1868.

Lieut.-General SABINE, President, in the Chair.

The following communication was read :—

"Report of the Committee on the Melbourne Telescope to the President and Council of the Royal Society." Communicated by the President.

The Committee were informed by Mr. Grubb at the close of last year that the telescope was ready for their final examination; but the bad weather which has prevailed in Ireland ever since precluded all trials of its optical power till February 17, when they met at Mr. Grubb's works in Rathmines Road.

1. The telescope was not finished at the time named in the contract; but the Committee have ascertained that the delay arose solely from unfavourable weather, which not only impeded the actual work of polishing, but for weeks together made it impossible to test the figure of the specula. They considered that it was far more important to send out a perfect instrument than to keep the exact time.

2. The Committee, after minutely and carefully studying the mechanical details of the equatorial, have come unanimously to the conclusion that it is a masterpiece of engineering. Its movements are surprisingly smooth and steady; it can be moved to any portion of the sky, even if it have to be reversed from one side of its pier to the other, in less than a minute by two operators and with very little exertion.

The clock is smooth and equable in its action, it is very powerful, and quite equal to its work. Great change of rate, as from sidereal to lunar time, is effected by an ingenious piece of differential gearing; small changes are made by a cam adjustment; moreover it rings seconds, for the double object of comparing its rate with a chronometer and to assist the observer in his observations.

3. The Committee are strongly impressed by the great convenience to the observer of the arrangements of the hour and polar-distance circles, the facility of controlling their adjustment, and the easy access to the eyepiece.

4. The stability of the tube was severely tested, both in respect of its general stiffness and its power of resisting torsion, such as might be produced by the weight of the small speculum when the telescope is off the meridian; and the results were highly satisfactory.

5. In large reflecting telescopes it is usual to make provision for keeping a given diameter of the great speculum always in a vertical plane. When they are equatorially mounted, this is done by rotating the tube in its cradle. Here the tube does not turn; but there is a special arrangement of hoop-suspension, by which, whatever diameter may be vertical, it is supported in a uniform and symmetrical manner. The system of trian-

gular levers at the back of the great speculum is also contrived so as to prevent them from exerting any pressure which might distort it. This is a matter of the highest importance, and the attention of the Committee was specially directed to it. They examined it most carefully, putting the telescope in various positions of Æ and P. D. on each side of the meridian, both by day and night, and could not find *any sign of flexure or any distortion of the image in any of these changes.*

6. In order to test the optical power of the second speculum B (the first one, A, had been tried and approved by a member of the Committee on October 12 last), the telescope was directed to the following objects:—In the daytime Venus and α Andromedæ; at night (which fortunately was clear and steady) Castor, the Great Nebula of Orion, ζ Orionis, γ Andromedæ, Uranus, 1 Messier, 37 Messier, 46 Messier, and 51 Messier, from which it will be seen that both the light-collecting and defining powers of the instrument were fairly tried. The powers used were 220 (the lowest which can take in the entire pencil), 350, and 450, all negatives. Of course one would not propose such an instrument for the measurement of close double stars, work for which telescopes such as those of Poulkova and Harvard are possibly better fitted; but the Committee found that the light even of large stars was collected into small, hard, and perfectly circular disks, free from rays; and though some diffused light* surrounded them, it was exactly concentric with the central disks. The 5th and 6th stars of the Trapezium of Orion were not only plainly seen, but were very bright; ζ Orionis was well shown, and the companion of γ Andromedæ was clearly divided with the powers of 350 and 450, and the different tints of the components were evident. Uranus was well seen, but was surrounded by such a multitude of very minute stars that, without access to the tables of his satellites, it was impossible to know whether any of them were seen†. 37 M. was broken into a heap of stars so large and brilliant that it quite lost the character of a cluster. The planetary Nebula in 46 M. brought out most strikingly the light-collecting power of this telescope; for it (which in most telescopes appears as a faint disk) was revealed as a ring, bright even on the dazzling ground of the surrounding stars, which here were as brilliant as the components of the Pleiades appear in ordinary instruments. With respect to the Nebulæ, it is needless to say more than that Lord Rosse considers its performance in bringing out the details of the Orion Nebula, 1 M. (the Crab), and 51 M. (the Great Spiral), quite satisfactory.

7. The Committee had no opportunity of testing the spectroscope on

* The cause of this diffused light has since been discovered and removed.

† On the 19th of February, however, the Committee had the advantage of the presence of Mr. Lassell in the examination of Uranus among other objects with Speculum A, and, guided by his familiarity with that object, they were enabled to make out stars, the positions of which, with great probability, corresponded to the places of the two most distant, and one of the nearest satellites.

stellar or nebular spectra; but they have tried it on solar and electric ones, and consider that it will be very effective, from the solidity of its structure, and its large dispersion combined with a small deviation.

8. The balance of the grant, which will probably be absorbed by the expense of packing the instrument for removal, was not sufficient to justify the Committee in ordering a photographic apparatus which should be worthy of the telescope; but some trials have been made with a "makeshift affair," which confirm them in their opinion that it is most important to turn to account the photographic power of this magnificent instrument, not only for the moon, but for the planets *and the sun.* On two or three occasions Castor and the moon were taken with this temporary apparatus: as to the star, its components impressed their images in good measurable disks in times varying from two to eight seconds; with respect to the moon, on February 1, when she was seven days old, and the air good, a remarkably hard and sharp picture, full of minute details, was obtained, which exhibits so strongly the great photographic power of the instrument, that they would regard it as a serious loss to science if this was not most fully brought into action. This is the more desirable because lunar and solar photography would utilize a considerable portion of time, during which the observing of nebulæ is impossible. After full inquiry, the Committee find that the cost of the necessary apparatus for this work, including a micrometer for measuring distances and positions on the photographs (like that described in the Philosophical Transactions, 1862, p. 373), could be provided for a sum not exceeding £400; and they earnestly hope that so valuable an addition may be made. They have been given to understand that the Melbourne Government have resolved on putting a roof over the instrument, but that they think it can be more economically constructed there than here. In this case it occurs to the Committee that the second form of roof described in Dr. Robinson's letter might be preferable to the more complex one there recommended; and as it would be less costly, the difference would more than cover the expense of the photographic apparatus. In this hope they have directed Mr. Grubb to prepare a detailed plan of that apparatus and of the second form of roof. The Committee conclude by stating that they have no hesitation in declaring that the instrument is perfectly fit for the work for which it was destined. They therefore consider that Mr. Grubb has fulfilled his contract, and have directed him to lose no time in preparing the necessary cases and packing it for Melbourne. They have also instructed him to ensure it against the risk of fire during its stay.

The Committee feel bound to say that Mr. Grubb has put a most liberal construction on the terms of his contract; and after their minute examination of the excellence of the telescope, and the amount and perfection of the machinery connected with it and its manufacture, they are convinced that Mr. Grubb has been more influenced by the desire of producing a perfect instrument than by any prospect of pecuniary advantage, and can *scarcely realize* the possibility of giving so much for the sum

named in the contract, especially when it is considered that special works had to be erected for the purpose of constructing the telescope.

ROSSE,
T. R. ROBINSON, D.D.
Feb. 19, 1868. WARREN DE LA RUE.

P.S. March 7th, 1868.—I would strongly recommend that the photographic apparatus should be fitted to the telescope before it leaves Ireland.
WARREN DE LA RUE.

The Society then adjourned over the Easter Recess to Thursday, April 23, 1868.

April 23, 1868.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

The following communications were read:—

I. "On the Geographical and Geological Relations of the Fauna and Flora of Palestine." By the Rev. HENRY BAKER TRISTRAM, M.A., F.G.S. Communicated by P. L. SCLATER, M.A. Received March 10, 1868.

(Abstract.)

A detailed examination of the fauna and flora exhibits results remarkably in accordance with the views expressed by Mr. Sclater and Dr. Günther on the geographical distribution of species. Palestine forms an extreme southern province of the Palæarctic region.

In every class, however, there are a group of peculiar forms, which cannot be explained simply by the fact of Palestine impinging closely on the Ethiopian, and more distantly on the Indian region, but which require a reference to the geological history of the country.

The results of the examination of the collections made in 1864 by the expedition assisted by the Royal Society, may be tabulated thus:—

	Total.	Palæarctic.	Ethiopian.	Indian, including those which are also Ethiopian.	Peculiar.
Mammalia	82	41	30*	13	7
Aves	326	258	36†	14	27
Reptilia	48	25	13‡	2	4§
Pisces, fluviatile	17	1	3	3	10‖
Mollusca	146	48	8	2	81
Flora, general	963¶				
Flora, Dead-Sea basin (Phanerogamic)	113	27	71**	26	3

* Of which 9 are also Indian.
† Of which 8 are also Indian.
‡ Of which 1 is also Indian.
§ And 5 others Asiatic, but not Indian.
‖ Of which 5 are also Syrian and Asia Minor.
¶ *About 1300 species are known from Palestine (Phanerogamic).*
** *Of which 26 are also Indian.*

Several of the Ethiopian Mammalia are sedentary forms, and seem to point to an earlier settlement than across the recent deserts. There is no trace of any immigration from the Indian region. Of the peculiar species, *Hyrax syriacus* belongs to an *exclusively* Ethiopian and isolated type, yet is specifically different from its congeners, which are all most sedentary in their habits.

The Avifauna is very rich in number of species, most unequally distributed. The Ethiopian and Indian types are almost exclusively confined to the Dead-Sea basin, excepting only the desert forms. There are several Indian species, as *Ketupa ceylonensis*, which have no affinities with any Ethiopian forms. Of the *peculiar* species, besides several modifications of well-known Palæarctic forms, there are eleven, belonging to as many different Ethiopian and Indian genera. Three of these are decidedly Indian in their affinities. The Avifauna of the Dead-Sea basin is decidedly distinct and typical, sometimes Indian, more generally Ethiopian in its character.

In the Reptilia there is a less prominent intrusion of Ethiopian types, there being a general similarity to the Egyptian herpetological fauna, which must be classed within the Palæarctic region. The Indian is present in *Daboia xanthina*; and the affinities of a new genus *Rhynchocalamus* are rather obscure. Snakes in particular are more limited to the original locality of the individuals, and the groups, like the individuals, are more stationary.

The fluviatile ichthyological fauna is much more distinct, though the number of species is small. In its consideration we confine ourselves to the Jordan and its tributaries, in which are three Nilotic fishes, three others extending eastward in Asia, six to other rivers of Syria, and four peculiar, bearing a strong affinity to the species and genera (as *Chromis* and *Hemichromis*) of tropical Eastern Africa.

Of the Mollusca, most of the peculiar species have no geographical signification. The Pulmonifera have developed in groups, which are modifications of desert types in the south, and of Mediterranean forms on the coast. Variation in this class appears rapidly to follow segregation, as shown by the Jordanic species. The fluviatile mollusca are much more distinct, and indicate a very ancient separation from any adjacent district.

Similar inferences may be drawn from the examination of the Arachnida, Lepidoptera, Hemiptera, and Orthoptera, as well as from the Rhizopod fauna, which is similar to that of the Indian Ocean. (The examination of the Coleoptera is not yet completed.)

The flora of Palestine is, on the coastline and highlands, simply a reproduction of that of the Eastern Mediterranean. That of the Jordan valley is *most* distinct. Of 113 species by the Dead Sea, only 27 are European, and these chiefly weeds of world-wide distribution. In this area the flora is almost exclusively Ethiopian, consisting largely of species extending from the Canaries to India.

Thus in the Dead-Sea basin, an area of but a few square miles, we find a series of forms of life in all classes, differing from those of the surrounding region, to which they do not extend, and having Ethiopian and, more strictly, Indian affinities. The basin is depressed 1300 feet below the sea-level; and as zones of elevation correspond to parallels of latitude, so here a zone of depression represents the fauna and flora of a low latitude. If the flora were *representative*, this law, that climatal zones of life are mutually repeated and represented by elevation or depression and latitude, would account for their existence.

But we have a *transported* flora; this negatives the idea of an independent origin on the spot. The theory of migration, *under present conditions*, is refuted by the coexistence of peculiar and unique forms, with others now found in regions widely apart. Of these, the physical character, and the phenomena of their present distribution, present insuperable obstacles to their migration under *existing* geological conditions.

Their existence must be mainly due to dispersion before the isolation of the area; this must have been after the close of the Eocene period, to which belong the most recent superficial deposits of Southern Palestine. There are no beds synchronizing with the miocene deposits of Sicily, &c.; it must have had a fauna and flora contemporaneous with the miocene flora of Germany. There is geological evidence that since the Eocene period the Jordan fissure has had no connexion with the Red Sea or Mediterranean. There are *subsequent* vast marl deposits of the Dead Sea when it was at a higher level, but they are wholly unfossiliferous. The diminution of the waters may, for reasons given, be fixed about the close of the tertiary epoch. We have also evidence of the extension of the glacial period thus far south, as in the moraines of Lebanon.

Still the lake existed before the glacial epoch in its present form, when there was an unusually warmer climate, and the more antique Ethiopian fauna and flora had a more northerly extension. This would be contemporaneous with the miocene continent of Atlantis, and the Asturian flora of South-west Ireland.

Palestine would then be East African. Afterwards the glacial inroad would destroy the mass of preexisting life, excepting the few species most tenacious of existence which survive in the still comparatively warm depression of the Jordan valley, which thus became a tropical "outlier," analogous to the boreal marine outliers of our own seas. The Indian types are explained by the former continuous miocene continent from India to Africa. The peculiar species may either yet be found in Arabia, or, if not, may be descendants of species which inhabited the country with a limited range, or may be variations stereotyped by isolation.

The peculiar fishes of the Jordan are most important, dating probably from the earliest period after the elevation of the land. The genera of the peculiar species are exclusively African, while the species are *representative rather than* identical. We may explain this by the miocene chain of fresh-

water lakes, extending from Galilee to the Nyanza, Nyassa, and Zambesi, when an ichthyological fauna was developed suited to the warm conditions that prevailed, part of which survives in the Jordan.

During the glacial period the temperature of Lebanon must have been similar to the present Alps, as the existing mammals and birds on the summits are identical with those of the Pyrenees and the Alps; not so the glacial flora, of which almost every trace has been lost. But the flora had not the same powers of vertical migration with the fauna, of which, however, the Elk, Red Deer, and Reindeer, found in the bone-caverns, have long since perished.

During the present period the Mediterranean forms have overspread the whole country, excepting the mountain-tops at an elevation of 9000 feet, and the Jordan depression. These two exceptions can be best explained by the fact that the traces of the glacial inroad are not yet wholly obliterated, and that the preceding warm period has left its yet stronger mark in the unique tropical "outlier" of the Dead-Sea basin, analogous to the boreal outliers of our mountain-tops, the concave depression in the one being the complement of the convex elevation in the other.

II. "New Researches on the Dispersion of the Optic Axes in Harmotome and Wöhlerite, proving these Minerals to belong to the Clinorhombic (Oblique) System." By M. A. L. O. Des Cloiseaux. Communicated by Prof. W. H. Miller, For. Sec. R.S. Received March 12, 1868.

(Abstract.)

We are already acquainted with a considerable number of crystals, natural as well as artificial, the forms of which have only been determined with precision by the examination of their optical properties as doubly refracting bodies. Harmotome and Wöhlerite furnish two fresh examples of this; and they afford all the more important proof of the necessity of appealing to these properties, inasmuch as the crystals of these substances would appear certainly to be derived from a right rhombic prism, so long as we consider only the apparent symmetry of their external forms, or the orientation of the plane containing their optic axes. The different sorts of dispersion which these axes might be capable of presenting are so feeble, and so difficult of appreciation on account of the slight transparency of Wöhlerite, and the complex structure of the crystals of Harmotome, that the determination of these dispersions has hitherto been too incomplete to allow of any conclusion being drawn as to the crystalline type they might otherwise serve to characterize.

It was a remark of M. Axel Gadolin that induced the author to resume the attentive study of the phenomena of dispersion, first in Harmotome, and then in Wöhlerite, and as a consequence to modify the crystallographic type to which these minerals have been in general referred.

Harmotome.

Several years ago the author showed that simple crystals of Harmotome did not exist, and that those of Strontian in Scotland (Morvenite), considered as such, prevented, in fact, a twinning formed by the interpenetration of two principal individuals. The particular orientation of the plane of the optic axes in each of the crystals of which the least complicated of such groups are composed had led him to refer their crystalline form to a right rhombic prism of 124° 47′; and he had been induced to look on this prism as presenting a peculiar sort of hemihedrism, or rather hemimorphism, such that only one-half of the fundamental rhombic octahedron existed, that namely formed of four faces parallel two and two, and lying in the same zone. More recently, in studying the modifications which heat induces in the position of the optic axes and of their plane, he observed a phenomenon less compatible with the hypothesis of a primitive rhombic form; but the slight transparency of the plates on which he operated, the wide separation of the optic axes, which rendered the examination of the two systems of rings almost impossible in air, and finally the almost complete absence of dispersion, led him to regard the observed result as an apparent anomaly, attributable to the highly complex structure of the crystals.

Desirous of verifying the truth of a suggestion communicated to him by M. Gadolin in June 1867, the author had some new plates cut normal to the acute, *positive* bisectrix from very transparent crystals of the Scotch Morvenite, and he has been able to establish the existence of a very decided twisted dispersion. In consequence of the smaller mutual inclination of the optic axes in these than in the former plates, the author was also able to satisfy himself directly that the displacement impressed by heat on the plane containing these axes is a rotary one, quite analogous to that which he had shown to exist in borax and Heulandite. It is therefore now beyond doubt that the crystalline type of Harmotome is the oblique rhombic prism, and the author has corrected the crystallographic description of the mineral accordingly.

Wöhlerite.

In his 'Manual of Mineralogy,' the author had described the crystals of Wöhlerite as derivable from a prism of very nearly 90°. From the point of view from which a consideration of the orientation of their optic axes had induced him to regard them, they appeared to offer a certain number of homohedral forms associated with forms that were hemihedral or hemimorphic, analogous to those that he had drawn attention to in Harmotome. Having proved that the latter mineral belongs to the clinorhombic system, he endeavoured to ascertain whether this was not also the case with Wöhlerite, all the forms of which would in that event be homohedral. But in this case a study of the different varieties of dispersion is rendered difficult by the yellow colour, and by the imperfect transparency

presented by the substance even when in very thin plates. Besides this, contrary to what is found in Harmotome, while the dispersion belonging to the optic axes is very distinct, the *horizontal and twisted* dispersions, which should be sought for in plates normal to the two bisectrices, are, on the contrary, but slightly evident. However, on examining in oil some thin plates placed so as to have the plane of their optic axes horizontal and perpendicular to the plane of polarization, the author observed in the plates normal to the obtuse, *positive* bisectrix, some faint blue and red fringes, dispersed in contrary directions above and below the bars which traverse the two systems of rings, indicating the existence of an appreciable *twisted* dispersion. In the plates normal to the acute, *negative* bisectrix, the transverse bar of each system is bordered on one side by a very pale blue, and on the opposite side by an equally pale yellow, the horizontal dispersion being thus feebly indicated.

The crystals of Wöhlerite ought, then, to be referred to an oblique rhombic prism, in which the plane of symmetry is normal to the plane which contains their optic axes. The primitive form which it seems most convenient to choose is a prism with an angle of very nearly 90°, which presents a cleavage, easy though interrupted, parallel to its plane of symmetry, and cleavages which are more difficult in the directions of the lateral faces *m* and of the plane *h'* which is parallel to the horizontal diagonals of the base.

The author then describes in detail the crystalline form of Wöhlerite as thus corrected.

III. "On the Law of the Resistance of the Air to Rifled Projectiles." By Charles W. Merrifield, F.R.S., Principal of the Royal School of Naval Architecture. Received March 19, 1868.

(Abstract.)

At the beginning of this month Lieut.-Col. H. R. Halford applied to the author to obtain for him the law of atmospheric resistance resulting from his experiments in shooting with Metford's match-rifle, a small bore with increasing pitch. Col. Halford had determined by experiment the elevations required for the ranges 100, 200, &c. up to 1100 yards, each determination being derived from a very large number of shots; and the table of experimental elevations, corresponding to these various ranges, formed the datum furnished to the author.

As all the trajectories were very low, the greatest elevation amounting to only 2° 35′ 30″, the author assumed, as a sufficiently close approximation, that the vertical motion was determined solely by the force of gravity, and that the effect of the resistance of the air on the velocity was the same as if the projectile had moved strictly in a horizontal line. Consequently the depression of the point in which the target is struck, below the initial

tangent to the path, becomes a measure of the time of flight, according to the usual law of falling bodies; and the mean horizontal velocity being thus known for a series of different ranges, we can calculate the mean velocity for every 100 yards of a long range, and thence determine the resistance.

The author commenced his calculations from an assumed velocity of 1360 feet per second, in accordance with the results obtained at short ranges, and assumed for trial a resistance varying as the square of the velocity, but found that this law did not fit the results at all. A resistance varying as the cube of the velocity was then tried, and found very nearly to agree with the results of observation; and the agreement became, we may say, perfect, when the assumed initial velocity was slightly corrected.

As the calculations and experiments were all made without any notion of the resulting law, and without any knowledge of the work already done by Professor Hélie and Professor Bashforth, they afford a remarkable confirmation of the results obtained by those gentlemen. This is the more worthy of notice, as their data belong to pieces of large calibre, and the author's to small arms.

IV. "Remarks on the Great Nebula in Orion." In a Letter addressed to Prof. G. G. Stokes, Sec. R.S. By W. Lassell, F.R.S. Received February 28, 1868.

I have been so much interested by the perusal of Lord Oxmantown's observations and drawing of the Great Nebula in Orion, published in the present volume of the 'Transactions,' that I venture to offer you a few remarks upon them—the more readily, as I may be supposed to be somewhat familiar with that object, though observed with less advantage of optical power.

On comparing the present drawing with my own, made with the four-feet equatorial during my late sojourn at Malta, I find that of the 93 new stars in Lord Oxmantown's list, there are, I believe, only 24 within the more limited area of my drawing. A good many of these have escaped my notice, while, on the other hand, I have detected several which I do not find in the present Catalogue. The following are instances of a few:—Three stars north-preceding No. 119; four stars about the hypothenuse of the nearly right-angled triangle formed by the stars 47, 52, and 53; two stars in the triangle formed by 30, 32, and 35; a delicate point about 20″ from 104, with several others. Some of these I have so repeatedly and certainly seen, that I wonder not to find them here. The explanation of this discrepancy may possibly in some instances be found in variability, but must, I think, be rather sought for in the influence which the state of atmosphere has on such delicate objects, when the highest combinations of ht and power are brought to bear upon them. As this is a nebula of

great extent, it is possible, and indeed probable, that there may be some spots on which the concentrated attention of any single observer has not been given under the most favourable circumstances. In my own observations, the question of resolvability appeared always an interesting one to settle, and therefore I gave quite as much attention to the detection of stars as to the tracing of the nebula.

The present drawing embracing much more of the extent of this nebula than mine (if not, indeed, the whole of it), I am able to compare mine with it only in part; but, so far as my limits extend, the representations are generally very coincident. In some parts there is a greater *hardness* than I have seen; but it is scarcely possible to convey a *true* impression of the form of the convolutions of the nebula without intensifying them in some degree. I recognized in my own observations more of the spiral or scroll-like character of the nebula about the stars 51 and 57, first pointed out, I believe, by Mr. Bond, than I find in this drawing. In an engraving sent to me by Mr. Bond in 1863, in which photography had, as I understood, been employed, this scroll-like appearance is strongly marked.

On the question of a real change of form of the nebula, no positive conclusion can, I think, as yet be arrived at; but as we have probably now reached much more nearly to the practical limit of optical power in our telescopes, future observations may be expected to be much more comparable with existing drawings than these are with those formerly made.

The evidence of *resolvability* seems to me to be rather on the negative side—my own deductions from what I have seen have been always in that direction; and such of the present observations as apparently look the other way are, I consider, too vague and wanting in precision and certainty, to establish it positively.

[Received April 17, 1868.]

In the account of my observations in Malta with the four-foot equatorial, published in the 36th volume of the Memoirs of the Royal Astronomical Society, there is but a slight reference to the Great Nebula of Orion; the immediate reason for which was, that the drawing I had made was on too large a scale for appearing therein as an engraving, and I was unwilling to subject it to a reduction of size.

Moreover, as Lord Oxmantown's elaborate description and drawings of this incomparable object have recently appeared in the Transactions of the Royal Society, and as the subject may be presumed to be a very interesting one, I beg leave to request of the Society their acceptance of my original drawing (forwarded this day), which contains the sum of all I have been able to make out (both of the details of the nebula and of the stars therein) with the above telescope during my three-years' residence in the Island of Malta.

I also subjoin from my Journal the following few notes relative to some

of the more interesting stars. The fifth star of the trapezium is reddish, and has a pretty broad and not very brilliant disk,—much larger than that of the sixth, which is more brilliant, and whiter. Yet the first impression of the fifth on the eye is more forcible than that of the sixth.

Adopting Herschel's numbers, 57 is much less bright than 51. The star marked II is nearly as bright as 57; but the two stars south-following II are the smallest points visible.

Of the delicate pair preceding δ of the trapezium, the following star is the brightest.

The minute stars preceding and north-following 93, were undoubtedly verified; but there seemed a want of precision in the image of the latter (if one may so speak of such a faint thing) which suggested the idea of its being double.

Herschel's 78, 82, and 91 have on no occasion been recognized; a star, however, has been seen as laid down in the drawing, a very little south-following the place of 78.

In the drawing, Herschel's numbers are adopted throughout; and for convenience of reference and identification, they are attached in minute figures to each star. Its scale, as given at the foot, is $100'' = 1{\cdot}194$ inch. All the stars which have been certainly and repeatedly seen are inserted, and, I believe, no others. Some attempt at indication of respective magnitude has been made in depicting the stars, but their estimated magnitudes are more precisely given in the margins, right and left of the picture. They are, however, more fully laid down in the following Catalogue of the stars contained in the map, the places being principally taken from Liaponov's measures. A good many of the stars, however, I have independently measured, and though my means for this especial purpose were probably inferior to those of M. Liaponov, I have added a list of my measures for comparison or identification.

W. Lassell.

Ray Lodge, April 1868.

Catalogue.

L. = Liaponov. | Ll. = Lassell.
S. = Struve. | H. = Herschel.

h. 78, 82, 90, 91, and (V.) not seen.

Magnitude.	Distance from θ.			Designation and Authority for Place.	
	″	″			
[illegible]	preceding 1047	171	S.		
9	975	12	N.	h. 5	L.
12·5	943	333	S.		
13	938	408	N.	place uncertain.	
8·5	927	676	N.	h. 6	H.
13·5	926	657	S.		
10·5	922	310	S.	h. 8	L.
8	910	823	S.	h. 7	H.
13	906	556	S.		
9	888	816	S.	h. 9	H.
12·5	870	630	S.		
12·5	862	653	[illegible]		
9	849	241	[illegible]	h. 10	L.
12·5	843	439			
9	829	945	S.	h. 11	H.
12	823	461	S.	h. 15	S.
9	769	64	N.	h. 12	L.
12	749	69	S.	h. 13	L.
11	729	73	S.	h. 14	L.
10·5	722	525	S.	h. 16	L.
11·5	718	110	S.		
13	710	518	N.	place uncertain.	
13·5	708	494	N.	place uncertain.	
10	703	46	S.	h. 17	L.
13	696	634	N.	place uncertain.	
14	691	657	N.	?	
9	686	253	S.	h. 18	L.
13·5	671	487	N.	place uncertain.	
13·5	654	718	S.		
11·5	647	15	N.	h. 19	L.
11	631	312	S.	h. 21	L.
13·5	626	697	S.		
12	610	538	S.	h. 22	
13	594	768	S.		
13	579	694	S.		
13	567	412	S.		
10	561	786	N.	h. 25	H.
12	549	647	N.	h. 28	H.
10	547	1036	S.	h. 29	H.
9	546	71	S.	h. 27	L.
10	544	208	S.	h. 26	L.
12	531	32	N.	h. 30	L.
12·5	508	178	N.		
8·5	493	289	N.	h. 32	L.
13·5	478	747	N.		
13	477	542	[illegible]		
10·5	466	108	[illegible]	h. 33	L.
13	462	756	N.		
13	462	144	N.	?	
12·5	459	963	S.		
13·5	453	82	S.		
[illegible]	432	659	S.	h. 34	L.
12·5	preceding 424	844	S.		

Catalogue (continued).

Magnitude.	Distance from θ.			Designation and Authority for Place.	
	″	″			
13·5	preceding 400	727	N.		
8·5	398	273	N.	h. 35	L.
12	384	46	S.	h. 36	L.
13·5	382	115	S		
9	357	592	S.	h. 37	L.
13·5	312	510	N.	h. 39	H.
7·5	311	424	S.	h. 40	L.
10	306	5	N.	h. 38	L.
12	292	498	S.	h. 43	L.
12	290	693	N.	h. 42	H.
12	282	31	S.	h. 41	L.
12	247	16	N.	h. 44	L.
9	243	117	S.	h. 45	L.
13	222	824	N.		
13	221	471	N.	h. 46	H.
13	207	589	S.		
13	206	889	S.		
12	197	400	S.	h. 47	L.
13	184	335	S.		
14	184	805	N.		
9	184	909	N.	I.	L.
13	178	379	S.		
9·5	175	511	N.	h. 48	L.
14	168	303	S.		
6·5	163	666	N	h. 49	L.
10·5	160	119	S.	h. 50	L.
12·5	159	238	S.		
13·5	116	749	S.		
11·5	107	396	S.	h. 52	L.
14	100	15	S.		
8	95	272	S.	h. 53	L.
13	90	513	S.	?h. 55	
12	88	177	S.	h. 54	L.
12·5	85	24	S.	h. 51	S.
11	79	387	N.	h. 56	L.
13·5	75	157	S.		
11·5	65	891	S.	?h. 58	
13·5	57	511	S.		
13·5	57	23	S.	h. 57	S.
?13·5	55	434	S.		
13	49	786	S.		
11·5	37	453	S.	h. 62	L.
10·5	34	966	S.	h. 59	H.
12	34	769	S.	h. 60	H.
13	26	993	S.		
12	22	947	S.		
14	17	18	N.		
14	12	19	N.		
11	10	12	N.	h. 64=E.	L.
12	9	949	S.	h. 63	
7	9	8	N.	h. 65=A.	L.
13·5	9	504	N.	h. 66	
12·5	7	28	S.	H.	S.
8	5	16	N	h. 67=B	L.
14	0	35	S.	? nebulous patch.	
4·5	preceding 0	0		h. 69=θ.	L.
11·5	following 3	2	S.	h. 71=F.	S.

Catalogue (continued).

Magnitude.	Distance from θ.	"	"		Designation and Authority for Place.	
9·5	following	3	953	S.	h. 74	L.
9·5		6	98	N.	h. 70	L.
14		7	10	S.		
13·5		8	107	N.	III.	S.
14·5		11	47	N.	?	
6		12	7	N.	h. 73=D.	L.
14		16	596	S.		
....		19	8	S.	?	
13·5		20	575	S.		
13		21	968	S.		
12·5		21	39	N.	h. 75	S.
11		25	407	N.	h. 79	L.
13·5		28	48	N.		
11·5		30	172	N.	h. 76	L.
11·5		34	433	N.	h. 83	L.
11·5		36	165	N.	h. 80	L.
12		54	146	N.	h. 84	L.
12·5		57	196	S.	?h. 81	S.
13		58	856	S.		
8·5		61	851	N.	h. 85	L.
9		62	100	N.	h. 87	L.
9·5		63	674	N.	h. 86	L.
12		69	24	[illegible]	h. 88	L.
13·5		70	200	S.		
14		74	430	S.	?	
11		74	854	N.		
13·5		76	467	S.		
14		77	96	[illegible]		
13·5		78	27	S.		
12		81	173	N.	h. 89	L.
14		88	109	S.	?	
5		98	94	[illegible]	h. 93	L.
12		102	673	[illegible]	h. 92	H.
13		102	742	N.		
10		117	442	S.	h. 95	L.
14		117	79	S.		
13		122	743	N.	h. 94	
12		126	814	N.	h. 97	
13		139	734	N.	h. 96	
13·5		140	427	S.		
10		142	494	N.	h. 102	L.
10		146	612	N.	h. 99	
10·5		146	884	N.	h. 98	
11		149	134	[illegible]	h. 100	L.
10·5		149	251	S.	h. 103	L.
6		150	96	S.	h. 101	L.
14		160	782	N.		
14		170	185	S.		
14·5		178	95	S.		
8·5		181	174	S.	h. 104	L.
12		185	693	S.	h. 105	H.
11·5		202	824	N.	h. 107	H.
11		208	567	S.	h. 106	L.
14·5		209	130	S.		
14		212	59	[illegible]		
13		214	273	N.	h. 109	L.
5·5	following	217	445	N.	h. 108	L.

Catalogue (continued).

Magnitude.	Distance from θ.				Designation and Authority for Place.	
		″	″ n-f.			
11·5					h. 108	
14·5	following	223	427	S.	only seen	24th Nov. 1864
7·5		225	110	S.	h. 110	L.
8		230	582	S.	h. 111	L.
		237	161	S.	? nebu	lous knot
13·5		242	340	S.		
10·5		244	465	S.	h. 112	L.
13·5		246	406	S.		
12·5		272	870	N.	?	
14		280	474	N.		
8		281	669	N.	h. 113	L.
14·5		285	460	N.		
13·5		286	345	S.		
12·5		299	129	N.	h. 114	H.
14		312	650	S.	?	
13		313	850	S.	? h. 115	
13·5		316	878	N.		
12		319	184	S.	IV.	Ll.
13·5		340	882	N.		
13·5	following	344	366	N.	h. 116	
13·5	var.	359	243	S.		
12·5		360	624	N.		
11·5		362	213	S.	h. 117	L.
12	var.	369	691	N.	h. 118	H.
11	following	369	846	N.	h. 119	H.
9·5		370	196	N.	h. 120	
10		385	284	S.	h. 123	L.
12·5		385	742	S.	h. 122	H.
9·5		387	588	N.	h. 124	L.
11·5		409	778	S.	h. 125	L.
13		417	182	N.		
		417	669	N.	?	
10·5		417	753	N.	....	H.
11		418	514	S.	h. 126	L.
13		437	617	N.		
12·5		453	334	N.	h. 128	S.
10		459	393	N.	h. 129	L.
13		504	714	S.	h. 131	
10		505	1015	S.	h. 132	H.
8		511	303	S.	h. 133	L.
12		514	418	N.	h. 130	
13		518	499	N.		
13		532	319	S.	h. 134	H.
13·5	following	563	185	S.		
6	dup.	574	851	S.	h. 135	L.
14·5		593	33	S.	?	
13·5		597	96	S.		
7·5		628	64	N.	h. 136	L.
10		641	984	S.	h. 138	H.
14		656	25	N.		
12		666	364	N.	h. 137	Ll.
12		784	267	N.	h. 141	H.
10·5		797	254	S.	h. 142	L.
14		877	831	S.		
5·5	following	889	916	S.	h. 143	L.

Remeasurement of some of the Stars of the above Catalogue.

Stars South of θ Orionis.			Stars South (*continued*).		
	″	″		″	″
h. 17	preceding 702·7	42·8	h. 54	preceding 85·7	175·1
18	683·7	244·1	62	39	449·3
26	536·2	207·6	88	following 66·7	24·4
27	543·3	71·8	91	112·8	53·8
33	463·3	113·6	93	96·3	93·2
36	382·7	50·6	95	113·9	440·5
37	355·3	583·3	103	150·1	248·1
40	317·2	420·9	101	151·5	97·2
41	270·6	36	104	178·7	170·5
43	290·5	483·4	111	227·1	579·1
45	241·8	117·9	112	239·6	457
47	192·6	397·8	117	359·5	213·2
50	163·6	116·7	123	380·4	284·4
52	105·5	398·3	126	412·6	503·5
53	91·4	273·1	133	507·1	301·7
51	preceding 84·2	21·8	142	following 783·6	254·9

Stars North of θ Orionis.			Stars North (*continued*).		
	″	″		″	″
h. 12	preceding 776·8	70·5	h. 86	following 63·4	669·0
19	639·9	10·2	87	61·1	99·8
32	492·8	286·4	99	148·8	611·2
35	400·2	270·4	102	144·6	488·0
38	304·0	0·0	109	214·4	272·8
44	246·8	8·6	108	210·7	440·3
48	172·9	502·1	113	277·7	658·9
49	161·2	663 9	120	367·3	195·3
56	preceding 86·1	380·4	124	384·8	584·7
70	following 9·0	96·6	129	459·0	387·9
75	24·1	41·5	136	625·5	65·7
76	32·2	166·3	137	following 666·1	363·9
79	following 21·0	400·4			

April 30, 1868.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "Observations on the Development of the Semilunar Valves of the Aorta and Pulmonary Artery of the Heart of the Chick." By MORRIS TONGE, M.A., M.D. Communicated by Dr. BEALE. Received March 24, 1868.

(Abstract.)

Kölliker is the only embryological author in whom I have found any information about the development of the semilunar valves of the aorta and

pulmonary artery, and I have not been able to discover any observations later than his. After speaking of the formation of the aorta and pulmonary artery by the division of the *truncus arteriosus* into two vessels, this being, as is well known, the large single arterial trunk conveying the blood from the rudimentary ventricle into the branchial arteries, he says*, "Simultaneously with the division the semilunar valves also become developed, and I saw them already present in both arteries in an embryo of the seventh week. They are, however, at first nothing but horizontally projecting crescentic growths of the middle and of the epithelial coats by which the *lumen* † at this spot receives the form of a three-rayed star. At what time they first become visible as distinct pockets I have not yet investigated."

The division of the *truncus arteriosus* is described by Rathke as occurring in birds and mammalia by the formation on its interior of two oppositely situated longitudinal ridges, which then grow together throughout its whole extent and completely divide the vessel into two lateral halves, one representing the commencement of the aorta, and the other that of the pulmonary artery. Though the semilunar valves are said by Kölliker, and quite correctly, to develope simultaneously with the division, he gives no information about the manner in which they are connected with it, or the part of the vessel in which they originate, and nowhere are any drawings given of them in their rudimentary state. I was hence led to conclude that very little was known about this point, and to make the observations the results of which are here recorded. They seem to me valuable, as throwing light on some of the congenital malformations of this part of the heart. They were made during 1865, 1866, and 1867, on the embryos of the common fowl, and I have had no opportunity of investigating human or other mammalian embryos with reference to this point. But from the great likeness between the hearts of birds, mammalia, and man at different periods of their development, it seems pretty certain that the arterial semilunar valves in man and mammalia generally must pass through the same stages of development as those of the bird, which, in the fully developed state, quite resemble them.

The eggs were incubated by artificial heat, and the hearts of more than fifty embryos, at various stages of development, were examined. The embryos were prepared by immersing them, immediately on their removal from the egg, in strong alcohol. By this the large vessels were obtained distended with blood and hardened. They were afterwards rendered transparent by soaking them in strong glycerine, in which they were dissected

* Kölliker, 'Entwickelungsgeschichte des Menschen,' pp. 404, 405 (1861).

† I have left this word untranslated because no single English word exactly expresses its meaning. It is obviously the bright area of the interior of a transverse section of the vessel held up to the light. The boundary of the bright area shows the form of the vascular canal at this point.

and examined by strong transmitted light, and were afterwards mounted in glycerine jelly.

The new facts observed demonstrate—

(1) The manner in which the *truncus arteriosus* divides into two vessels, which is different from that commonly supposed to occur.

(2) The close connexion between this process of division, and the formation of the semilunar valves of the aorta and pulmonary artery, and their place of origin and mode of development.

The following is a brief account of the manner in which the division of the *truncus arteriosus* takes place. It should be said that about the third day of incubation, just before the division begins, the somewhat spirally twisted truncus arteriosus is everywhere smooth, and free from ridges on its interior, and ends abruptly in the three pairs of branchial arteries which then exist. These are the third, fourth, and fifth pair. There is no valvular apparatus at its branchial end, but next the ventricle the deficiency of valves seems to be supplied by a considerable development of the elastic wall of the truncus arteriosus on its two opposite sides, so that the ventricular aperture, which is at first circular, becomes slit-shaped. The two lips of the slit seem to prevent in great measure the reflux of blood into the ventricle, before the semilunar valves are sufficiently developed to do so.

The division of the vessel commences about the 106th hour of incubation, at rather less than one-fifth of the whole period of incubation, which is 21 days.

It begins at the branchial end of the truncus arteriosus by the extension into it of a plane septum growing horizontally downwards into the vessel from the terminal arterial wall between the openings of the fourth and fifth pair of branchial arteries. Its lower margin is forked, so that it extends further along the sides than along the centre of the vessel, and it is inclined a little obliquely across the vessel, sloping downwards from left to right. The little channel in front of this septum leads to the third and fourth pair of branchial arteries, and is the rudimentary aorta; the channel behind it leads to the fifth pair of branchial arteries, and is the rudimentary pulmonary artery.

At the same time, or slightly before this, the canal of the vessel just below the septum becomes constricted by the formation—

(1) On its anterior and left surface, of two flattened prominences, separated by a groove. These are the rudiments of the anterior semilunar valve of each artery.

(2) On its anterior and right surface, of a flattened ridge, extending obliquely across the vessel nearly opposite to the anterior valve rudiments, afterwards becoming prominent and pyramidal in the centre, and extending gradually down the posterior surface of the vessel. The right and left ends of this ridge are the rudiments of the inner semilunar valves of each artery. As these growths enlarge, the forked septum grows downwards into the

artery, twisting gradually from left to right, its left leg passing between and separating the anterior semilunar valve rudiments, and its right leg growing into the central portion of the oblique ridge on the posterior surface, now becoming prominent and pyramidal, and separating from each other the rudiments of the inner valves. Between the outer and inner semilunar valves in each artery there is a vacant space left on the wall of the vessel, from which the outer semilunar valve in each artery afterwards grows out, the outer valves appearing later than the others. The division of the truncus arteriosus proceeds by the gradual growth downwards of the forked septum along the course of the ridge on the posterior surface, which gradually becomes more prominent, the right leg of the fork, which proceeds along it, being always a little in advance of the other. The anterior or left leg of the fork corresponds with the right margin of the anterior aortic valve, and terminates almost immediately on the anterior surface, no ridge being formed along the anterior surface as there is along the posterior. As the forked septum between the aorta and pulmonary artery grows down the vessel, the semilunar valves gradually become more developed, and the rudiments of the outer valves appear. They appear soon after the 117th hour of incubation, by which time the aorta and pulmonary artery are separated for some little distance.

During these changes the aperture into the ventricle has become a rectangular slit, passing horizontally backwards and to the right, and having a left-hand and a right-hand lip, the left-hand lip sloping from before backwards and upwards into the artery, and joining the lower end of the ridge that has been gradually forming on the posterior surface of the vessel. As the division proceeds the ends of each lip of the ventricular slit disappear, and the central portions, especially of the left-hand lip, become more prominent. By this a channel is left in front and towards the left, and behind and to the right. By the time the division has descended to the ventricular aperture, the original right-hand leg of the forked septum has wound round to the centre of the left-hand lip of the slit, the left-hand leg to the centre of the right-hand lip, so that the aortic channel has passed from front to back, and the pulmonary channel from back to front, the anterior portion of the ventricular slit thus becoming the root of the pulmonary artery, and the posterior portion the root of the aorta.

The septum of the ventricles has been gradually forming during the process of division of the truncus arteriosus, and by the time the division and valves have descended nearly to the base of the ventricles, there remains merely an oval aperture in the upper portion uniting the ventricular cavities. It forms a short canal with a left ventricular border and a right ventricular border. The arterial infundibula are finally separated from each other by the union of the lower half of this right ventricular border with the lower border of the forked arterial septum. The anterior portion of the right ventricular border is continued upwards and forwards into the

termination of the original right leg of the fork in the central part of the left-hand lip of the ventricular slit, while the posterior portion passes off slantingly upwards and forwards as a ridge, which forms the termination of the original left leg of the fork in the central part of the right-hand lip of the slit. Thus a twisted, hourglass-shaped aperture connects the arterial infundibula, by whose closure the pulmonary infundibulum and root of the pulmonary artery become separated from the root of the aorta and the canal of the aperture in the septum, which then becomes the aortic infundibulum. This process is completed about the end of the eighth day. The separation of the vessels does not become visible externally till it has advanced a considerable distance down the truncus arteriosus, and the semilunar valves are considerably developed.

The division of the truncus arteriosus into the aorta and pulmonary artery does not therefore take place by the formation of *two* oppositely situated longitudinal ridges, and their subsequent growth together, but occurs, as above described, by the extension into it of a plane septum from between the fourth and fifth pair of branchial arteries, and which twists down the vessel along the line of a single thick pyramidal ridge which forms gradually on its posterior aspect.

The formation of the semilunar valves is very closely connected with the process of division of the *truncus arteriosus*, and the following are the new facts arrived at with respect to their origin and development :—

1. It is a remarkable fact that the rudiments of the semilunar valves first appear on the interior of the truncus arteriosus *at a considerable distance from the heart*, near the termination of the truncus arteriosus in the branchial arteries, and not near the heart, as one might have been led to expect.

2. It is also very remarkable that the rudiments of the anterior and inner semilunar valves of each artery make their appearance before the partition, which has already begun to separate the aorta from the pulmonary artery, has quite descended to that part of the truncus arteriosus in which these valves originate.

3. The rudiments of the anterior semilunar valves of the aorta and pulmonary artery are the first to appear, those of the inner valve of each artery the next, and those of the outer valves the last. The development of the last valve to appear remains behind that of the others throughout.

4. The anterior valve-rudiments appear close together, rather on the right side of the anterior surface of the truncus arteriosus, about the 106th hour of incubation, simultaneously with the commencement of the division, and a short distance below it, and opposite the commencement of the ridge which forms on the posterior surface of the vessel, and which appears about the same time.

5. The rudiment of the inner valve of each artery grows from the

corresponding side of the ridge which forms gradually on the posterior surface of the vessel, a little later than the anterior valves.

6. The rudiment of the outer valve in each artery arises from that part of the inside of the wall of the truncus arteriosus left vacant between the outer margins of the rudiments of the anterior and inner valves soon after the 117th hour of incubation. It arises level with the other valves, when the aorta and pulmonary artery are already separated from each other for some little distance, and therefore a little nearer to the heart than the other valves, though still at a considerable distance from it.

7. The anterior valve-rudiments commence as transverse thickenings of the interior of the vessel, sloping off above and below into the general surface of the vessel, and are separated by a slight groove.

8. The inner and outer valves first appear as simple pyramidal thickenings of the vascular wall.

9. All the semilunar valves are solid at first.

10. The anterior and inner valves consist of one single segment for each valve.

11. The outer valve is at first a single pyramidal eminence. It may remain single, or become deeply notched and develope into two valves, or even more.

12. By the time the third valve in each vessel has appeared, the form of the valves has become more defined. They then have the shape of a short crystal of triple phosphate , its flat surface being attached, its edge projecting into the vessel, and its ends sloping off upwards and outwards above, and downwards and outwards below. The valves are more developed in the direction of their length than transversely, and their course down the wall of the vessel is parallel to that which the axis of its canal afterwards assumes.

13. About the 144th hour of incubation they are (though still solid and at some distance from the heart) sufficiently developed to close the canal of the vessel pretty completely, and to prevent much reflux of blood into its undivided portion.

14. By this time the valvular function of the two lips of the opening into the ventricle has become abolished.

15. The valves are further developed by the hollowing out of the solid pyramid above and near the wall of the vessel, while they grow in other directions.

16. The pocketing of each valve commences in each in the order of its appearance, and begins in the anterior and inner valves of each artery about the time that their bases have descended to the level of the bases of the ventricles, *i. e.* at the 147th hour of incubation, and is distinct in these valves at the 165th hour. The pocketing of the outer valves is not distinct till much later. About the time that it commences, the valves

have assumed nearly their final positions with respect to the base of the heart, and the aperture of communication between the arterial infundibula is nearly closed up.

17. After the complete separation of the aortic and pulmonary infundibula from each other, the further changes in the semilunar valves consist principally in increase in size and diminution in thickness, so that they become more and more membranous, *pari passu*, with the growth of the other parts of the heart.

In the description given above of the division of the truncus arteriosus, it has been shown that the aperture in the septum of the ventricles does not close up entirely as is commonly supposed, but finally developes into the aortic infundibula.

The fifth vascular arch on each side gives off the branch to the lung of that side, and becomes ultimately the corresponding branch of the pulmonary artery, according to the view long ago propounded by Von Baer.

In conclusion I must thank several kind friends for assistance received from them during the preparation of this paper, which I here beg leave to acknowledge. In particular Dr. Beale, who has given me much valuable advice throughout; the Rev. George Kempson and my cousin Mr. Charles Paddison, who sent me abundant supplies of fresh eggs; and Dr. Cayley, who kindly revised the translations from the German authors referred to.

II. "On the Phenomena observed to attend the propulsion of Lymph from one of the Lymphatic Hearts into a Vein in the Frog." By THOMAS WHARTON JONES, F.R.S., Professor of Ophthalmic Medicine and Surgery in University College, &c. Received March 28, 1868.

(Abstract.)

An anæmic frog, killed, as regards sensation and voluntary motion, without stoppage of the circulation, by plunging into water at 110° or 120° Fahr., was laid open, and the posterior part of the anterior lymphatic heart of one side, in the niche behind and below the extremity of the large transverse process of the third vertebra, brought into view. By the removal of the skin of the back from over the scapular region, the part of the heart mentioned admitted of examination by transmitted light under a simple microscope—the lens ½-inch focus. It was seen that when the lymphatic heart contracted, a stream of lymph was propelled from it into a vein at its posterior border, and swept before it the blood in that vessel, whilst the flow from behind was arrested. As soon, however, as diastole of the lymphatic heart supervened, the flow of blood from behind became re-

established, and drove the lymph onward in its turn. Systole of the heart now again ensuing, the lymph-stream propelled into the vein swept forward the blood in that vessel as before, whilst the flow of blood from behind was arrested; and so the same series of phenomena was repeated.

It was thus seen that the phenomena attending the propulsion of lymph from the anterior lymphatic hearts of the frog into the veins at their posterior border, with which they communicate by a valvular opening, are essentially similar to those attending the propulsion of the lymph from the caudal heart of the eel into the caudal vein.

The vein at the posterior border of the heart, after receiving the lymph, turned behind the large transverse process of the third vertebra, and passed forwards along the inner to the anterior border of the heart, where it inosculated with the large blackish vein which runs up on the side of the neck.

This large blackish vein was described by Professor Johannes Müller as issuing from the heart; but the author has not found it to do so. It is merely in close connexion, so that it is dragged backwards by communication of the movement of the heart in contracting, and recoils forwards into its previous position when diastole takes place.

III. "Researches on Solar Physics. Heliographical Positions and Areas of Sun-spots observed with the Kew Photoheliograph during the years 1862 and 1863." By Warren De la Rue, Ph.D., F.R.S., F.R.A.S., Balfour Stewart, LL.D., F.R.S., F.R.A.S. (Superintendent of the Kew Observatory), and Benjamin Loewy, F.R.A.S. Received March 31, 1868.

(Abstract.)

In this paper the sun-pictures taken by the Kew photoheliograph for the years 1862 and 1863 are discussed; the heliographic latitude and longitude of every spot is given, and the area of each group on each day when it was observed is expressed in millionth parts of the sun's whole hemisphereal area. The Kew photoheliograph itself, as well as the instrument invented by Mr. De la Rue for measuring sun-pictures, have been already described by Mr. De la Rue in the Bakerian Lecture for 1862. These descriptions are not therefore repeated in this paper; but, on the other hand, the method by which the heliographic position of spots is deduced from the measurements made is given at considerable length.

The results of succeeding years, and their final discussion with reference to the sun's elements, will be published hereafter.

IV. "The Specific Heat of Mixtures of Alcohol and Water." By A. DUPRÉ, Ph.D., Lecturer on Chemistry at the Westminster Hospital, and F. J. M. PAGE. Communicated by C. BROOKE, M.A. Received March 26, 1868.

(Abstract.)

The authors have examined a number of mixtures of alcohol and water. They show that the specific heat of these mixtures, up to an alcoholic strength of about 36 per cent., is higher than the specific heat of water itself.

Two methods were employed for estimating the specific heat exactly opposite in principle.

The first consisted in heating a metallic weight to a certain temperature in a steam-oven, similar to that employed by Regnault in his researches, and then plunging it into the liquid the specific heat of which is to be estimated. The rise in the temperature of equal quantities of different liquids produced by the introduction of the same weight, heated to the same temperature, is inversely proportional to the specific heat of such liquids.

Two weights and several calorimeters of different sizes were used. One of the weights was made of brass and weighed 246·49 grms., the other was of copper gilt weighing 614·49 grms. Both weights were made in the form of stout rings, and in the inner cylindrical opening of each a small fan-wheel was inserted.

These rings, after being heated and let down into the calorimeter, were attached to a strand of worsted, and held freely suspended in the liquid of the calorimeter. The worsted had previously been twisted, and when now allowed to untwist it causes a rapid rotation of the ring. The fan-wheel fixed inside the ring thereby produces a current, which, passing through the ring, not only serves to mix the liquid thoroughly, but also considerably facilitates the rapid cooling of the weight.

The calorimeters, as usual, consisted of cylindrical vessels made of very thin polished brass, supported on stretched silk cords, and surrounded by a double cylinder of tin-plate to prevent, as far as possible, any gain or loss by radiation.

The temperature of the liquid was taken by a small thermometer, having a bulb 60 millims. long and about 2·5 millims. diameter. Each degree was divided into twenty parts, and by means of a telescope $\frac{1}{200}$ of a degree could be read off.

The authors give experiments which prove that the high specific heats observed are not due to evaporation caused by the introduction of the heated metals into the calorimeter.

The second method used was that generally employed. A certain weight of the liquid, the specific heat of which is to be estimated, enclosed in a suitable vessel, is heated and then plunged, vessel and all, into a

calorimeter containing a known weight of distilled water. The temperature of the calorimeter will rise, owing to the introduction of the heated liquid, and the elevations in temperature produced by different liquids will, in this case, be directly proportional to their specific heats.

The following Tables give the means of the various results obtained.

Four series of experiments were made. In the first series the brass weight was employed; it was heated to a temperature of about 98° C. In the second and third series the copper weight was used, heated to about 98° and 42° C. respectively. The fourth series was conducted in the ordinary manner.

Specific heat of

5 per cent. spirit	Series II.		101·5
10 per cent. spirit	Series I.	103·55	
	Series II.	103·49	
	Series III.	103·83	
	Series IV.	103·71	
	Mean		103·64
20 per cent. spirit	Series I.	104·16	
	Series II.	104·27	
	Series IV.	104·49	
	Mean		104·30
30 per cent. spirit	Series II.		102·47
36 per cent. spirit	Series II.		99·90
43 per cent. spirit	Series II.		97·59
83 per cent. spirit	Series II.		65·88

The authors finally draw special attention to the circumstance that the specific heat of these mixtures not only rises in some cases (up to an alcoholic strength of 36 per cent.) above the specific heat of water, but is above the calculated mean specific heat up to an alcoholic strength of about 74–80 per cent.; beyond which it seems slightly below the calculated mean according to the researches of Regnault and Kopp.

The maximum elevation above the calculated mean coincides pretty closely with the point of maximum contraction.

May 7, 1868.

Dr. W. B. CARPENTER, Vice-President, in the Chair.

In conformity with the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows:—

John Ball, Esq., M.A.
Henry Charlton Bastian, M.D.
Lieut.-Colonel John Cameron, R.E.
Prof. R. Bellamy Clifton, M.A.
Morgan William Crofton, Esq., B.A.
Joseph Barnard Davis, M.D.
P. Martin Duncan, M.B.
Peter Griess, Esq.
Augustus George Vernon Harcourt, Esq.
Rear-Admiral Astley Cooper Key, C.B.
Rear-Admiral Erasmus Ommaney, C.B.
James Bell Pettigrew, M.D.
Edward James Stone, Esq., M.A.
Rev. Henry Baker Tristram, M.A.
William Sandys Wright Vaux, Esq., M.A.

The following communications were read:—

I. "Researches on the Blood.—On the Action of Nitrites on the Blood." By Arthur Gamgee, M.D., F.R.S.E., Assistant to the Professor of Medical Jurisprudence in the University of Edinburgh. Communicated by Prof. Frankland, F.R.S. Received April 1, 1868.

(Abstract.)

The paper commences with a statement of the facts with which we are at present acquainted, relating to the nature and character of the blood-colouring-matter, and its relation to gases.

I. The action of nitrites in modifying the colour and spectrum of blood is then described. Under the influence of nitrites, arterial blood assumes a chocolate coloration. Coincidently the bands of scarlet cruorine (or oxidized hæmoglobin) become very faint, and an additional absorption band, occupying the same position as that of acid hæmatin, appears. The addition of ammonia to blood in which nitrites have induced the characteristic change of colour and spectrum, causes the red colour to return and gives rise to a new spectrum in which the normal blood-bands are again better defined, but accompanied by a faint and rather undefined absorption band in the orange. It appears from the experiments of the author that the change in optical properties induced by ammonia is not due to any decomposing action exerted upon the body formed under the influence of nitrites; for on neutralizing the solution to which ammonia has been added, the original spectrum is reproduced. When sulphide of ammonium, or a reducing-solution of iron is added to a blood solution which has been acted upon by nitrites, all effects of their action disappear, and the solution again possesses the spectrum of oxidized blood-colouring-matter, although precau-

tions have been taken to exclude atmospheric air. The continued action of the reducing-solution then leads to the reduction of the blood-colouring-matter, which when shaken with air again yields the perfectly normal spectrum of blood. It would therefore appear that when nitrites act upon the blood-colouring-matter they do not *decompose* it, nor thrust out or remove the loose oxygen with which it is combined.

II. The author then describes a series of experiments instituted with the object of determining whether blood which has been acted upon by nitrites has lost its power of combining with the atmospheric oxygen. The apparatus and methods used are described, and it is shown that the amount of oxygen which nitrite-blood absorbs is much smaller than that absorbed by normal blood.

III. In the next series of experiments the author made use of carbonic oxide gas as a reagent to indicate whether after the action of nitrites the loose oxygen of the colouring-matter is still capable of expulsion by CO. With this object the blood was arterialized by agitation with air and treated with a solution of a nitrite. After some time it was brought in contact with a measured volume of pure carbonic oxide; after being well agitated and allowed to remain in contact with it for some time, the gas was removed and analyzed. It was found in these experiments that, after the action of nitrites, the loose oxygen of the blood-colouring-matter (which the observations mentioned under I. had been shown to be neither expelled nor taken possession of by the nitrite) was so locked up as to be irremoveable by carbonic oxide.

IV. The methods which have been employed by other observers for removing the gases from the blood are then examined, and the author describes the way in which he employed Sprengel's mercurial aspirator to effect the object which he had in view. He shows that with this instrument and following his method, the gases of the blood may be separated by boiling *in vacuo* during twenty-five or thirty minutes.

The gases of both normal blood and blood treated with nitrites were boiled out *in vacuo,* their amount estimated, and their composition determined. It is shown that when blood has been acted upon by a nitrite, the amount of oxygen which can be removed by ebullition in a very perfect vacuum is immensely diminished, the greatest difference being perceived when the nitrite had been in contact with the blood during the longest period of time.

V. Although blood which has been acted upon by nitrites has, to a great extent, lost its power of absorbing oxygen, it still retains the property which normal blood possesses of ozonizing the atmospheric oxygen. Nitrite-blood reacts with guaiacum paper exactly like normal blood, and when added to a solution of peroxide of hydrogen, it causes an evolution of oxygen.

VI. The changes in the optical properties of blood are shown to be due to the formation of compounds of the nitrite used with oxidized hæmoglobin; these compounds, with the exception of that with nitrite of silver,

present the same crystalline form, colour, and spectrum, whatever the nitrite which has been employed. The author has obtained compounds of hæmoglobin with nitrite of sodium, potassium and silver, and with nitrite of amyl. The methods of preparing them are described, and the results of analysis given; these show that the amount of a nitrite which adds itself to oxidized hæmoglobin varies considerably.

Having stated the conclusions which he thinks may legitimately be drawn from his investigations, the author concludes by making some observations upon the relation which the compounds of nitrites with hæmoglobin bear to the previously known hæmoglobin compounds.

We have hitherto been acquainted with hæmoglobin itself, as well as with its O, CO, and $N_2 O_2$ compounds.

These compounds are all isomorphous, and possess almost the same physical characters; in each of them hæmoglobin free from oxygen (*i. e.* reduced hæmoglobin) has apparently linked to it a molecule of O, CO, or $N_2 O_2$ respectively, the stability of the compound being least in the case of the O, and greatest in that of the $N_2 O_2$ compound. All these bodies, and preeminently the O compound, are examples of a class of bodies which stand, as it were, on the boundary line which separates chemical from physical combination, being examples of the class to which the term "molecular compounds" has been given. Like other molecular compounds, their composition varies extraordinarily within certain limits, and is influenced by circumstances and conditions which have no action on chemical compounds proper.

That a body possessing such a very complicated molecular structure as hæmoglobin should present numerous points of attachment, as it were, for the linking on of such active condensed bodies as the nitrites is not improbable; nor is it remarkable that, as in the case of other combinations "of a molecular kind," such as the union of salts with their water of crystallization, of sugar with bases, of albumen with metallic oxides, of the compound ammonias with iodine, the amount of the new and more simple body added to the hæmoglobin should vary within wide limits.

Simultaneously with the researches which the author has conducted on the action of nitrites on blood, those now being made by Hoppe Seyler * and Preyer †, although discrepant in many particulars, seem to show that hydrocyanic acid possesses, like nitrites, the power of linking itself to oxidized hæmoglobin, forming a body which is isomorphous with it, but possessing a different absorption spectrum, and incapable of absorbing oxygen.

This body appears not to possess the power of ozonizing the atmospheric oxygen, a fact which is strange, as, besides being possessed by the O com-

* Medicinisch-chemische Untersuchungen. Zweiter Heft, 1867, Cyanwasserstoffhämoglobinverbindungen, p. 201.

† Die Ursache der Giftigkeit des Cyankalium und der Blausäure, von W. Preyer. Virchow's Archiv, Bd. xl. 21 Hft. Sept. 1867.

pound, this property is also possessed by the CO and N_2O_3 compounds of hæmoglobin, as well as by the nitrite compounds of oxidized hæmoglobin. It is probable that we may now find that a large number of condensed bodies have the property, like the nitrites, of forming combinations with the blood-colouring-matter.

II. "Microscopical characters of the rhythmically contractile Muscular Coat of the Veins of the Bat's Wing, of the Lymphatic Hearts of the Frog, and of the Caudal Heart of the Eel. In Three Parts.—Part I. Microscopical characters of the rhythmically contractile Muscular Coat of the Veins of the Web of the Bat's Wing." By THOMAS WHARTON JONES, F.R.S., Professor of Ophthalmic Medicine and Surgery in University College, &c. Received April 8, 1868.

(Abstract.)

This is Part I., of a series of three, of a paper on the microscopical characters of rhythmically contractile muscular tissue, other than that of the blood-heart. It comprises a reexamination of the microscopical characters of the *rhythmically* contractile muscular coat of the veins of the bat's wing, and is offered by the author as Appendix No. 3 to his paper in the Philosophical Transactions for 1852, entitled "Discovery that the veins of the Bat's Wing (which are furnished with valves) are endowed with rhythmical contractility, and that the onward flow of the blood is accelerated by each contraction." This reexamination supplies additional details, illustrated by more correct figures, confirmatory of the author's previous description of the microscopical characters of the muscular coat of the veins of the bat's wing. The author examines also, by way of comparison, the *tonically* contractile muscular coat of the arteries, and points out that, though the fibrils of the muscular coat of the veins do not present transverse markings, they differ in their microscopical characters as much from the fibrils of the muscular coat of the arteries, as the transversely striped muscular fibrils of the bat's heart do from them. He insists, therefore, in conclusion, that there are no grounds for an implied physiological form of the doctrine of isomerism, viz. similarity of structure, with different endowments.

Part II. "Microscopical characters of the rhythmically contractile Muscular Coat of the Lymphatic Hearts of the Frog." Received April 13, 1868.

The author, in this second part of his paper, first calls attention to the fact that, on viewing the anterior lymphatic heart from the front, after dissecting down upon it from the back, he sometimes found its cavity filled with air or blood. The way by which the air or blood had entered

he considers to have been through the lymph-spaces opened into in the course of the dissection; and the mode of entrance he considers to have been by suction during diastole of the heart. The sucking action, by which the heart thus draws air or blood into its cavity when the lymph-spaces are cut into, must operate, according to his view, as a means of promoting the flow of lymph in the natural state. After describing the mechanism of the process, the author examines the microscopical characters of the proper muscular tissue composing the wall of the lymphatic heart. The result of his observations on this point is, that the muscular tissue of the lymphatic hearts of the frog is similar to that of the veins of the bat's wing, as regards both its granular semitransparent aspect and the breadth of its fibrillations, whilst it differs from the muscular tissue of the blood-heart of the animal in being destitute of transverse markings.

Part III. "Microscopical characters of the rhythmically contractile Muscular Coat of the Caudal Heart of the Eel." Received April 21, 1868.

The caudal heart of the eel lies in a kind of framework on the abdominal aspect of the extreme end of the vertebral column. The body of the last caudal vertebra forms the dorsal side of this framework, and a ridge of bone, extending along its concave abdominal aspect, must project into the caudal heart, partially dividing it into right and left compartments. The caudal heart of the eel would thus appear to represent the two caudal sinuses of certain other fishes *run into one.*

From the manner in which the caudal heart is connected with the surrounding structures of the tail, its movements are communicated to them as described in the author's paper, entitled "The Caudal Heart of the Eel, a Lymphatic Heart," &c. By the elastic recoil of the structures, on the other hand, the cavity of the heart is drawn into a state of dilatation; and the result must be, as in the analogous case of the anterior lymphatic hearts of the frog, that lymph will be forced into the heart from the adjacent lymphatic vessels or spaces.

The muscular fibres composing the walls of the caudal heart resemble in shape the sheathed primitive fasciculi of the muscles of the skeleton, but are only half as broad, and they are not transversely striped. They have a granular aspect, and on close examination are found to be a fasciculus of fibrils $\frac{1}{10,000}$ of an inch broad, contained in a delicate structureless sheath. These fibrils resemble the fibrils of the muscular coat of the veins of the bat's wing, and of the muscular coat of the lymphatic hearts of the frog, and may be grouped, the author thinks, together with them under a common head, viz. *unstriped rhythmically contractile muscular fibrils.*

III. "On Waves in Liquids." By W. J. Macquorn Rankine, C.E., LL.D., F.R.S. Received April 16, 1868.

(Abstract.)

(1) *Object of this Paper.*—It has long been known that in an uniform canal filled with liquid, the speed of advance of a wave in which the horizontal component of the disturbance is uniform from surface to bottom is equal to the velocity acquired by a heavy body in falling through half the depth of the canal. But, so far as I know, it has not hitherto been pointed out that a similar law exists for waves transmitting a disturbance of any possible kind in a liquid of limited or unlimited depth, provided only that the upper surface of the liquid is a surface of uniform pressure. The object of this paper is to demonstrate that law, and to show some of its applications.

(2) *Velocity of Advance defined.*—Throughout this investigation the velocity of advance of a wave will be defined to be the mean between the velocities with which the shape of the wave advances relatively to a surface-particle at the crest, and to a surface-particle in the trough respectively. In ordinary rolling waves the velocities of particles in those two positions are equal and contrary, so that the speed of advance as above defined is equal to the speed of advance of the wave relatively to the earth. A wave of translation in which the velocities of particles at the crest and hollow are not equal and contrary, may be regarded as produced by compounding the motion of a rolling wave with that of a current whose velocity is half the difference of the velocities of those particles.

(3) *Relation between height of wave and horizontal disturbance at the surface.*—The following relation between the height of a wave and the horizontal disturbance of the surface-particles has already been proved and made use of by various authors; and it is demonstrated here for convenience only. Let $+u_1$ and $-u_1$ be the velocities of a surface-particle at the crest and trough of a wave respectively. Let a be the velocity of advance of the wave as defined in article 2. Conceive a horizontal current with the uniform velocity $-a$ to be combined with the actual wave-motion; the resultant motion is that of an undulating current, presenting stationary waves in its course; and the forces which act on the particles are not altered. The resultant velocity of a particle at the crest becomes $-a+u_1$; and the resultant velocity of a particle in the trough becomes $-a-u_1$. Let the height from trough to crest be denoted by Δz; then, since the upper surface of the liquid is supposed to be a surface of uniform pressure, the principle of the conservation of energy gives the following equation:

$$g\Delta z=\tfrac{1}{2}\{(a+u_1)^2-(a-u_1)^2\}=2au_1. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

(4) *Virtual Depth of Uniform Horizontal Disturbance.*—By the phrase "virtual depth of uniform horizontal disturbance," or, for brevity's sake, *virtual depth,* I propose to denote *the depth in the liquid to which an uni-*

form horizontal disturbance would have to extend, in order to make the amount of horizontal disturbance equal to the actual amount. That is to say, conceive that a pair of vertical planes normal to the direction of advance, and each of the breadth unity, coincide at a given instant, one with the trough-line or furrow, and the other with the crest-line or ridge, which bound one of the slopes of a wave. We will suppose this to be the front slope, merely to fix the ideas; for similar reasoning applied to the back slope leads to the same results. At a given depth z below the surface, let $-u''$ be the horizontal velocity with which particles are in the act of passing backwards through the plane of the trough, and $+u'$ the velocity with which particles are passing forwards through the plane at the crest; then the rate by volume at which liquid is passing into the space between those two planes is

$$\int u'dz+\int u''dz;$$

the integrations extending from the surface to the bottom. Let k denote the virtual depth; then

$$k=\frac{\int u'dz+\int u''dz}{2u_1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

(5) *Relation between Virtual Depth and Speed of Advance.*—In an indefinitely short interval of time dt, the volume of liquid which passes into the space between the two vertical planes mentioned in article 4, is

$$2ku_1dt;$$

and in order to make room for that volume of liquid, the front slope of the wave must sweep in the same interval of time through an equal volume. But the volume swept through by the front of the wave is

$$adt\Delta z;$$

so that, cancelling the common factor dt, we have the following equation:

$$a\Delta z=2ku_1;$$

but, according to equation (1), $\Delta z=\frac{2au_1}{g}$; which value being substituted in the above equation, gives

$$\frac{2a^2u_1}{g}=2ku_1,$$

and therefore

$$\frac{a^2}{g}=k, \text{ and } a=\sqrt{gk}; \quad . \quad . \quad . \quad . \quad (3)$$

so that the *velocity of advance of a wave* (defined as in article 2) *is equal to that acquired by a body in falling through half the virtual depth*; and this is true for all possible waves in which the upper surface is a surface of uniform pressure.

(In article 6 of the paper, the speed of advance of a wave of translation is expressed by combining the speed of a rolling wave, $\sqrt{gk}$, with that of a supposed current, as stated in article 2.

In articles 7, 8, and 9 the law which connects the speed of advance of a wave with the virtual depth is compared with the already known laws of the transmission of rolling waves in water of limited or unlimited depth. The principal results may be summed up as follows. Let T be the periodic time of a wave, in seconds; $h=\frac{gT^2}{4\pi^2}$ the *equivalent pendulum*, that is, the height of the pendulum whose period is the same; $c=\frac{\text{length}}{2\pi}$ the *rolling radius*, being the radius of a circle whose circumference is equal to a wave-length; u_1 the greatest horizontal velocity, and w_1 the greatest vertical velocity of a surface-particle; a the velocity of advance; then

$$a=\sqrt{gk}=\sqrt{\frac{w_1 gc}{u_1}}=\frac{w_1}{u_1}\cdot\sqrt{gh}=\frac{w_1}{u_1}\cdot\frac{gT}{2\pi},$$

and

$$k=\frac{w_1}{u_1}\cdot c=\frac{w_1^{\,2}}{u_1^{\,2}}\cdot h.)$$

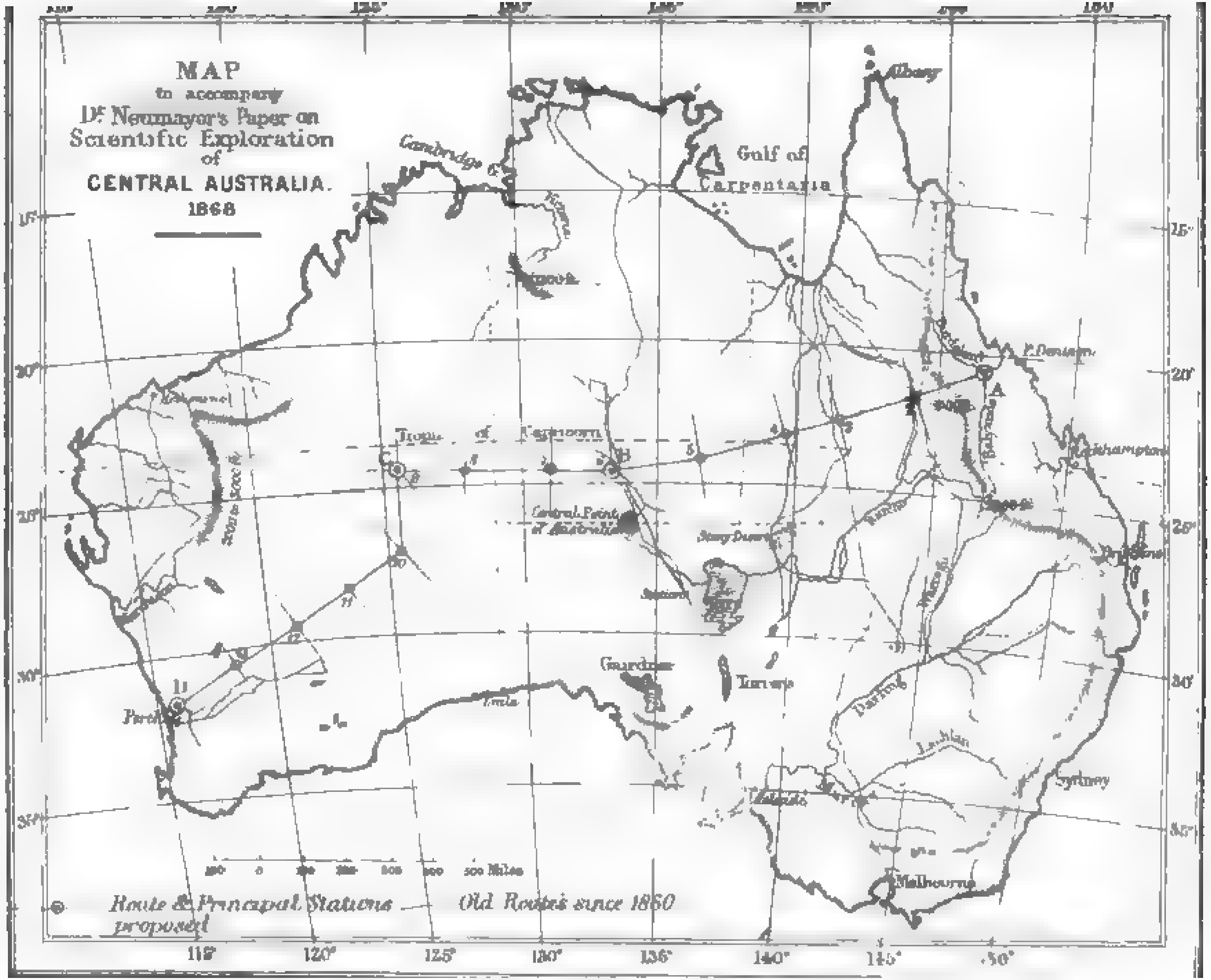
(10) *Oblique Advance of Forced Waves.*—Let s be the velocity with which a floating solid body is driven horizontally; the wave which that solid body pushes or drags along with it is forced to advance at the velocity s also; while the virtual depth of disturbance, k, bears some relation to the depth of immersion and figure of the solid body. If the speed of advance corresponding to that depth, $a=\sqrt{gk}$, is less than s, a pair of wave-ridges diverge obliquely from the path of the floating body towards opposite sides; and the sine of the angle which each of those ridges makes with that path is $\frac{a}{s}$. Such is the mode of formation of the obliquely spreading waves which travel along with ships*.

When the velocity of the floating body is less than the speed of advance corresponding to the depth to which it disturbs the liquid in its immediate neighbourhood, it is probable that the virtual depth of disturbance of parts of the liquid beyond the immediate action of the floating body adjusts itself to the velocity, and assumes the value $\frac{s^2}{g}$.

11. *Possibility of Obliquely Advancing Tidal Waves.*—It is possible that instead of a depth less than the virtual depth corresponding to the speed of advance of a tidal wave, the ridge of that wave may place itself in a position oblique to the parallels of latitude, according to the principle stated in article 10. It still remains to be ascertained, by the study of tidal observations, whether such phenomena take place in the tides of the ocean.

12. *Terminal Velocity of Waves.*—It is known that in deep water all waves left free from the action of disturbing forces tend ultimately to assume the condition of free rolling waves whose velocity of advance depends on

* See Watts, Rankine, Napier, and Barnes, 'On Ship-building,' Division I. Article 156, p. 79.


MAP
to accompany
Dr. Neumayer's Paper on
Scientific Exploration
of
CENTRAL AUSTRALIA.
1868
Gulf of
Carpentaria
Cambridge G.
Tropic of Capricorn
Central Point of Australia
P. Denison
Rockhampton
Brisbane
Sydney
Melbourne
Adelaide
Perth
Gairdner
Torrens
Darling
Lachlan
Eucla
Albany
Route & Principal Stations proposed
Old Routes since 1860
500 Miles
15°
20°
25°
30°
35°
115°
120°
125°
130°
135°
140°
145°
150°

their periodic time, and is expressed by the equation $a=\frac{gT}{2\pi}$. This, then, may be called the *terminal velocity* of a wave of a given period. It follows that if a wave is raised through the disturbance produced by a solid body, that wave will at first travel with a speed depending on the virtual depth of the original disturbance; but as it advances to a greater and greater distance from the disturbing body, the velocity of advance will gradually approximate to the terminal velocity corresponding to the periodic time, and the virtual depth will continually adjust itself to the changing velocity, and approximate gradually to the equivalent pendulum corresponding to the periodic time. Such is the cause of the forward curvature of the ridges of the obliquely diverging waves which follow a ship*.

May 14, 1868.

Lieut.-General SABINE, President, in the Chair.

The Right Hon. the Earl of Rosse was admitted into the Society.

The following communications were read:—

I. "Scientific Exploration of Central Australia." By Dr. G. Neumayer. Communicated by the President. Received April 20, 1868.

If we look on a map of the Australian continent published ten years ago, we are struck by the immense expanse of land then unexplored; we perceive at a glance that the south-eastern sea-board only of this great continent had then been examined with any degree of accuracy, and that very little was known to us respecting the character of its shores on the west and north-west. In two quarters only had the zeal and daring of the explorer succeeded in forcing a path towards the central portions of this vast territory, Sturt having penetrated as far as 24° South and 138° East, and Gregory as far as 21° South and 128° East. The nature of the country traversed by these two eminent explorers was such as to countenance the supposition, that the interior of Australia was little better than one vast desert, offering almost insurmountable obstacles to exploration. The idea, originally advanced by Oxley, that the greater part of the interior was occupied by vast inland lakes, was then abandoned; and the theory just mentioned took its place. In such a state of utter uncertainty as to the nature of the interior of a vast continent, it is but natural that various theories should be started; and no doubt they will, in the end, help to keep up the spirit for rigorous examination and exploration, yet care must be taken that they do not, by the unfavourable nature of their suppositions, tend to discourage enterprise. From such a danger we had a narrow escape

* This is explained in greater detail in a paper read to the Institution of Naval Architects on the 4th of April 1868.

during the years following A. C. Gregory's exploration of the interior, and his expedition in search of Dr. Leichhardt, as at that time it was generally believed that the arid plains and stony deserts met with in the east and south, and the sandy desert in the north-west, were but the outskirts of a desert country unparalleled on our globe.

Fortunately, however, for the progress of civilization and geographical knowledge, this unfavourable opinion as to the nature of the interior was not universally entertained. Many men, well versed in terrestrial physics, especially with reference to the Australian continent, could not, from reasons based upon meteorological observations made in the south-east, concur in the prevailing belief. They urged the possibility of tracts of fine country interspersing the so-called desert, and the necessity, in any case, of clearing up the mystery still surrounding this important geographical question; and in this view they were strongly supported by the improbability (generally speaking) of the existence of a desert country of such enormous extent and such a description in any part of the earth's surface. It would be useless to enter now upon the arguments for and against the various opinions set forth during that period of uncertainty, except that they might in some instances serve to put us on our guard against advancing or accepting bold conjectures which may be put forward at any future time, and more particularly in the special case we have to deal with in this paper. Suffice it to say, that the spirit of enterprise and the desire of increasing our geographical knowledge triumphed at last. The year 1860 gave a fresh impulse to Australian exploration, and will for ever be memorable as having inaugurated a new era in this respect. In the end of that year and the beginning of 1861, Burke and Wills crossed the continent with comparative ease. It was now said by many of the adherents of the old desert doctrine, that Burke had merely hit upon a narrow strip of good country, which carried him across; had he but deviated to the one side or the other of his path he must have failed in his attempt. But when M'Dougal Stuart three times crossed and recrossed the continent in other parts, and the last time from shore to shore—when M'Kinlay made his way from South Australia, by way of the Gulf of Carpentaria, to the coast of Queensland, driving before him a flock of sheep—when Walker and Landsborough had accomplished their journeys through the imaginary desert country—the old opinion could no longer be maintained, and the desert theory fell utterly into discredit, at least as far as the eastern part of the continent is concerned. Since that time this once so much dreaded task has been several times successfully accomplished, so that we are now enabled to give a pretty accurate description of the features of the country forming the scene of these glorious achievements; and as it will doubtless prove of material assistance to the complete understanding of the following exposition and plan, I may here be permitted to give in a few words an outline of the main character of the ground to the north of the parallel of latitude 26°, between the coast-ranges of Queensland and Stuart's route through the

centre, which embraces nearly the whole of the country discovered and examined since 1860.

From the records of the various explorers, it would appear that the line which divides the waters flowing to the coast, and those flowing to the interior, runs from 19½° S. on Stuart's track to 19° S. latitude on Landsborough's south-west expedition, in 138° east longitude. According to Mr. M'Intyre's apparently reliable observations, this line would then pass through 22° S. in 141½° E., whilst M'Kinlay places in the same locality the watershed between the Middleton and Müller rivers, in a latitude but slightly differing from that just named; and in Walker's Diary we find it passing through 21½° S. and 145° E., where I find the highest elevation recorded by the latter explorer on his expedition in search of Burke and Wills. Mr. A. C. Gregory's exploration places the continuation of this line of watershed through the north-west in 13° S. and 130° E., whilst in the east it meets the main watershed between the Belyando and Thomson on the one side, and the Burdekin and Flinders on the other, in about 21° S. and 145½° E. (See accompanying map.) The sweep of this line leads across a sandstone tableland of considerable elevation—in parts even as high, if we may rely on the data hitherto collected respecting it, as 1900 or 2000 feet. On its coast side this tableland inclines somewhat abruptly, whilst towards the interior it slopes more gently, thus affording a ready explanation of the marked difference existing between its river-systems, those flowing in the former direction passing through well-defined valleys, while those in the latter direction run generally in shallow beds, and are consequently subject to excessive evaporation and inundation according to the season of the year. The general incline of the country from longitude 145° to Stuart's track in about 135° is towards south-west, until reaching near the latter its lowest point, as we feel justified in concluding from the great number of hot springs on the route Stuart pursued when crossing the continent. This immense tract of land abounds in creeks and ill-defined water-courses. In many cases the dividing or separating ridges of sandstone are (probably from the effects of floods and weather) broken up, and cover the surface, which then presents a desert-like appearance, only here and there covered with a scanty vegetation, consisting mostly of Spinifex (*Triodia irritans*). Very frequently, however, these ridges have succeeded in resisting the destroying effects, and we then meet with valleys of good soil, covered with fine grass and gum forest; which state of things, happily for the future of the Australian continent, seems to predominate, as we learn from the reports of explorers who passed through fine country after having once entered the tropic. These water-courses of the interior basin drain partly towards the Barcoo River (Cooper's Creek), partly towards the south, splitting up into innumerable little creeks and rills without any definite direction. As an example of the latter kind may be mentioned the Burke Creek, which at times causes immense inundations in the tract of country near 25° S. and 140° E., as we learn from the reports of

Burke, Wills, Howitt, M'Kinlay, and Gregory. To the south of this region the at one time so much dreaded "Stony Desert" seems to extend in the manner indicated on our map; and it certainly bears every appearance as if this region of sandstone was principally caused by the effects of the inundations and floods already referred to. For whereas it is very easy to trace this peculiar phenomenon to the south, it is hardly possible to define it exactly towards the north, which fact accords well with the explanation of its true origin just suggested. The careful researches of Mr. B. Hassenstein* have tended to throw new light upon this subject, and reduce the Stony Desert to its proper limits. We know now that the arid plains described by some explorers, which others had found well grassed and covered with an even luxuriant vegetation, are the receptacles of the waters flowing from the north, and form the boundary of the Stony Desert; we know moreover that extensive tracts of fine country are interspersed with strips of "stony desert" of a very limited extent. Such is the nature of the whole country, as far as we know it, from the meridian of 145° to Burke's track. Of the country between the latter's course and that of Stuart very little is known; but it is not unreasonable to suppose that it will prove to be mostly of the same description as that already well known to us, as it forms only the lower part of the interior basin above described, the very bottom of which we have been made tolerably well acquainted with by Stuart's exploration, which bears out that supposition. Further to the west, in the unknown country between Stuart's track and the west coast, the same sandstone tableland probably rises again to the high ranges which have been observed by the various explorers who have penetrated into the interior from the west, attaining in some parts an elevation of from three to four thousand feet above the ocean. Therefore it is not at all unlikely that we shall find in the western half of the central basin in the main the same state of things which we have found to exist in the east, with but such slight modifications as may be determined by the configuration and lay of the country in connexion with the prevailing system of winds and contingent meteorological phenomena.

In order to complete the general description of this portion of the Australian continent, we must call to mind the effect a tropical and subtropical sun must naturally exert upon a territory constituted as this interior tableland. During the time the sun is north of the equator, in the winter months of the southern hemisphere, the prevailing winds are from south-east all over the northern continent, with little and only occasional rain; but on the sun's approach, during October and November, the monsoon shifts to north-west, and brings on the rainy season, gradually advancing from south to north in the middle of December and January. At the end of this season, in February and March, and about the time preceding the shifting back to south-east of the monsoon in April, the heavy falls of rain and the soaked state of the soil cause those immense floods which are

* Petermann, Mittheilungen, 1867, p. 80.

recorded in the Journals of the various explorers; and it is during this period that in the territory south of the Burke Creek and north of the Stony Desert, as we have already explained, great ravages are caused by inundation. Further to the south the meteorological phenomena of the northern portion of the continent pass gradually over to those prevailing in the south, namely that of two alternating currents of air, with winter rains and a short rainy season in September and October.

Great as have been the recent achievements with regard to the geographical knowledge of the continent, comparatively little has been done by any one of the exploring expeditions towards the advancement of science. Information calculated to throw light on the elevation of the interior or on its geological character flows very scantily from the journals kept by the explorers; and even the astronomical determinations of localities must, at least as far as longitude is concerned, be received with caution, as the means at their disposal, the comparatively small practice of most of the observers in determinations of this kind, and the very methods employed, would hardly admit of anything like a close approximation to the truth. Indeed we may safely assert that it would have been scarcely compatible with the general pioneering object of parties in the field since 1860 to have devoted more attention to matters of science, strictly speaking, than was sufficient to carry them through the difficulties they had to encounter. Perhaps it may not be considered out of place if I express here my deep regret, on this very ground, for the untimely death of my young friend, W. J. Wills, the astronomer of Burke's expedition; for, had he but survived his first feat, there can be but little doubt that his zeal for the advancement of science, and the knowledge he had obtained during the time he was on the staff of the Observatory over which I then presided, would have enabled him to take the first place as a scientific explorer, whereas we can now only admire him for his courageous and enterprising spirit as a pioneer. This hope, however, is at an end; and up to the present time absolutely nothing has been done towards the scientific examination of the vast interior of Australia—an examination of such immense importance for the advancement of almost every branch of physical science, and for the development of the natural resources of this great country. It is with regard to this matter that I venture to address this Society, with the view of soliciting its important assistance in starting an expedition, having for its object the exploring of the western half of Australia, and the scientific survey of the route across the entire continent.

When the celebrated Australian explorer, Dr. Leichhardt, started on the expedition which was to be his last, he did so with the intention of crossing the continent from east to west, for the purpose of discovering the extent of Sturt's desert, and the character of the western and north-western coas , and of observing the gradual change in vegetable and animal life from one side of the continent to the other*. It is now exactly twenty years

* History of Discovery and Exploration of Australia, by the Rev. J. E. T. Woods, vol. ii. p. 518.

lay down a route across ... of the interior as to n
mere conjecture. Circumstances, however, hav
considerably; at least one half of the continent
such a way as to make us acquainted with the na
may expect to meet with and turn to account, wh
coast numerous expeditions have tended to dimi
such an undertaking would be invested. And in
this Society to Dr. Leichhardt's idea, with the vie
I do so with the conviction that now the proper ti
in hand an enterprise of such importance for the f
colonies, and especially for the advancement of scie
festing alike our appreciation of the various interes
noble mind who risked all he had in their furthera
of such a work, its scope may now be materially e
rough survey on a single line from north-east to sou
ratus which allowed of but a limited attention being
tific matters, it is now proposed to form a base-line fo
of science through the interior of a vast continent.
now no longer be regarded as impracticable or premat
cessful completion cannot be otherwise than replete w
rest, I hope to be able to show in the course of this pa

After these introductory remarks, I shall proceed
which I propose to follow in carrying out the explora
half of the continent, and the scientific survey of the
followed in traversing the interior from the east shores
Western Australia.

It is proposed that the expedition ...
on the Burd...

extremity of the continent, nothing is known; and in choosing a practicable route, we must be guided entirely by surmises as to the nature of the country, based upon the supposition that we shall most probably have to deal with a state of things in the west very similar to that already known to exist in the east. I shall, in the first place, however, lay down the entire course, and then enter upon my reasons for having done so. From point B I propose keeping nearly on the same parallel as far as point C, in longitude $125\frac{2}{3}°$ East; from thence proceeding on the same meridian to a point in 27° South, and thence to a point D in $116\frac{3}{4}°$ East and $31\frac{1}{2}°$ South. Near the latter point, on our route to Perth in Western Australia, we shall strike the Swan River. The entire distance on that route is about 2649 miles, of which 1080 comprise the distance from A to B, and the remainder that from B to C and D, an allowance of 20 per cent. being made for curvature, which percentage must always be understood when reference to distance occurs in the course of this paper.

As a close examination of the country traversed is the primary object of the expedition, it is proposed to accomplish it in thirteen stages, so that fourteen separate depots will have to be established during its progress, each depot being intended to be retained only for such a time as will be requisite for the survey and exploration of the surrounding country, and for the formation and transport to the next. From A to B the number of depots will be six, so that the average distance between two consecutive depots will be 216 miles, while from B to C and C to D there will be eight depots, with an average distance of 200 miles. On the map these depots are marked; but it is evident that it is next to impossible to assign them their proper positions with any degree of accuracy, as these will depend so much upon circumstances at present almost entirely unknown to us. It is only for the line from A to B that we are enabled to fix with any certainty the positions of such depots, as we are already acquainted with some localities the nature of which will probably recommend them for such a purpose. Thus we should propose fixing depot No. 2 on the meridian of 145° and near Walker's track, depot No. 3 on the Middleton River of M'Kinlay, depot No. 4 near the "fine open plains" of Burke and Wills, and depot No. 6 on Stuart's track somewhere between the Fincke and Hugh rivers, in each of which cases the country is described as well grassed and provided with permanent water. For depot No. 6, and depots 7 to 12, we have no data whereby to guide us, until we again approach the regions already explored from the west. As it is proposed from these various depots to strike out in different directions, they will have also to be selected with a view to enable these minor expeditions to yield the greatest possible amount of information with respect to the largest possible tracts of country.

With reference to the time required for accomplishing the whole expedition in a manner commensurate with the scientific objects of the undertaking, it may be said, that it is proposed to devote three years and six months to it, of which fifteen months are taken up by the examination of

the country between A and B, and twenty-seven months for the exploration and scientific survey of that part of the route within the entirely unknown region between points B and D. Now let us examine what prospects of success this would allow for the performance of the task proposed. The distance from the Burdekin to Stuart's track may easily be travelled through, at the moderate rate of ten miles a day, in 108 days, or three months and a half, allowing a time of stay in each of the several depots of nearly two months (1·9). For the route through the western country, the time has been increased at a rate proportionate to the increased risk incurred and the care to be bestowed upon the explorations within its regions. The time requisite to travel over the line from B to D would be about five months and a half, which would allow of a period of stay in each of the depots of 2·4 months; counting, however, depot No. 6 twice, as it is proposed, for reasons presently to be explained, to prolong the stay at that point beyond the time generally allowed to the other depots. From this exposition, it is evident that ample opportunities are offered for an exact study of the ground travelled over with reference to the various branches of science to be included in the scope of the entire work. This will become still more apparent on the further unfolding of the details of the organization of the expedition. Dr. Leichhardt intended to travel over nearly the same distance in about two years and a half, and that too without having also, as regards the eastern portion of the Continent, any information whatever to guide him in his route; and it was probably to some extent owing to the insufficiency of time allowed by him for the carrying through of such an extensive undertaking, and the consequent deficiency of provisions and outfit, that he failed in its accomplishment. The extent, moreover, of the scientific labours proposed to be undertaken on this occasion is something widely different from what explorers twenty years ago could attempt, and is such as would of itself alone justify an increase of time by twelve months.

I shall now have to enumerate a few of the reasons prompting me in proposing the route I have laid down in the preceding pages. From what I have already said respecting the character of the tropical and subtropical interior of Australia, it appears that the line of route from the Burdekin to the Fincke of Stuart passes, as far as known, through practicable country, well watered and grassed, abounding in game of all kinds, and likewise well peopled with aboriginals. We are on this route likely to meet with the most practicable country in the interior we propose to explore and examine, and shall probably shun entirely the so-called desert country, subject to destructive inundations. There is much likelihood that the unknown regions in the west bear in many respects a great resemblance to the eastern half; and I therefore feel inclined to believe that on the same parallel of latitude (23° S.) we shall meet with no greater obstacles to progress than we are likely to encounter in the east. The explorations of Mr. Gregory in the same latitude and in longitude 117° seem to corroborate this opinion, as the country appeared to him from that point towards the east to bear

for many miles a promising appearance. Observations made in other parts of the north-west *littorale* of Australia confirm this view, and some of the rivers have been supposed to take their origin in high granite and trap ranges three hundred miles from the coast. They would in this case be only four hundred miles from our point C, towards which the high western tableland, of which these ranges are probably the watershed, inclines until again reaching the lowest part near Stuart's hot springs and the lake district forming the receptacle of the drainage from the Barcoo River. The course proposed passes very nearly through the centre of the unknown interior, and offers therefore, as an inspection of the map will show, an opportunity for exploring these unknown regions, and most likely also for tracing the limits of Mr. Gregory's sandy deserts in the north-west. With reference to the south-west extremity, I accept for the greater part the suppositions thrown out by the Rev. J. E. T. Woods respecting its nature, first ably set forth in a letter addressed to the 'Melbourne Argus' some years ago, and again repeated in his excellent work on Australian exploration (vol. ii. p. 511), from which we quote the following passage. Mr. Woods says,—"If the western end of the tableland be on an average two thousand feet high, there must be a drainage to the interior nearly equal to that which causes so many rivers on the west coast. The watershed has never yet been crossed from the west side; but one cannot help remarking that wherever it has been crossed elsewhere good land has been found. It is no evidence against the existence of a river that none are found on the south coast, especially in the Australian Bight, where it would be most likely to appear. Many places in the interior have an extensive drainage, which never reaches the sea. The Barcoo drains into Lake Eyre, which is the receptacle of many other streams. A stream from the west coast might empty itself into Lake Gairdner*. There must at any rate be some important drainage in connexion with that large sheet of water." It is scarcely needful to add anything to the reasons here set forth for the necessity of the existence of a large drainage area in the south-western extremity of the continent; but it may perhaps be not out of place to recall to mind here that the Barcoo River (Cooper's Creek) drains a territory of nearly nine degrees of longitude and seven degrees of latitude before emptying itself into Lakes Eyre and Gregory, forming, after its bifurcation near the locality where the final scenes of the Burke and Wills tragedy were enacted, an immense river delta, far exceeding any of the well-known deltas in the world†. If we now place a drainage system, in dimensions similar to that just spoken of, to the west of Lakes Eyre and Gairdner, it would in all probability be intersected by the course proposed through the western interior. An expedition, after having once struck such a river-system, would of course have to follow up the discovery; and would, in its further course towards south-west, have mainly to be

* I rather feel inclined to believe that the receptacle spoken of is formed by some lakes to the north of the Australian Bight, yet undiscovered.

† According to the recent exploration of Major Warburton.

guided by the watercourses, without however losing sight of its primary object. It is not unlikely that, by the time the proposed expedition would reach the country north of the Australian Bight, expeditions from Western Australia, and even from the recently opened harbour on the south coast (Eucla), will have thrown new light upon this subject, thereby considerably facilitating this portion of the undertaking.

It has already been mentioned that at point B on Stuart's track it is proposed to make a longer stay than in any of the other depots. The principal reason for so doing is to afford the expedition at that stage an opportunity of communicating with the settled portion of South Australia, previous to entering on the unknown territory to the south-west. A small party may be detached from the main body for the purpose of travelling to the nearest point of settlement, on which occasion collections and documents may be sent to Adelaide or Melbourne, as well as any information received which may be of importance for the progress of the expedition, and chiefly such respecting the progress of exploration in Australia, having an immediate bearing on the problem at issue. Such an expedition may also present an opportunity of exchanging or making up our complement of men and horses, replenishing stores, &c. That this does not present any serious difficulty in execution we know from Stuart's expeditions, who on an average travelled the distance from the Fincke to Mount Margaret * (Mr. Jarvis's station) in twenty-four days. It is therefore fair to suppose that within ten weeks this party could proceed to the first settlements and return, after having accomplished its objects.

In the preceding part of this paper I have laid down the track I propose following, and have, I believe, succeeded in explaining the reasons which guided me in doing so. I have avoided, however, all matters of detail as to the branch expeditions, which are intended to be undertaken on both sides of the main route, as such expeditions must to a great extent depend on the nature of the country to be explored, on which point our knowledge is still very scanty. I shall now in a few words give an outline of the scientific objects of the expedition, and then proceed with some details respecting its organization and probable expense.

An expedition passing through the centre of such a vast continent, travelling through 32 degrees of longitude and 12 degrees of latitude, cannot be otherwise considered than as productive of the most material advantages to the cause of science generally, provided the plan of its working be such as accords with the present state of scientific inquiry, and the conduct of the whole be entrusted to competent hands. It is frequently supposed that, in expeditions of this nature, it is expedient to confine the scientific researches and observations within the narrowest limits. Indeed, a rigorous scientific inquiry is frequently thought incompatible with geographical discovery. This is a grievous mistake, and has invariably proved to be such whenever an expedition has taken the field, in the organization of which

* Probably the stations are now still further advanced towards the north.

proper attention had been paid to the objects it had to serve. There is no doubt that the many unemployed hours, even of those of the party generally and more exclusively engaged upon exploration matters, may, for the benefit of the well-being of the whole expedition, well be turned to account in assisting scientific inquiry. On such principles the scientific plan of operations of the proposed expedition, as detailed hereafter, has been framed. It includes the following branches:—

1. *Astronomical Science and Surveying.*—In addition to such operations as are absolutely necessary for the mapping of the country, it is proposed to organize a system of observations of the moon, with a view to assist longitudinal determination. Such more accurate determinations of the geographical position may be carried out at the various depots above spoken of.

2. *Terrestrial Physics and Meteorology.*—Systematic registration of meteorological phenomena and terrestrial magnetism is likewise to be carried out at the depots, particularly with a view to assist barometrical measurements of elevations and magnetic observations in the field.

If it should prove at all practicable, it would likewise be advisable to include in the general plan of working observations on the length of the pendulum vibrating seconds.

3. *Geology, Palæontology, and Mineralogy.*—A geological sketch-map of the whole route across the continent is to be made, and palæontological specimens are to be collected. Special attention has to be paid to the mineral resources of the country travelled over.

4. *Botany.*—Observations on the physiology and geographical distribution of plants are to be included, and collections made.

5. *Zoology and Comparative Anatomy, including Ethnology.*—This branch is likewise to be attended to with a view to assist physiological studies, and to examine into the applicability of Darwin's Theory on the Fauna of Australia, ancient and modern. Collections are to be made.

Character and language of the aboriginals in the various parts of the continent are to be studied.

6. *Sketching and Photography.*—These arts will be employed more particularly for the purpose of representing the character of the various tracts of country passed through, but will have likewise to assist the various branches of science in the complete execution of their parts.

I refrain from entering into particulars respecting the general scheme of scientific work to be adhered to in the course of the expedition; the more so as it is intended that the authorities in the various scientific branches, both in England and in the colonies of Australia, should be consulted on this matter, and their cooperation and advice solicited. It is especially by the aid of the scientific men and institutions in Australia, and their extensive labours in the various branches of science, that I am led to hope for a great success in the scientific part of the work of the expedition, as these labours form an excellent base whereon to build and start from. Thus much, however, I may state, that it is to be made a rule that nothing be

.. .. this subject, and includin
an expedition to aid in clearing up that my
outset of this paper, twenty years have now e
pearance of that great explorer, and there is n
lihood of rescuing any of his party alive. But
ing the fate of that brave body of men should
the object be attained, though there can be no
our doing so grow smaller from year to year,
conflagration of large tracts of forest diminishin
scientific survey entailed upon such an expediti
great facilities also for the promotion of a search
explorers. In addition to which I consider it o
such a purpose that the search should be instit
which the missing expedition intended to move,
to cut Leichhardt's probable route at right angl
ineffectual. When we consider that the eastern
continent has been so frequently crossed and recr
years without any material information having b
hardt's fate, we feel almost disposed to believe tl
from Upper Victoria and the Alice River, in a di
to that taken by Mr. Gregory on his expedition i
larly as such an opinion is supported by such an
exploration as Mr. J. E. T. Woods in his recent
opinion the explorer was compelled to follow th
perished in the country to the west of Lake Eyre
we must not forget that such ...

fore the country between Stuart's and Burke's track must be well examined with a view to find traces of the party. Although it may at first sight appear difficult to conceive that Stuart could have passed six times through the country which Leichhardt most probably crossed on his way to the west without observing any trail or mark of the latter's course, nevertheless there is no improbability in such being the case; and unremitting zeal should be displayed throughout the whole expedition in endeavouring to lift the veil from off this sad tragedy. Should the expedition fail in finding the remains of the party in the east, they will have to search for them in the west and south-west extremity of the continent.

An expedition through the vast interior of Australia, with such an extended scheme of operations and so many important matters to attend to, should be organized on such a base as to give full guarantees for being able to accomplish its main objects, as well as to protect itself against attacks of the aboriginals and the destructive effects of unforeseen misfortunes. It is therefore proposed that the expedition should number twenty-five men. The following is the plan showing how the exploring expedition proper is to be composed:—

Leader, assistant leader, storekeeper and overseer, saddler and tent-maker, blacksmith and wheelwright, twelve stockmen, and three aboriginals. To these are to be added the following scientific men of the party:—1. Geologist and mineralogist; 2. Botanist and chief medical officer; 3. Zoologist, palæontologist, and medical assistant; 4. Artist, photographer, and custos of collections; 5. Assistant for physical science and observer.

With reference to the organization of the party, so as to ensure a satisfactory cooperation of all concerned, it is proposed to adhere to the following principles:—

1. The scientific members of the expedition, with the exception of the leader and the assistant leader, do not form part of the exploring party proper, but are under the leader's supervision, and may be employed as may appear to him desirable for the advancement of the objects of the expedition.

2. The exploring party proper consists of the leader as first officer, the assistant leader second officer, and the overseer third officer, two artisans, twelve stockmen, and three aboriginals.

3. The exploring party proper is to be divided into three bodies of men:—

- *a. Depot party.*—Storekeeper and overseer, saddler and tent-maker, three men, and an aboriginal. To this party the assistant observer is to be attached.
- *b. Field Party* I.—Leader, blacksmith, four men, and an aboriginal. To this party any of the scientific men may be attached as best answering the purpose, care being, however, taken that one of the medical men be included among them.

c. Field Party II.—Assistant leader, five men, and an aboriginal. To this party &c. as above.

4. The depot party remains in the depots which will successively be formed in the course of the expedition. It will chiefly be employed in keeping everything in repair and good order, in preparing provisions, and propagating useful plants. The sick and convalescent are likewise to be received into it. Systematic registration of meteorological and magnetical phenomena is to be carried on by it under the immediate superintendence of the assistant observer.

5. The field parties will be employed in such a manner that one will be examining the neighbourhood of the depot, say thirty miles round, while the other will undertake the larger excursions on both sides of the main route. In case of a removal of the depot in the direction of the main route, all parties will have to cooperate. It is proposed, moreover, to employ the field telegraph, as well for the promotion of the scientific objects as for the more satisfactory cooperation of the whole party engaged. The perfection to which ballooning has been brought by the zeal and energy of Mr. Glaisher makes it not unlikely that it may be employed with advantage in this expedition for the facilitation of the exploring and mapping of tracts of country otherwise barely accessible.

For means of transport it is proposed to employ fifty horses and eight or ten camels, which latter animals have now been acclimatized in the colonies, and show a special fitness and adaptation for Australian exploring work.

We may now add a few words as to the probable amount of expenditure an expedition of this kind would involve, referring, however, for particulars to the appendix.

The following is an abstract of the probable expense:—

1. Expenses previous to the organization of the expedition	£880	0	0
2. Outfit of the expedition, exclusive of provisions......	2,980	0	0
3. Salaries, wages, and contingencies for three years and six months....................................	17,675	0	0
Total expenditure..........	£21,535	0	0

This estimate has been framed without regard to any expenses in connexion with the publication of the results of the expedition. The sum may, at first sight, appear somewhat large; but when we come to take into consideration the objects which the expedition professes to advance—when we remember that, for the first time in the history of Australian exploration, the various governments are to unite in support of a uniform and well-planned scheme of exploration—when we consider that this sum is to be distributed over a period of three years and a half—we cannot fail to perceive the moderate amount of the sum proposed to be expended.

It is proposed that the expenditure for this great scheme of exploration of the vast interior of Australia, and the scientific researches contingent

upon it, shall be borne by the mother country in conjunction with the various Australian colonies. So soon, therefore, as an arrangement to that effect may have been arrived at, trustees should be appointed, residing in the colonies, who would act as a Committee of administration, such Committee to consist of not more than five members. All funds would be placed at their disposal, and all money transactions in connexion with the expedition would be made under their supervision and subject to their approval.

The objects of the expedition having been attained, and the time arrived when the same is to be broken up, the residue of the stock and stores—horses, camels, equipment, instruments, provisions, &c.—would be handed over to the Committee of Trustees, to be disposed of as they might think fit.

All observations, journals, maps, natural-history collections, drawings, and photographs, without exception, would likewise be handed over to the Committee of Trustees on the completion of the expedition, in order that the same may be turned to account in furtherance of the interests of science, and of the various countries that have taken a part in this great undertaking. Members of the expedition would not be permitted to make private collections, and none of the results would be made public, unless by special authority of the Committee of Trustees.

Such are the objects and leading principles of an expedition which I hope to be able in person to carry out; and I trust it will not be deemed presumptuous on my part if I add a few words in support of my claims to be entrusted with the conduct of so noble an enterprise. I have been connected with Australia, with brief interruptions, ever since the year 1852, and the greater part of that long interval of time has been employed by me in studying the physical character of that great continent. In 1858 I succeeded in establishing an Observatory at Melbourne for the advancement of our knowledge of terrestrial physics, and my labours and publications on the observations made up to the time of my resignation and retirement from that institution in 1863, will in a very short time come to a conclusion. The magnetical and other observations collected during my travels through Victoria, part of New South Wales, and of South Australia, comprising an area of nine degrees of longitude and six degrees of latitude, are now in course of publication on behalf of the colony of Victoria. As soon as this is accomplished, I purpose again to devote my energies to further inquiries respecting the physical geography of Australia, but on this occasion likewise as an explorer. In this determination I am prompted by no other motives than the advancement of science, and my attachment towards a rising country—an attachment not unnatural, after a connexion extending over a period of so many years.

It is on these grounds that I solicit the powerful support of this Society in this great national undertaking, which, I am persuaded, will, if successfully carried out, conduce equally to the advancement of the interests of science and to the material welfare of Australia. Men may differ as to the

mode of proceeding in its execution; but none, I presume, will venture to deny its importance, especially with regard to the development of the natural resources of that immense area, in the interests of civilization— none, I feel sure, will oppose it as being premature or inopportune. It would be presumptuous on my part were I to urge the importance of the opening up of the western interior for the successful settlement of Western and North-Western Australia, which at some future time, and under certain contingencies that might arise, would have a most important bearing on the security of the British possessions in India. For I am well aware that men, regarded as authorities in colonial policy, have long ago brought this subject under the consideration of the Government. Nor need I speak of the enlightened spirit in which the various Governments in Australia have ever shown themselves ready to assist the cause of exploration and scientific research. The many and valuable contributions that science, in nearly every one of its branches, has received from the colonies cannot fail to assure us of their assistance and cooperation in a systematic and scientific exploration of the unknown interior round which they are clustered. I feel confident there needs but an impulse from England, and the sanction of its highest scientific authorities, to secure for the undertaking a ready assent and strenuous support on the part of the people and the Governments of Australia.

APPENDIX.

A.—*Annual Expenditure of the Expedition.*

a. Salaries and wages for the members of the expedition proper.

1. Leader, annually	£450	0	0
Assistant leader, annually	350	0	0
Storekeeper and overseer, annually	200	0	0
Saddler, tent-maker, &c., monthly £12	144	0	0
Blacksmith, wheelwright, monthly £12	144	0	0
	£1288	0	0
2. Twelve stockmen at £8 a month	£1152	0	0
Three aboriginals at £4 a month	144	0	0
	£1296	0	0

b. Salaries for the scientific men.

Geologist, mineralogist, annually	£300	0	0
Botanist and medical officer	300	0	0
Zoologist, palæontologist, medical assistant	300	0	0
Artist and photographer	300	0	0
Assistant observer for physical science	200	0	0
	£1400	0	0

Total Salaries and Wages.

a. 1	£1288	0	0
a. 2	1296	0	0
b.	1400	0	0
	£3984	0	0

c. Expenditure for provisions, tobacco, &c.	£866	0	0
And for wear and tear and repairs..................	200	0	0
	£1066	0	0

Total of the annual expenditure........	£3984	0	0
	1066	0	0
	£5050	0	0
Total for three years and a half	£17675	0	0

B.—*Expenditure of the Expedition for Instruments, Outfit, exclusive of Provisions.*

1. Instruments—Astronomical	£200	0	0
Physical and meteorological	250	0	0
Physiological and botanical	80	0	0
Surgical medicine-chest	60	0	0
Geological	40	0	0
Photographical	50	0	0
Packing and transport to Australia	60	0	0
	£740	0	0
2. Tools and implements, including rifles, revolvers, ammunition, rockets, blue lights, horseshoes, &c.	£400	0	0
3. Tents, a boat, and a small iron vehicle..............	£300	0	0
4. Saddlery, including thirty riding saddles and pack saddles, water bags, &c..	£440	0	0
5. Live stock; fifty horses at £20 each................	£1000	0	0
Some sheep......	100	0	0
	£1100	0	0

TOTAL OUTFIT.

1	£740	0	0
2	400	0	0
3	300	0	0
4	440	0	0
5	1100	0	0
	£2980	0	0

C.—*Expenditure during and prior to the Organization of the Expedition.*

Expenses in Europe..........	£100	0	0
Passage money for the officers to Australia..........	380	0	0
Probable expenditure prior to the organization	400	0	0
Total	£880	0	0

GRAND TOTAL EXPENDITURE.

A	£17,675	0	0
B	2,980	0	0
C	880	0	0
	£21,535	0	0

II. "On some New Derivatives of Acetone." By MAXWELL SIMPSON, M.D., F.R.S. Received April 18, 1868.

The compounds which form the subject of the present paper came accidentally under my notice whilst I was engaged in an unsuccessful attempt to form leucic acid by a new synthesis.

By saturating a mixture of acetone and absolute alcohol with dry hydrochloric acid gas, I had hoped to form a body having the composition

$$C_5H_{11}OCl-C_3H_6O+C_2H_6O+HCl=C_5H_{11}OCl+H_2O,$$

and that this, when treated successively with cyanide of potassium and caustic potash, would have yielded the desired acid according to the following equations:—

$$C_5H_{11}OCl+KCy=C_5H_{11}OCy+KCl,$$

and

$$C_5H_{11}OCy+\left.\begin{matrix}K\\H\end{matrix}\right\}O+\left.\begin{matrix}H\\H\end{matrix}\right\}O=C_6H_{11}KO_3+NH_3.$$

By saturating the above mixture with hydrochloric acid, I obtained, it is true, a large quantity of a chlorinated oil; but I could not ascertain whether it was the compound I expected or not, as I found it impossible to render

it sufficiently pure for analysis. I therefore subjected it directly (having simply washed it with a dilute solution of carbonate of soda) to the action of the before-mentioned reagents *. The results were altogether unexpected.

I have since ascertained that the chlorinated oil can be obtained without the intervention of alcohol. I will now give a detailed account of my experiments.

Dry hydrochloric acid gas was passed to saturation into a quantity of pure acetone contained in a glass vessel surrounded with cold water. The product was set aside for ten or twelve days, and then well washed with a dilute solution of carbonate of soda. Equal weights of the oil thus formed and of pure cyanide of potassium were then introduced into a glass balloon together with a large quantity of alcohol. The balloon was attached to a reversed Liebig's condenser, and subjected for about twelve hours to the temperature of a water-bath. At the expiration of this time its contents were, when quite cold, filtered, and the precipitate well washed with cold alcohol, and then with cold distilled water till the wash-water ceased to give a precipitate with nitrate of silver. A white powder was thus obtained which was insoluble in cold water and in cold alcohol. Boiling alcohol, however, dissolved it to a small extent, from which it crystallized on cooling in beautiful shining plates like naphthaline. These sublime at a high temperature (about 300° C.), apparently without decomposition. It is a neutral body and contains nitrogen. It does not evolve ammonia when heated with an alcoholic solution of caustic potash. It is decomposed by nitrous acid, with the production of a compound having acid properties. The composition of this body I hope to be able to give in a future communication.

I will now turn to the alcoholic solution filtered from the neutral body I have just described. This I introduced into a balloon together with some sticks of caustic potash. The balloon was then attached to a reversed Liebig's condenser, and exposed to the temperature of a water-bath till ammonia had ceased to be evolved. When this was observed, the alcohol was distilled off and the residue dissolved in water. The solution was then neutralized with hydrochloric acid, filtered, evaporated considerably, and then treated with a large excess of the same acid. After standing for some time it became a mass of crystals. These were thrown upon a filter and washed with cold distilled water till the filtrate ceased to give a precipitate with nitrate of silver. The powder obtained in this way was readily purified by crystallizing from hot alcohol, and then from boiling water. From the latter solvent it separates in brilliant colourless prismatic needles, sometimes upwards of an inch in length. Dried at 100° C., these gave the following numbers on analysis, which accord sufficiently well with the formula $C_8 H_{13} NO_3$:—

* I had performed these experiments before the appearance of Baeyer's paper "Ueber die Condensationsproducte des Acetons," Annalen der Chemie, vol. cxl. p. 297.

		Theory.					
			Per cent.	I.	II.	III.	IV.
C_8	....	96	56·14	56·28	56·79		
H_{13}	....	13	7·60	7·97	8·21		
N	....	14	8·19	..	..	8·32	8·23
O_3	....	48	28·07				
		171	100·00				

I have also prepared and analyzed the silver-salt of this acid. The results I obtained confirmed the above formula.

	Theory. Per cent.	Experiment. I.	Experiment. II.
Metallic silver ($C_8H_{12}AgNO_3$)	38·84	38·98	38·70 *

The salt was obtained in beautiful mother-of-pearl plates by boiling a solution of the acid with an excess of freshly prepared oxide of silver. It is very soluble in water, and is not much affected by light. It does not suffer decomposition when dried at 100° C.

The new compound has an acid reaction, and displaces carbonic acid from the soluble carbonates. It is insoluble in cold, pretty soluble in hot water and in cold alcohol, and sparingly soluble in ether. It melts at 171° C. The nitrogen appears to be retained with unusual force within the molecule of the new acid. It refuses to give it up in the form of ammonia when subjected to the action of an alcoholic solution of potash, a fact we have already learned from the manner of its formation. Neither can it be made to yield it up by exposing it to the action of nitrous gas. I have tried this gas upon a solution of the acid both in water and nitric acid. It dissolves in large quantity in strong hydrochloric acid, and, on standing, crystallizes out unaltered, not combining chemically with that body.

The salts of this acid are, as a general rule, very soluble in water. The neutralized acid yields no precipitate with nitrate of silver, corrosive sublimate, or chloride of barium. It renders a solution of acetate of lead but slightly turbid, and communicates a red colour to perchloride of iron without precipitating it.

The soda-salt is very soluble and does not crystallize well. It is prepared by neutralizing the acid with carbonate of soda. One molecule of the acid, assuming it to have the composition $C_8H_{13}NO_3$, I found required exactly one molecule of pure and recently ignited carbonate of soda for complete neutralization. This experiment, and the composition of the silver-salt, render it highly probable that the acid is monobasic.

The mercury-salt crystallizes in beautiful pearly plates. It is prepared by boiling a solution of the acid with freshly precipitated oxide of mercury. It is a very soluble salt.

* The salt which served for this analysis was made from a freshly prepared batch of the acid.

When acetone is saturated with hydrochloric acid, several condensed compounds are formed, which remain in union with the acid. The question now arises, which of these compounds generate the acid we have just been studying? and which the neutral body? In the hope of determining this point, I prepared the two most important of these compounds in a state of purity, namely oxide of mesityle and phoron, and saturated them with hydrochloric acid gas. After the lapse of twelve hours the two saturated bodies were well washed with water, and treated separately with cyanide of potassium and caustic potash in the manner I have just described. The results were decisive. The mesityle compound yielded the acid, and the phoron compound the neutral body. The following equations will explain the formation of the acid:—

$$C_6H_{10}\Theta + 2HCl = C_6H_{12}\Theta Cl_2,$$

$$C_6H_{12}\Theta Cl_2 + 2KCy = C_6H_{12}\Theta Cy_2 + 2KCl,$$

and

$$C_6H_{12}\Theta Cy_2 + \left.\begin{matrix}K\\H\end{matrix}\right\}\Theta + \left.\begin{matrix}H\\H\end{matrix}\right\}\Theta = \underset{\text{Potash-salt of new acid.}}{C_8H_{12}KN\Theta_3} + NH_3.$$

It will be observed that only one of the cyanogen atoms is transformed into $C\Theta\Theta K$.

The foregoing derivatives of acetone are, I think, in many respects very remarkable bodies. I therefore propose to submit them to a careful study. I propose also to ascertain whether or not the true aldehydes yield analogous bodies when treated in a similar manner.

III. "Researches on the Hydrocarbons of the Series $C_n H_{2n+2}$.—No. IV." By C. Schorlemmer. Communicated by Prof. G. G. Stokes. Received April 13, 1868.

On the relation between Boiling-point and Chemical Structure.

It is from researches published only during last year that we have obtained a more definite knowledge of the chemical structure of some of the hydrocarbons of the above series, so that we are enabled to explain the mode in which the carbon atoms are united. This has been achieved by obtaining these hydrocarbons by synthesis from other compounds, the structure of which is perfectly well known.

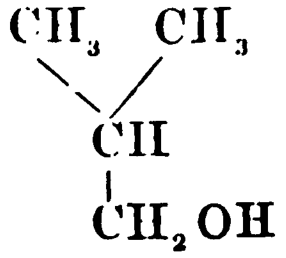
Thus Friedel and Ladenburg* prepared, by acting upon methylchloracetol, $C\left\{\begin{matrix}CH_3\\CH_3\\Cl_2\end{matrix}\right.$, with zincethyl, the hydrocarbon C_7H_{16}, which they call carbdimethyldiethyl, and which has the structure $C\left\{\begin{matrix}CH_3\\CH_3\\C_2H_5\\C_2H_5\end{matrix}\right.$. Butlerow†

* Ann. der Chem. und Pharm. vol. cxlii. p. 310.

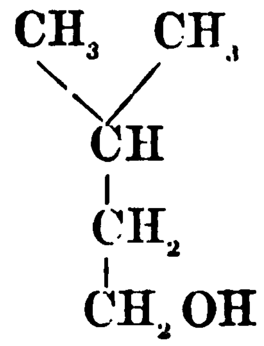
† Ibid. vol. cxliv. p. 10.

replaced in tertiary butyl alcohol the group HO by hydrogen, and obtained an isomer of diethyl to which he gives the name trimethylformen, $C\begin{cases} CH_3 \\ CH_3 \\ CH_3 \\ H \end{cases}$. In my last communication to the Society I described di-isopropyl and amylisopropyl, and pointed out their constitution*. Further, Erlenmeyer has shown that amyl alcohol and butyl alcohol formed by fermentation have the following structure†:—

Butyl alcohol.



Amyl alcohol

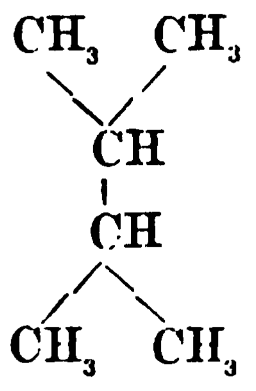


These two compounds contain, therefore, the group isopropyl, $CH(CH_3)_2$, which also must be present in the hydrocarbons derived from these alcohols.

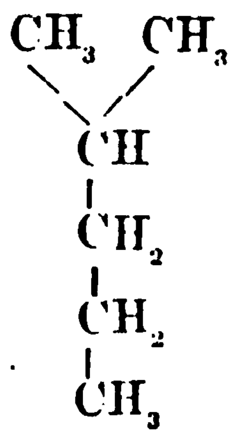
All hydrocarbons of known structure may be arranged in four groups; the members of each group, which are liquids at the mean temperature of the air, exhibit a very regular increase in the boiling-point for each increase of CH_2.

1*st. group*. Hydrocarbons in which each atom of carbon is united with not more than two other carbon atoms, or in which the carbon atoms are arranged in a single chain.—To this group belong the three lowest members of the series $C_n H_{2n+2}$, of which no isomers exist, as well as diethyl, $C_4 H_{10}$, hexylhydride, $C_6 H_{14}$, derived from suberic acid ‡, and heptylhydride, $C_7 H_{16}$, from azelic acid §. My reasons for considering that the two last ones belong to this group are: (1) They boil at a higher temperature than their isomers of known structure; and we find that the simpler the manner in which the carbon atoms are combined, the higher the boiling-point. Thus we have,—

Di-isopropyl boils at 58° C.



Ethylbutyl boils at 62° C.



* Proc. Roy. Soc. vol. xvi. p. 34.

† Zeitschrift für Chemie. vol. iii. p. 117.

‡ Journ. Chem. Soc. N. S., vol. ii. p. 260.

§ Proc. Roy. Soc. vol. xiv. p. 464.

Now hexylhydride boils at 69°·5 C., and the only structure more simple than ethylbutyl is the following, which must express that of hexylhydride,

$$\begin{array}{c} CH_3 \\ | \\ CH_2 \\ | \\ CH_2 \\ | \\ CH_2 \\ | \\ CH_2 \\ | \\ CH_3 \end{array}$$

The same is the case in the hydrocarbons having the composition $C_7\ H_{16}$,—

Carbdimethyldiethyl. Boiling-point 86°.	Ethylamyl. Boiling-point 91°.	Heptylhydride. Boiling-point 100°·5.
$\begin{array}{c} CH_3 \\ \vert \\ CH_2 \\ \vert \\ H_3C\text{-}C\text{-}CH_3 \\ \vert \\ CH_2 \\ \vert \\ CH_3 \end{array}$	$\begin{array}{c} CH_3\ \ CH_3 \\ \diagdown\ \diagup \\ CH \\ \vert \\ CH_2 \\ \vert \\ CH_2 \\ \vert \\ CH_2 \\ \vert \\ CH_3 \end{array}$	$\begin{array}{c} CH_3 \\ \vert \\ CH_2 \\ \vert \\ CH_2 \\ \vert \\ CH_2 \\ \vert \\ CH_2 \\ \vert \\ CH_2 \\ \vert \\ CH_3 \end{array}$

(2) The formation of these hydrocarbons points out that they must have a very simple constitution. The acids from which they are derived are obtained by the splitting up of compounds containing a large number of carbon atoms, and from these acids they (hydrocarbons) are derived by a further separation of carbon The difference in the boiling-points of hexylhydride and heptylhydride is 31° C.

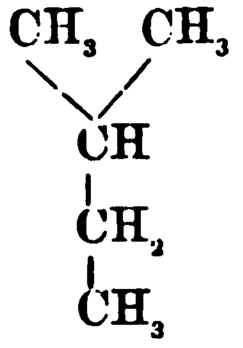
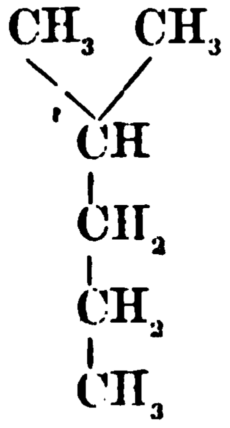
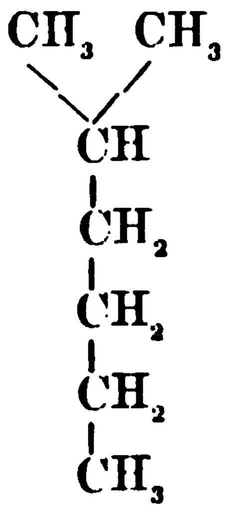
	Boiling-point.
C_6H_{14}	69°·5
C_7H_{16}	100·5

The hydrocarbon C_6H_{14}, found in American petroleum appears to be identical with that prepared from suberic acid. The higher specific gravity which the hexylhydride from rock-oil shows, as observed by Cahours and Pelouze and by me, is occasioned by impurities. As I have shortly mentioned in my last communication, I have studied the action of nitric acid upon this hydrocarbon. On oxidizing in this way about 120 grms., of which the boiling-point was 67° to 69° and the specific gravity 0·6709, at 15°, about 10 grms. were left unattacked, which certainly must have been very pure. This remaining portion boiled at 70°, and had the specific gravity 0·6651 at 16°·5. The hexylhydride which Erlenmeyer and

Wanklyn prepared from mannite * also exhibits in its boiling-point and specific gravity a close agreement with the hydrocarbon from suberic acid, and appears to be the same,—

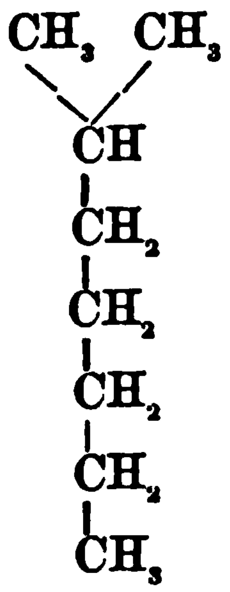
	Boiling-point.	Spec. grav.
C_6H_{14} from suberic acid	69°·5	0·6617 at 17°·5
„ „ mannite	68-70	0·6645 at 16·5
„ „ rock-oil........	70	0·6651 at 16·5

2nd group. Hydrocarbons in which one atom of carbon is united with three others, or which contain the group isopropyl.—The members of this section are trimethylformen, amylhydride, ethylbutyl, ethylamyl, and the hydrocarbon C_8H_{18} which I have prepared from the caprylalcohol, obtained from castor oil †. Trimethylformen boils at about −15° C.; the other members are liquid at common temperature, and show the same difference in their boiling-points as those of the first group, namely 31°.

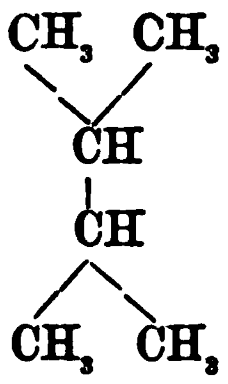
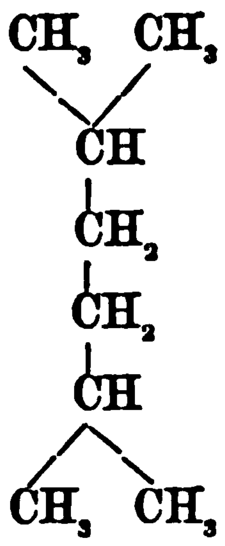
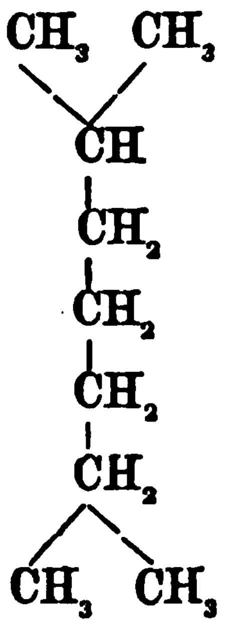
		Boiling-point. Observed.	Boiling-point. Calculated.
Amylhydride, C_5H_{12} =		30°	30°
Ethylbutyl, C_6H_{14} =		62°	61°
Ethylamyl, ..C_7H_{16} =		91°	92°

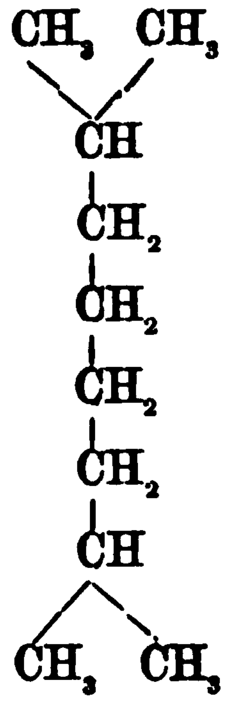
* Journ. Chem. Soc. N. S. vol. i. p. 233.

† This alcohol is, as I have found, a secondary or iso-alcohol. An account of this investigation, and the reasons why I class the hydrocarbon derived from the alcohol in the above group, I shall communicate to the Society shortly.

			Boiling-point.	
			Observed.	Calculated.
Octylhydride, C_8H_{18} (from caprylalcohol)	=		124°	123°

3rd group. Hydrocarbons which contain the group isopropyl twice.—The difference in the boiling-points of the members of this group is 25° C. They are di-isopropyl, dibutyl, which, as I have shown, is identical with amylisopropyl, butylamyl, and diamyl.

Di-isopropyl, C_6H_{14}	=		58°	58°
(Not known) C_7H_{16}		——	—	83°
Dibutylamyl, isopropyl. } C_8H_{18}	=		109°	108°
Butylamyl .. C_9H_{20}	=		132°	133°

		Boiling-point.	
		Observed.	Calculated.
Diamyl ....$C_{10}H_{22}$ =		158°	158°

4th group. Hydrocarbons in which one atom of carbon is combined with four other carbon atoms.—Of this group only one member is known, namely carbdimethyldiethyl, which boils at 86°.

It thus appears that from the boiling-point of a hydrocarbon of the series $C_n H_{2n+2}$ conclusions may be drawn concerning its constitution, just as in the series of aromatic hydrocarbons *. Further researches must show whether the law which I have pointed out in this paper is a general one.

IV. "Researches on the Hydrocarbons of the Series $C_n H_{2n+2}$.—No. V." By C. Schorlemmer. Communicated by Professor G. G. Stokes, Sec. R.S. Received May 7, 1865.

Oxidation Products.

In a former communication† I have shortly described the action of different oxidizing agents upon some of the saturated hydrocarbons; the following paper contains some further results which I have since then obtained. One of the most striking properties of these compounds is, that they are with the greatest difficulty acted upon by any oxidizing substance in the cold. On heating them, however, a reaction sets in, and either they are completely burnt up to carbonic dioxide and water, or other oxidation products besides those two are formed in comparatively small quantities; thus chromic acid produces some acetic acid. Fuming nitric acid, which in the cold shows no action whatever, even if left in contact with one of these hydrocarbons for months, acts rather violently on gently heating; acid of the specific gravity 1·4 acts in a similar way, and produces the same products, but the reaction is much less violent. The apparatus which I

* Compare, about the boiling-points of the aromatic hydrocarbons, the elaborate paper of Kopp in Ann. der Chem. und Pharm. vol. v. (Supplement) p. 315.

† Proceedings of the Royal Society, vol. xvi. p. 38.

used consisted of a glass flask of about one litre capacity, the narrow neck of which was several feet in length, and surrounded by a wider tube through which cold water flowed. The hydrocarbons treated in this way were hexylhydride and octylhydride (from petroleum), and diamyl. They were heated with the acid as long as red fumes were evolved; the liquid left in the flask was then distilled in a retort, until the unaltered hydrocarbon together with the greater part of the diluted nitric acid had passed over. The syrupy residue was heated in a steam-bath as long as nitric acid vapours escaped. A thick syrupy mass was left, from which, on cooling, a crystallized acid was deposited; on adding water these crystals dissolved, whilst a thick yellowish oil separated. This oil is insoluble in water, but somewhat soluble in the aqueous solution of the crystalline acid, which therefore cannot be obtained quite free from the oily substance by recrystallization only; but this may be effected by washing the crystals with cold ether, which dissolves very little of them, whilst the oil itself is very soluble. The acid obtained from octylhydride and that from diamyl melted at 180° C., and showed all the characteristic reactions of succinic acid; that from hexylhydride, from which I obtained only a very small quantity, could not be completely freed from the yellow oil, and therefore did not show a definite melting-point; it began to fuse at about 120°, and became perfectly liquid at 150°; it exhibited, however, all the reactions of succinic acid; and the following analyses, although they do not agree very well, yet show that it was this compound. From the acids the calcium and the silver-salt were prepared by neutralizing the aqueous solution with calcium carbonate and concentrating the filtered solution by boiling, when the salt separated in microscopic needles. Calcium succinate obtained in this way has the formula $C_4 H_4 CaO_4 + H_2 O$; the quantities of water and calcium found agree with this composition. The water was determined by drying the salt at 180° C., and the calcium by heating the dried salt over the blowpipe until the residue had a constant weight.

	Calculated for $C_4 H_4 CaO_4 + H_2 O$.	Found: Acid from hexylhydride.	Found: From diamyl.
H_2O	10·3 per cent.	9·4 per cent.	9·8 per cent.
CaO	32·2 ,,	33·3 ,,	32·6 ,,

To obtain the silver-salts, the solution of the calcium-salt was precipitated by silver-nitrate and the washed precipitate dried at 120°, and the silver determined by igniting.

	Calculated for $C_4 H_4 Ag_2 O_4$.	Found: From hexylhydride. I.	Found: From hexylhydride. II.	Found: From octylhydride.	Found: From diamyl.
Ag =	65·06 per cent.	62·3 p. c.	64·1 p. c.	64·6 p. c.	64·7 p. c.

The yellow oil which is formed besides succinic acid contains nitrogen; it is not volatile, and decomposes on heating; caustic potash converts it

into a red resinous substance. It dissolves very easily in fuming nitric acid; on boiling this solution for some time the oil is converted into a white solid, which separates as crystalline powder on addition of water, and which crystallizes from alcohol in large flat needles. I did not obtain sufficient of this compound for a satisfactory analysis; also the quantity of succinic acid obtained is always very small in comparison to the amount of hydrocarbon used, which is to the greatest part oxidized to water and carbonic dioxide.

The distillate obtained after the oxidation contains, besides unaltered hydrocarbon and diluted nitric acid, also a small quantity of fatty acids and of nitriles. I have only examined those which were derived from diamyl. The liquid was neutralized with sodium-carbonate, the diamyl separated and distilled; after the hydrocarbon had passed over, the thermometer rose, and between 230°–235° a small quantity of a yellowish liquid passed over, which had the characteristic smell of the nitriles of the fatty acids. On heating it with an alcoholic potash solution, ammonia was given off. I tried to convert the potassium-salt thus obtained into the silver-salt, but obtained the latter only in small quantity and in an impure state, so that I could not analyze it. The boiling-point of the nitrile agrees with that calculated for caprinitrile, $C_{10}H_{19}N$. The solution of the sodium-salts was evaporated, and the residue distilled with a small quantity of nitric acid; the acid distillate, on which oily drops swam, smelt of valerianic acid. It was neutralized with ammonia, and precipitated with silver-nitrate in three fractions.

(1) Fraction contained 45·27 per cent. of silver.
(2) „ „ 46·84 „ „
(3) „ „ 49·50 „ „

Silver-œnanthylate contains 45·57 per cent. Ag, and silver-valerate 51·67 per cent. Ag. The fatty acids, which were formed by oxidizing diamyl with nitric acid, consisted therefore of œnanthylic, valerianic, and no doubt also caproic acids.

I have further examined the products which are obtained by oxidizing the amyl acohol derived from petroleum. As I have shown in a former communication*, the amyl compounds obtained from fusel-oil appear to be identical with those from petroleum, as they have the same specific gravity and the same boiling-points; the only difference which I found is that the boiling-point of amylhydride is about 4° higher than that derived from the fermentation alcohol. The whole quantity of amyl alcohol which I had prepared from petroleum was only 3 grammes. As oxidizing mixture I used a solution of two parts of potassium bichromate in ten parts of water, to which was added three parts of sulphuric acid. The alcohol was added to the cold liquid, and the reaction moderated by surrounding the flask with cold water. As soon as the reaction was finished the liquid was distilled, and the acid distillate, which smelt strongly of valerianic acid, neutralized

* Proceedings of the Royal Society, vol. xv. p. 131.

with sodium-carbonate; a small quantity of a neutral oil remained undissolved, which was removed, and the solution of the sodium-salt evaporated to dryness. The residue was distilled with sulphuric acid, and from the distillate, on which an oily acid swam, a silver-salt prepared.

(1) 0·3190 of this salt gave 0·1830 Ag.
(2) 0·2408 of salt of another preparation gave 0·1358 Ag.

Salt No. 1 contained, therefore, 57·3 per cent. Ag, and No. 2 56·4 per cent. Ag. This composition differs very much from silver-valerate, which contains 51·67 per cent. Ag; yet the characteristic odour of the distillate leaves no doubt that a considerable quantity of valerianic acid was present, which must have been mixed with a lower member of the fatty acid series. I soon found that acetic acid was present; for on distilling the residue in the retort and collecting separately the last distillate, a liquid was obtained which smelt of acetic acid. It was converted into the silver-salt. 0·2340 of this salt gave 0·1485 Ag, or contained 63·46 per cent. Ag, whilst the calculated percentage is 64·67 per cent. Pedler has found that active amyl alcohol yields on oxidation, besides valerianic acid, a considerable quantity of acetic acid*, the same products, therefore, as amyl alcohol from petroleum.

The neutral oil above mentioned was treated again with the oxidizing mixture, which had hardly any action on it in the cold; it was therefore dried and distilled. The greatest portion boiled between 97°–120°, and distilled on rectification nearly wholly between 95°–105°. It had a pleasant fruity smell, and formed with hydrogen sodium-sulphite a crystalline compound. The analysis yielded the following results:—

0·3080 substance gave 0·7740 CO_2 and 0·3220 H_2O.

C	= 68·5
H	= 11·6
O	= 19·9
	100·0

The only simple formula which can be calculated from these numbers is $C_5 H_{10} O$, although the quantity of carbon is 1 per cent. short; probably it contained a little amylacetate; the odour of it certainly resembled that of this ether, and, as the following calculated compositions of $C_5 H_{10} O$ and of amylacetate show, such an admixture would lower the percentage of carbon.

	Calculated composition of	
	$C_5 H_{10} O$.	$\left.\begin{matrix} C_5 H_{11} \\ C_2 H_3 O \end{matrix}\right\} O$.
C	69·77	64·61
H	11·63	10·77
O	18·60	24·62
	100·00	100·00

* Journ. Chem. Soc. 2 ser. vol. vi. p. 76.

How this compound, which without doubt was an aceton, has been formed I am at a loss to understand. The very small quantity of liquid boiling above 120° consisted chiefly of amylvalerate; at least it had the odour of this ether.

I should have wished to be able to give more definite results, but the preparation of amyl alcohol from petroleum is difficult and requires a very long time. But as the oxidation-products of the different amyl alcohols are being at present investigated by different chemists, I thought it would not be without interest to publish these incomplete results.

V. "On the Constitution of Capryl Alcohol from Castor-oil." By C. Schorlemmer. Communicated by Prof. G. G. Stokes, Sec. R.S. Received May 7, 1868.

There is perhaps no other compound known which has been so often and so fully investigated by different chemists, and yet whose constitution is clouded in so much obscurity, as the alcohol which is obtained by distilling castor-oil soap with caustic alkalies. From the time of its discovery until recently, this compound has been alternately considered by one investigator to be capryl or octyl alcohol, and by another to consist of œnanthyl or heptyl alcohol. As a proof that it is capryl alcohol, Bouis states that, by the moderate action of nitric acid, a small quantity of caprylic acid is produced, the greater part of the alcohol, however, being oxidized to lower members of the fatty acid series*; and Kolbe concludes, from the formation of these acids, that it is a secondary or isoalcohol, probably methyl-hexyl carbinol, $C\left\{\begin{array}{l}CH_3\\C_6H_{13}\\H\\OH\end{array}\right.$ †. As I shall show in this paper, Kolbe's view is correct; by moderate oxidation, the alcohol loses two atoms of hydrogen, and is converted into the corresponding acetone, methyl œnanthol, the same compound which is generally obtained as a byeproduct in the preparation of the alcohol. The alcohol which I used was prepared by distilling a mixture of castor-oil soap and caustic soda in a flask of thin copper-sheeting as quickly as possible. The distillate was repeatedly rectified over fused caustic potash, the portion boiling below 200° C. only being collected. The alcohol was isolated from this liquid by fractional distillation; its corrected boiling-point was 181° C. The portions having a lower boiling-point consist of hydrocarbons, which combine with bromine, probably members of the olefine series, amongst which octylene, boiling at 125° C., preponderates. A considerable quantity of liquid distilled above 160°, the boiling-point remaining somewhat constant at 170° C. Neither

* Ann. de Chim. et de Phys. 3e Sér. vol. xliv. p. 123.

† Ann. der Chem. und Pharm. vol. cxxxii. p. 116.

this fraction nor any other distillate contained an acetone, as none combined with hydrogen-sodium sulphite. According to Chapman, the liquid boiling at 170° consists chiefly of heptyl alcohol*. In order to isolate this alcohol, I acted upon the liquid boiling between 160°–175° with iodine and phosphorus. The product, subjected to fractional distillation, was found to consist of isoctyl iodide, boiling at 210°–215°, and of hydrocarbons, distilling below 160°; the portion which came over between 160° and 210° was very small, and diminished after each further distillation. This shows that no heptyl alcohol was present, and that the original liquid boiling at 170° was a mixture of isoctyl alcohol and hydrocarbons, which could not be separated by simple fractioning.

To obtain the oxidation products of isoctyl alcohol, I acted upon it with a solution consisting of 3 parts of sulphuric acid, 2 parts of potassium bichromate, and 10 parts of water, the reaction being moderated by surrounding the vessel with cold water. As soon as no further action was observed, the liquid was distilled. The distillate consisted of an aqueous liquid, which had a slight acid reaction, and a light oily fluid; it was neutralized with sodium carbonate, and the oil treated again with the oxidizing mixture, which, however, had hardly any action upon it in the cold. This oily liquid is methyl œnanthol; it has the characteristic odour of that compound, and when shaken with a concentrated solution of hydrogen-sodium sulphite, solidifies to a mass of pearly white crystals. These were dried between blotting-paper, and decomposed by a dilute solution of caustic soda. The oil which separated was dried over calcium chloride; it distilled completely between 170°–172°, the boiling-point of methyl œnanthol being 171°.

In order to oxidize this compound further, it had to be heated with the oxidizing mixture, when a slight evolution of carbon dioxide was observed. The acid distillate was neutralized with sodium carbonate, and the acetone unacted upon treated again as before. The different solutions of the sodium-salts were evaporated, and the solid residue decomposed by diluted sulphuric acid. An oily acid separated, which, after drying, distilled at 198°–200°, which is the boiling-point of caproic acid, the characteristic odour of which it also exhibited. To place beyond doubt that it was really this compound, a portion of it was neutralized with ammonia and precipitated by silver nitrate. A white curdy precipitate was obtained, which was nearly insoluble in cold water, and only slightly soluble in boiling water. It did not darken either by exposure to the light or to a temperature of 100°. From the hot saturated solution it separated as a crystalline powder; nor could I obtain it in definite crystals by evaporating the solution *in vacuo*.

(1) 0·2407 of this salt gave 0·1170 silver.
(2) 0·3720 gave 0·1810 silver.

* Journ. Chem. Soc. New Ser. vol. iii. p. 295.

Calculated for $C_6H_{11}AgO_2$	Found I.	Found II.
48·43 per cent. Ag.	48·60 per cent.	48·65 per cent.

Another portion of the acid was converted into the barium-salt, which crystallized from a hot saturated solution in long needles, grouped in stars, the characteristic form of barium caproate.

0·2475 of the barium salt gave ·1318 barium carbonate.

Calculated for $(C_6H_{11}O_2)_2Ba$.	Found.
37·33 per cent. Ba.	37·40 per cent.

I also prepared the ethyl compound, which I found to boil at 160°–162°; the boiling-point of ethyl caproate is, according to Fehling, 162° *.

Besides caproic acid, a large quantity of acetic acid was formed, which was isolated by distilling the aqueous liquid from which the caproic acid had been removed. With the first portion of the distillate, oily drops of caproic acid came over; the latter portion, which was collected separately, had the pure odour of acetic acid; it was rectified twice, and the last portion collected, which was boiled with silver carbonate. From the filtered solution silver acetate crystallized on cooling in characteristic long, flat needles.

(1) 0·2140 of this silver-salt gave 0·1373 silver.

(2) 0·5895 of the salt gave 0·3825 silver.

Calculated for $C_2H_3AgO_2$.	Found I.	Found II.
64·68 per cent. Ag.	64·20 per cent.	64·88 per cent.

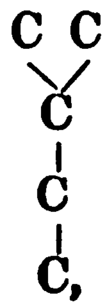
These experiments prove that the capryl alcohol from castor-oil is a secondary or isoctyl alcohol to which, adopting the nomenclature proposed by Kolbe †, the name methyl hexyl carbinol, $C\begin{cases}CH_3\\C_6H_{13}\\H\\OH\end{cases}$ must be given. This alcohol yields, on oxidation, first, the corresponding acetone, methyl œnanthol, $C\begin{cases}CH_3\\C_6H_{13}\\O\end{cases}$, which, under further oxidation, splits up into caproic and acetic acid, exactly as the theory requires.

Another question to be answered was, what is the structure of the group Hexyl C_6H_3, contained in this isoalcohol? The caproic acid contained in fats and that prepared from amyl cyanide appear to be identical with that which I obtained ‡. Now, according to Erlenmeyer, in the amyl compounds the carbon atoms are grouped in the following manner:—

* Ann. der Chem.und Pharm. vol. liii. p. 408.

† Ibid. vol. cxxxii. p. 103.

‡ Caproic acid deviates the plane of the polarized light, whilst that from cocoa-nut-

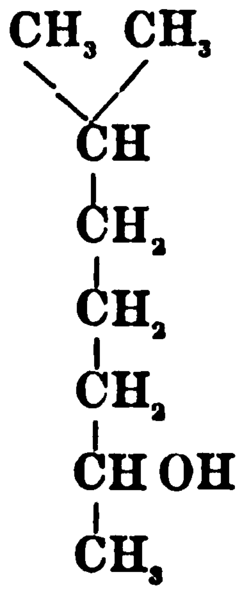


and this grouping must therefore also exist in caproic acid and in isoctyl alcohol. To obtain evidence respecting this question, I converted the alcohol into the corresponding hydride by a very simple method, which is a general one, and by which, with the greatest ease, the corresponding hydrocarbon can be obtained from any alcohol. The alcohol was first converted into the iodide, which was brought together with zinc turnings and diluted hydrochloric acid in a flask which was surrounded with cold water. After a few hours the heavy layer of iodide had disappeared, and a light liquid swam on the top. This consisted almost entirely of the hydrocarbon $C_8 H_{18}$; traces of iodide and alcohol which still adhered were removed by treating the liquid with sulphuric and nitric acids, and by distilling it over sodium. The pure hydrocarbon boils constantly at 124° C., and has the specific gravity 9·7083 at 12°·5.

0·2870 of this liquid gave 0·8840 CO_2 and 0·4150 H_2O.

	Calculated.		Found.
C_8	96	84·2	84·0
H_{18}	18	15·8	16·1
	114	100·0	100·1

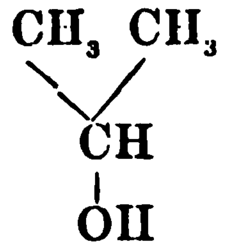
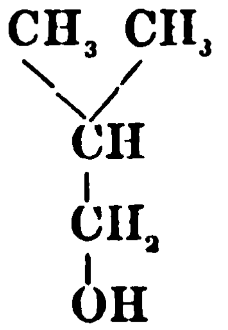
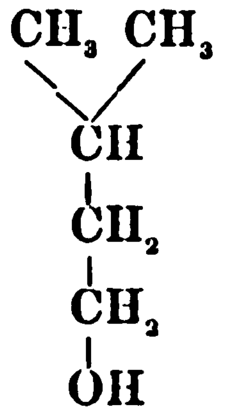
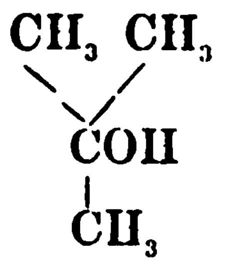
As I have shown in my last communication to the Society, a hydrocarbon, having the formula $C_8 H_{18}$ and the boiling-point 123°, contains one carbon atom, which is combined with 3 others, or the carbon atoms are grouped in a similar manner as in the amyl compounds. The structure of isoctyl alcohol will therefore most probably be expressed by the following formula:—



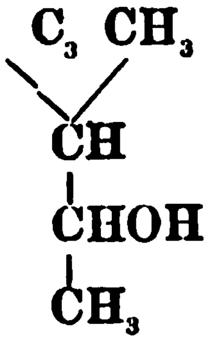
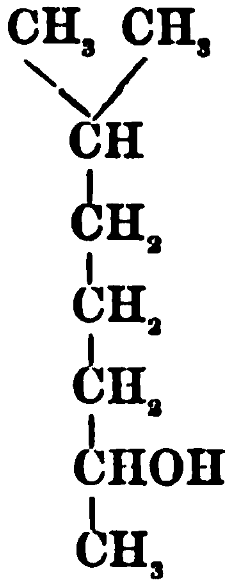
oil is inactive. This physical difference is most probably caused by a different arrangement of the molecules, and not by a different grouping of the atoms in the molecule.

By distilling sebacic acid with caustic baryta, Riche* obtained the hydrocarbon C_8H_{18}, which boiled at 127°. Riche, however, did not obtain this compound in a pure state. I prepared this hydrocarbon in the same way, and found that the distillate is a mixture of different compounds, just as is the case when suberic and azelaic acids are distilled with baryta†. The pure hydrocarbon, isolated from this mixture by means of strong acids and so on, boils at 123°–125°, and has the same specific gravity as that from isotyl alcohol, viz. 0·7083 at 12°·5. These two hydrocarbons appear, therefore, to be identical, and the carbon atoms in sebacic acid must also be arranged in a similar manner as in isoctyl alcohol.

The secondary amyl and octyl alcohols may be considered to be derived from the tertiary butyl alcohol, in a similar manner as the butyl and amyl alcohols formed by fermentation from the secondary propyl alcohol, viz.—

		Boiling-point.	Difference.
Secondary propyl alcohol,		84°	
			24°
Fermentation butyl alcohol,		108°	
			24°
Fermentation amyl alcohol,		132°	
Tertiary butyl alcohol,		82°	
			26°

* Ann. der Chem. und Pharm. vol. cxv. p. 111.
† Dale, Journ. Chem. Soc. New Ser. vol. ii. p. 258.

		Boiling-point.	Difference.
			26°
Secondary amyl alcohol,		108°	
			$3 \times 24°$
Secondary octyl alcohol,		181°	

It is seen that the difference in the boiling-points in both series is about 24° for each increase of CH_2; and further, that the boiling-point of a member of the first series is the same as the boiling-point of that member of the second series which contains CH_2 more.

VI. "Announcement of the intention of the Swedish Government to send out a new Polar Expedition." In a Letter addressed to the President, by Prof. A. E. Nordenskiold. Communicated by the President. Received May 14, 1868.

Stockholm, May 9, 1868.

Dear Sir,—Some days ago I had the pleasure of receiving your kind letter of the 12th February, and the same day I was informed that the sum necessary for a new Polar Expedition was furnished by some private gentlemen of Gottenburg. Since then this undertaking has been embraced with great interest by the Swedish Government and the navy, who have granted the new expedition a strong and excellent screw-steamer, which will be supplied with provisions for a year.

The main object of the expedition will be to penetrate northward from Spitzbergen; but several naturalists will also accompany it.

I hope the new undertaking will be embraced by you with the same interest as the former ones, though the measurement of an arc of meridian does not yet make the aim of our enterprise. But I think that during the expedition I shall be able to amend my omission of measuring the hill south of Fairhaven*, which surely will furnish an excellent datum for determining

* Philosophical Transactions, 1824, p. 290.

the rising of the land in this part of Spitzbergen. I shall also, where the rocks are hard enough for the purpose, bore along the shores as many watermarks as possible, to give in the future sure data for the settling of the same interesting question.

If it were possible to obtain a good pendulum we also would make pendulum observations, at least if the expedition remains in these regions during the winter.

P.S. The expedition will start from Gottenburg July the 15th.

VII. "Further Observations on the Spectra of the Sun, and of some of the Stars and Nebulæ, with an attempt to determine therefrom whether these Bodies are moving towards or from the Earth." By William Huggins, F.R.S. Received April 23, 1868.

(Abstract.)

§ I. *Introduction.*

The author states that at the time of the publication of the "Observations on the Spectra of the Fixed Stars," made jointly by himself and Dr. W. A. Miller, Treas. R.S., they were fully aware that the direct comparisons of the bright lines of terrestrial substances with the dark lines in the spectra of the stars, which they had accomplished, were not only of value for the more immediate purpose for which they had been undertaken, namely, to obtain information of the chemical constitution of the investing atmospheres of the stars, but that they might possibly serve to reveal something of the motions of the stars relatively to our system. If the stars were moving towards or from the earth, their motion, compounded with the earth's motion, would alter to an observer on the earth the refrangibility of the light emitted by them, and consequently the lines of terrestrial substances would no longer coincide in position in the spectrum with the dark lines produced by the absorption of the vapours of the same substances existing in the stars.

The method employed by them would certainly have revealed an alteration of refrangibility as great as that which separates the lines D. They had, therefore, proof that the stars which they had examined, among others Aldebaran, α Orionis, β Pegasi, Sirius, α Lyræ, Capella, Arcturus, Castor, Pollux, were not moving with a velocity which would be indicated by such an amount of alteration of position in a line.

Since, however, a change of refrangibility corresponding to that which separates the components of D would require a velocity of about 196 miles per second, it seemed to them premature to refer to this bearing of their observations. The earth's motion, and that of the few stars of which the parallax has been ascertained, would make it probable that any alteration in position would not exceed a fraction of the change which would have been observed by them.

The author has since, for several years, devoted much time and labour to this investigation, and believes that he has obtained a satisfactory result.

He refers to Doppler, who first suggested that the relative motion of the luminous object and the observer would cause an alteration of the wave-length of the light; and to Ballot, Klinkerfues, Sonnche, Fizeau, and Secchi, who have written on the subject.

The author is permitted to enrich his paper with a statement of the influence of the motions of the heavenly bodies on light, and of some experiments made in an analogous direction, which he received in June 1867 from Mr. J. C. Maxwell, F.R.S.

It is shown that if the light of the star is due to the luminous vapour of sodium or any other element which gives rise to vibrations of definite period, or if the light of the star is absorbed by sodium-vapour, so as to be deficient in vibrations of a definite period, then the light, when it reaches the earth, will have an altered period of vibration, which is to the period of sodium as $V+v$ is to V, when V is the velocity of light and v is the velocity of approach of the star to the earth. Equal velocities of separation or approach give equal changes of wave-length.

§ II. *Description of Apparatus.*

A new spectroscope is described, consisting in part of compound prisms, which gives a dispersive power equal to nearly seven prisms of 60° of dense flint glass. Various methods were employed for the purpose of ensuring perfect accuracy of relative position in the instrument between the star spectrum and the terrestrial spectrum to be compared with it. A new form of apparatus, which appears to be trustworthy in this respect, was contrived. Many of the observations were made with vacuum-tubes or electrodes of metal, placed before the object-glass of the telescope.

§ III. *Observations of Nebulæ.*

The author states that he has examined satisfactorily the general characters of the spectra of about seventy nebulæ. About one-third of these give a spectrum of bright lines; all these spectra may be regarded as modifications of the typical form, consisting of three bright lines, described in his former papers.

Some of these nebulæ have been reexamined with the large spectroscope described in this paper, for the purpose of determining whether any of them were possessed of a motion that could be detected by a change of refrangibility, and whether the coincidence which had been observed of the first and the third line with a line of hydrogen and a line of nitrogen would be found to hold good when subjected to the test of a spreading out of the spectrum three or four times greater than that under which the former observations were made. The spectrum of the Great Nebula in Orion was very carefully examined by several different methods of comparison of its spectrum with the spectra of terrestrial substances.

The coincidence of the lines with those of hydrogen and nitrogen remained apparently perfect with an apparatus in which a difference in wave-length of 0·0460 millionth of a millimetre would have been detected. These results increase greatly the probability that these lines are emitted by nitrogen and hydrogen.

It was found that when the intensity of the spectrum of nitrogen was diminished by removing the induction-spark in nitrogen to a greater distance from the slit, the whole spectrum disappeared with the exception of the double line, which agrees in position with the line in the nebulæ, so that, under these circumstances, the spectrum of nitrogen resembled the monochromatic spectra of some nebulæ. It is obvious that if the spectrum of hydrogen were greatly reduced in intensity, the strong line in the blue, which corresponds to one of the lines of the nebular spectrum, would remain visible after the line in the red and the lines more refrangible than F had become too feeble to affect the eye.

It is a question of much interest whether the few lines of the spectra of these nebulæ represent the whole of the light emitted by these bodies, or whether these lines are the strongest lines only of their spectra which have succeeded in reaching the earth. Since these nebulæ are bodies which have a sensible diameter, and in all probability present a continuous luminous surface, we cannot suppose that any lines have been extinguished by the effect of the distance of the objects from us. If we had reason to believe that the other lines which present themselves in the spectra of nitrogen and hydrogen were quenched on their way to us, we should have to regard their disappearance as an indication of a power of extinction residing in cosmical space, similar to that which was suggested from theoretical considerations by Chéseaux, and was afterwards supported on other grounds by Olbers and the elder Struve.

It is also shown that at the time of the observations this nebula was not receding from us with a velocity greater than 10 miles per second; for this motion, added to the earth's orbital velocity, would have caused a want of coincidence of the lines that could have been observed. If the nebula were approaching our system, its velocity might be as much as 20 or 25 miles per second, for part of its motion of approach would be masked by the effect of the motion of the earth in the contrary direction.

§ IV. *Observations of Stars.*

A detailed description is given of the comparisons of the line in Sirius corresponding to F, with a line of the hydrogen spectrum, and of the various precautions which were taken against error in this difficult and very delicate inquiry. The conclusions arrived at are:—that the substance in Sirius which produces the strong lines in the spectrum of that star is really hydrogen; further, that the aggregate result of the motions of the star and the earth in space, at the time the observations were made,

was to degrade the refrangibility of the dark line in Sirius by an amount of wave-length equal to 0·109 millionth of a millimetre.

If the velocity of light be taken at 185,000 miles per second, and the wave-length of F at 486·50 millionths of a millimetre, the observed alteration in period of the line in Sirius will indicate a motion of recession between the earth and the star of 41·4 miles per second.

At the time of observation, that part of the earth's motion which was in the direction of the visual ray, was equal to a velocity of about 12 miles per second from the star.

There remains unaccounted for a motion of recession from the earth amounting to 29·4 *miles per second, which we appear to be entitled to attribute to Sirius.*

Reference is made to the inequalities in the proper motion of Sirius; and it is stated that at the present time the proper motion in Sirius in declination is less than its average amount by nearly the whole of that part of it which is variable, which circumstance may show that a part of the motion of the star is now in the direction of the visual ray.

Independently of the variable part of its proper motion, the whole of the motion which can be directly observed by us is only that portion of its real motion which is at right angles to the visual ray. Now it is precisely the other portion of it, which we could scarcely hope to learn from ordinary observations, which is revealed to us by prismatic observations. By combining both methods of research, it may be possible to obtain some knowledge of the real motions of the brighter stars and nebulæ.

Observations and comparisons, similar to those on Sirius, have been made on α Canis Minoris, Castor, Betelgeux, Aldebaran, and some other stars. The author reserves the results until these objects have been re-examined. It is but seldom that the atmosphere is favourable for the successful prosecution of this very delicate research.

§ V. *Observations of the Sun.*

The author has observed the sun with three distinct objects in view:—

1. He has sought to discover if the spectrum of the light from the less luminous part of the sun near the limb, differs in any respect from that of the light from the central parts of his disk.

2. He hoped to obtain a view of the red prominences visible during a solar eclipse by reducing the light from our atmosphere by dispersion; for, under these circumstances, if the red prominences give a spectrum of bright lines, these lines would remain but little diminished in brightness, and might become visible.

His observations in these two directions have been hitherto unsuccessful.

3. He proposed to seek to gain from an examination of the spectra of the umbræ and penumbræ of solar spots, some information as to the nature of these phenomena. He has successfully applied the large spectroscope, already described, to the light from the umbra of a spot.

His observations are in accordance generally with those communicated by Mr. Lockyer to the Royal Society.

The author describes the examination of a spot on April 15th, 1868. He shows that about three-fourths of the apparent light of the umbra came from that region of the sun, and the remaining fourth from the intervening illuminated atmosphere of the earth. He observed an increase of width in most of the dark lines of the solar spectrum. The lines C and F, due to hydrogen, did not appear stronger in the spectrum of the umbra. No new lines were detected, nor were any of those of the normal solar spectrum observed to be wanting in the spectrum of the light from the umbra. No bright lines were seen.

Some of the conditions of the solar surface are considered which the phenomena observed may be supposed to indicate.

A cooler state of the heated vapours by which the lines of absorption are produced would diminish the radiation from the gas itself, and so leave more completely uncompensated the absorption by the gas of the light from behind it. Though in this way an apparent increased intensity of the dark lines would result, the observations seem to suggest a state of the vapours connected with tension and temperature in which their power of absorption for each line embraces an increased range of wavelength. Some of the conditions under which this state of things may be brought about are discussed.

The absence of bright lines is not considered as conclusive of the complete absence of light in the umbra from luminous gas; for if there existed in the spot or above it the same vapours in a cooler state, the light would be almost wholly absorbed, and the feebler emanations of the cooler vapour might not do more than render less intense the dark gaps produced by the vapours in the stronger light of all refrangibilities which is evidently present.

What is the source of the light in the umbra which gives the continuous spectrum? May the dense and intensely heated gases, which probably form the inner substance of the sun, emit, in some cases, lines so greatly expanded as to form, when numerous spectra are superposed, a sensibly continuous spectrum? Dr. Balfour Stewart has suggested that, as gases possess a power of *general* absorption of light, a heated mass of gas, if sufficiently dense to be opaque or nearly so, would give a continuous spectrum as well as the spectrum of bright lines peculiar to it.

VIII. "On the Spectrum of Brorsen's Comet, 1868." By William Huggins, F.R.S. Received May 14, 1868.

In January 1866 I communicated to the Royal Society the result of an examination of a small comet visible in the beginning of that year*. I

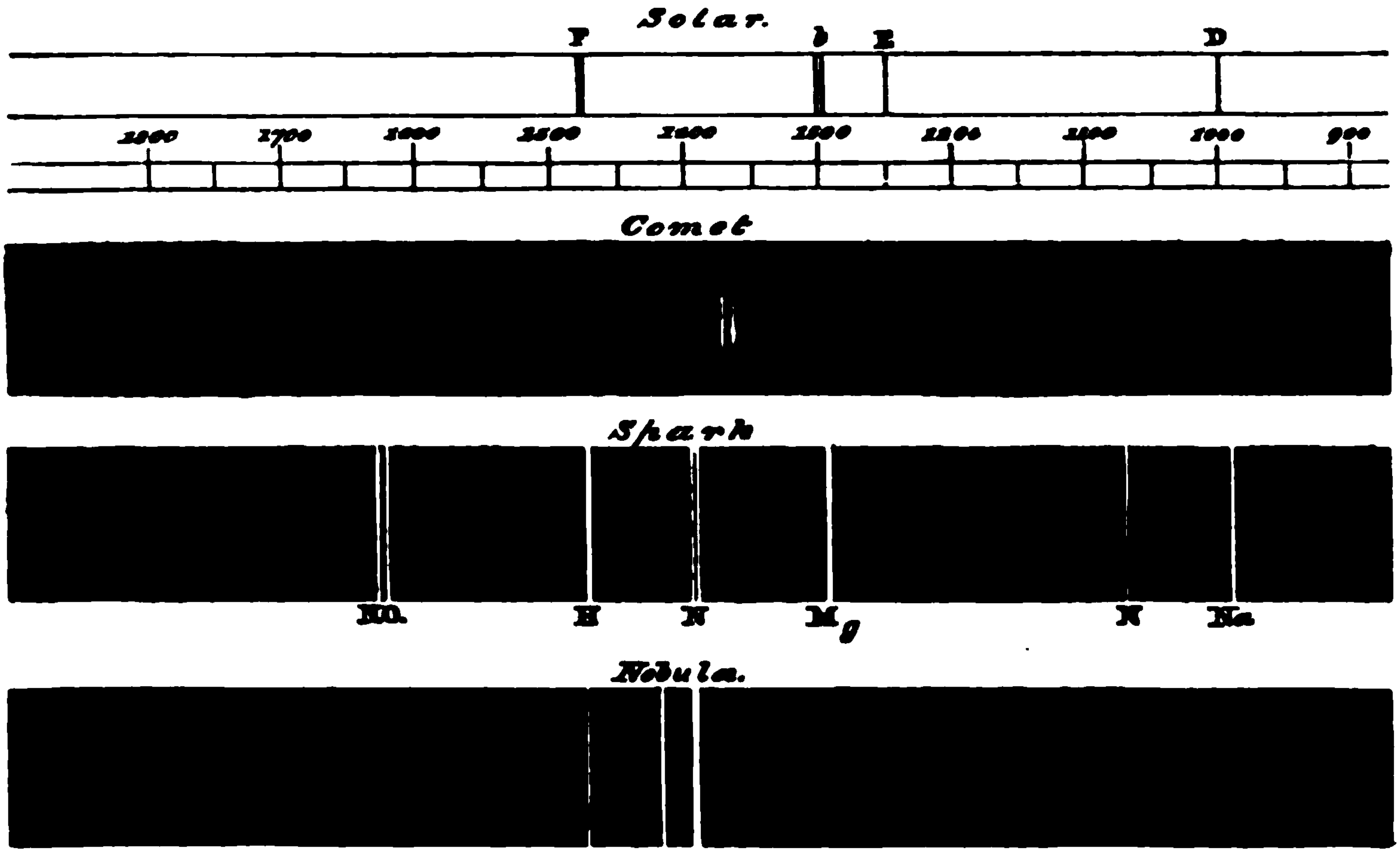
* Proceedings of the Royal Society, vol. xv. p. 5.

examined the spectrum of another small and faint comet in May 1867. The spectra of these objects, as far as their very feeble light permitted them to be observed, appeared to be very similar. In the case of each of these comets, the spectrum of the minute nucleus appeared to consist of a bright line between B and F, about the position of the double line of the spectrum of nitrogen, while the nebulosity surrounding the nucleus and forming the coma gave a spectrum which was apparently continuous.

Unfavourable weather prevented me from obtaining an observation of Brorsen's comet, at its present reappearance, before April 29. Since that evening I have examined it on May 2, 4, 6, 7, 12, 13. As I have not noticed any change in its spectrum during this time, I will put together the results of my observations on different nights, in order to avoid the repetition which would occur if the observations were arranged in the order in which they were made.

I tried various spectroscopes upon this object. The best views of its spectrum were obtained with a spectroscope of the form already described in my former papers*, and furnished with one prism of very dispersive flint glass, with a refracting angle of 60°. Some measures were taken with a similar spectroscope, with two prisms of 60°.

The comet appears in the telescope as a nearly round nebulosity, in which the light increases rapidly towards the centre, where on some occasions I detected, I believe, a small stellar nucleus. Generally this minute nucleus was not to be distinguished from the bright central part of the comet.



I suspected two or three bright points in the coma. May 7, I perceived a small extension of the faint surrounding nebulosity in a direction opposite to the sun, so as to form a short tail.

The spectrum of this comet consists for the most part of three bright

* Philosophical Transactions, 1864, p. 415.

bands. The length of the bands in the instrument shows that they are not due alone to the stellar nucleus, but are produced by the light of the brighter portions of the coma.

I took some pains to learn the precise character of these luminous bands. When the slit was wide they resembled the expanded lines seen in some gases; for example, the line F in the spectrum of hydrogen at the atmospheric pressure. As the slit was made narrow the two fainter bands, namely the one in the yellow and the one in the blue, appeared to fade out without becoming more defined. I was unable to resolve these bands into lines. In this respect they are very different from the bright lines of the nebulæ, which become narrow as the slit is made narrow.

The middle band, which is so much brighter than the others that it may be considered to represent probably three-fourths, or nearly so, of the whole of the light which we receive from the comet, appears to possess similar characters. In this nebulous band, however, I detected occasionally two bright lines, which appeared to be shorter than the band, and may be due to the nucleus itself. This suspicion seems to be strengthened by the circumstance that when by moving the telescope the image of the comet was made to pass before the slit, these brighter lines were only observed when the middle of the comet was upon the slit, while the nebulous band continued as long as any part of the comet, except its extreme margin, was upon the slit.

Besides these three bright bands there was a very faint continuous spectrum. This spectrum is omitted in the diagram, as it could scarcely be represented without making it appear too strong relatively to the bright bands.

The position in the spectrum of the bands was determined by micrometrical measures, and also by simultaneous comparison, of the bands with the bright lines of magnesium, sodium, hydrogen, and nitrogen. The brightest band, which is in the green part of the spectrum, is nearly in the position of the brightest line of the nebulæ, which coincides with the double line of the spectrum of nitrogen; but, as the diagram shows, the band in the comet is in a small degree less refrangible than the line of nitrogen. This difference of refrangibility cannot be attributed to the comet's motion, since at the time the observations were made the comet was approaching the earth.

The band in the blue is considerably more refrangible than F, and is nearly as refrangible as the group of bright lines in the air-spectrum, which have the numbers 2642, 2669 in the map and tables of my paper "On the Spectra of the Chemical Elements" *.

The least refrangible of the bands occurs in the yellow part of the spectrum, at about the distance from E of one-third of the interval which separates E from D.

The spectrum of this comet resembles the diagram given by Donati of

* Astronomische Nachrichten, No. 1488.

the spectrum of Comet I., 1864*. The positions of the three bands seen by him appear to agree with those which the bright bands of this comet occupy.

This comet differs remarkably from the two small comets which I examined in the much smaller relative proportion of the light which forms a continuous spectrum. In Brorsen's comet, as it now appears, the bright middle part of the nebulosity seems to have a constitution analogous at least to that of the nucleus, and to be self-luminous; in the other comets the coma, which surrounded a distinctly marked nucleus, gave a continuous spectrum. The three comets resemble each other in the circumstance that the light of the bright central part was emitted by the cometary matter, while the surrounding nebulosity reflected solar light.

The telescopic observations of the heads of Donati's comet and of other large comets have shown that the luminous material is not at once driven off into the outer portions of the coma and the tail, but usually forms in front of the nucleus a dense luminous cloud, which for a time seems to be identical in the character of its light with that of the nucleus. It is, I believe, the outer portions only of the coma, which are frequently separated by dark spaces from the nucleus, and the tail, which the polariscope has shown to shine by reflected light.

The positions of the bands in this comet would seem to indicate a chemical constitution different from that of the nebulæ, which give a spectrum of bright lines. It will be seen in the diagram that, though the brightest of the bands in the spectrum of the comet differs but little in position from the brightest line of the nebulæ, the other bands are found in parts of the spectrum widely removed from those in which the other lines of the nebulæ occur. The suggestion presents itself whether the broad, nebulous bands may not indicate conditions of temperature and molecular state different from those which occur in the gaseous nebulæ. Plücker has shown that nitrogen and some other substances give totally different spectra, under different conditions of temperature and tension. The spectrum of this comet, however, does not resemble the other spectrum of nitrogen, which Plücker distinguishes as the spectrum of the first order†.

IX. "Memoir on 'Undevelopable Uniquadric Homographics.'" By MARTIN GARDINER, C.E. Communicated by the Rev. R. TOWNSEND, F.R.S. Received April 13, 1868.

(Abstract.)

In this paper the author's method of investigation is purely geometrical throughout, its arrangement of details is systematic and natural, and it is divided into eight chapters, the first seven of which are preparatory to the consideration of the interesting problem discussed at length in the eighth,

* Philosophical Transactions, 1864, p. 158.

† Ibid. 1865, p. 9.

the direct, general, and complete solution of which is claimed to be given in it for the first time.

Chapter I., after some preliminary general properties, resulting immediately from the known properties of homographic systems of points on plane conics, treats more particularly of the simplest case of such systems on undevelopable quadrics, viz. of the case of systems in perspective, the several pairs of whose corresponding constituents possess manifestly the property of interchangeability; shows that systems having three double points not in the same tangent plane to the quadric on which they lie are necessarily of that class, except only when they have a fourth double point not in the plane of the other three, in which case they altogether coincide; and gives simple instances in which the problem, whose solution is the principal object of the memoir, is manifestly either "wholly or partially porismatic," as he terms it.

Chapter II. treats of systems whose several pairs of corresponding points are interchangeable but which are not in perspective; shows that their several chords of connexion intersect the same two reciprocal lines with respect to the quadric on which they lie; shows the relation between either system and the perspective of the other to any point on either of those lines, also the relation between either of two systems in perspective and the perspective of either to any point conjugate to their centre of perspective with respect to the surface; and shows how to construct the two reciprocal lines from two pairs of corresponding points of the systems.

Chapter III. treats of systems whose several pairs of corresponding points connect through a single common line, which have therefore an infinite number of double planes passing through that line; shows that their several chords of connexion, besides intersecting the line, all touch a second quadric having double contact with the original, both at its two points of intersection with the line, and also at its two points of intersection with the reciprocal line; proves a property of the cone enveloping either surface from any vertex taken arbitrarily on either line; shows the relation between either system and the perspective of the other to any point on either line; and shows how to construct the two reciprocal lines from two pairs of corresponding points of the systems.

Chapter IV. treats of systems having two of their four double planes non-tangential to the quadric on which they lie; shows that the chords connecting their several pairs of corresponding points touch two cones enveloping the quadric along two planes colinear with and harmonically conjugate to each other with respect to the two non-tangential double planes; proves that those touching along a plane section of either cone generate a skew quadric; shows that the two homographic systems determined by the two correspondents in the two systems of a variable point on the quadric are of the class considered in the preceding chapter, having an infinite number of double planes passing through the line of intersection of the two non-tangential double planes of the original systems; and shows the

relation between either system and the perspective of the other to any point on that line of intersection.

Chapter V. treats of systems having their four double planes all tangential to the quadric on which they lie, shows that the chords connecting their several pairs of corresponding points touch two other quadrics having quadruple contact with the original at the four double points of the systems, and gives various constructions for the determination of the four double points when the law connecting the several pairs of corresponding points of the systems is given or known.

Chapter VI. gives various criteria for determining in certain cases to which of the preceding classes two homographic systems belong, where, as in the problem whose solution forms the principal object of the memoir, the law connecting the same pairs of corresponding points of the systems is given or known.

Chapter VII. contains numerous theorems, several of much interest and originality, respecting open and closed polygons inscribed in undevelopable quadrics, whose sides pass in the same order of sequence through a common system of points in space, all deduced from the principles established in the preceding chapters, and several having direct reference to the interesting problem to be considered in the next and closing chapter.

Chapter VIII.—Given an undevelopable quadric and n fixed points in to find the space; first extremities of inscribable closed n'gons, or the locus of the first extremities when the inscription of the closed n'gons is porismatic.

When the number n *of given points is* odd.

Assume any three points a_1, b_1, c_1 in the surface, no two of which are on one generator, as first extremities, and proceed to inscribe $2n$'gons.

(1) If the three points be found to be first extremities of closed n'gons, then will the trace of their plane be the locus of first extremities of closed n'gons, the problem in such case being partially porismatic.

(2) If the points are first extremities of closed $2n$'gons, or if two of them be first extremities of closed $2n$'gons and the third one a first extremity of a closed n'gon, or if one of the points be the first extremity of a closed $2n$'gon, and the other two points first extremities of closed n'gons, then the line or lines forming the closing chords of the open n'gons composing the $2n$'gon or $2n$'gons (as may be) and the tangent plane or planes at the first extremity or extremities of the closed n'gon or n'gons (as may be) meet in one point ρ, the trace of whose polar plane R is the locus of first extremities of inscribable closed n'gons, the problem in such case being partially porismatic.

(3) If two of the points be first extremities of closed n'gons and the third point the first extremity of an open $2n$'gon, then the problem is non-porismatic, and the two closed n'gons are the only inscribable closed n'gons. Moreover the reciprocal of the line joining the first extremities of the two closed n'gons will pierce the quadric in points which are the first extremi-

ties of inscribable closed $2n$'gons (real or imaginary according as the quadric S is ruled or convex).

(4) If one of the three chosen points be the first extremity of an open $2n$'gon (no matter as to the other two points) the problem is non-porismatic, and we can find the first extremities of the closed n'gons by either of the four following methods:—

First method.—Continue the $2n$'gon until a $4n$'gon be formed, and draw the plane P which contains the extremities of this $4n$'gon and the point of junction of the two open $2n$'gons composing it. Assume another point in the surface, not in the trace of the plane P, and, making it a first extremity, inscribe another $4n$'gon; and through the extremities of this $4n$'gon and the point of junction of the two open $2n$'gons composing it draw a plane Q. Then with the line xx of intersection of the planes P and Q pierce the quadric in the only points (real or imaginary as may be) which are first extremities of closed n'gons; and the line ii reciprocal to xx will pierce the quadric in points (real or imaginary as may be) which are first extremities of closed $2n$'gons.

Second method.—By the additional inscription of another open n'gon convert the open $2n$'gon into an open $3n$'gon, and put A, B, C to represent the three open n'gons composing the open $3n$'gon. Find the point of puncture of the line through the first extremity of A and the final extremity of B with the tangent-plane at the junction of A and B; find the point of puncture of the line through the first extremity of B and the final extremity of C with the tangent-plane at the junction of B and C. Then will the line xx through the two points of puncture pierce the quadric in two points (real or imaginary as may be) which are the first extremities of the only inscribable closed n'gons; and the line ii, which is reciprocal to xx, will pierce in first extremities of closed $2n$'gons.

N.B. When S is a hyperboloid of one sheet, and that the first extremities of the closed n'gons are *real*, then the first extremities of the closed $2n$'gons are also *real*; and it is evident there are two pairs of generators which are corresponding interchangeable lines in the homographic figures in which the extremities of the inscribable n'gons are pairs of corresponding points. It is moreover evident that when all the corresponding points of such figures are not interchangeable, these are the only pairs of interchangeable generators; and we must not assume the first extremities of the $4n$'gons or $3n$'gons in these lines.

Third method.—Let $o_1, o_2, o_3, \ldots o_n$ be the n fixed points through which the sides must pass in order.

Assume the constant homological ratio -1 for homological systems, and making o_1 vertex and its polar plane axis, find the point α_1 homological to the centre α_0 of the quadric; assume o_2 as vertex and its polar plane as axis, and find the point α_2 homological to α_1; and proceed thus directly in order through the n points until arrived at the point α_n. Assume o_n as vertex and its polar plane as axis, and find the point α_{-1} homological to the

centre α_0 of the quadric; assume o_{n-1} as vertex and its polar plane as axis, and find the point α_{-2} homological to α_{-1}; and proceed thus in reverse order through the rest of the n given points until arrived at the point α_{-n} (the points α_n, α_{-n} will be distinct). Draw the diametral plane A_0 which bisects the chords parallel to the line $\alpha_n \alpha_{-n}$ (it will also bisect $\alpha_n \alpha_{-n}$); assume any two points in the trace of this plane as first extremities, and inscribe two n'gons in the quadric; through the point α_n and the final extremities of these n'gons draw the plane A_n; find the line of intersection ii of the planes A_0, A_n, and its reciprocal xx. Then will xx always pierce in the two points (real or imaginary as may be) which are the first extremities of the only inscribable closed n'gons; and the line ii will pierce in first extremities of closed $2n$'gons.

Fourth method.—Find the points α_{-n} and α_n as in last method; assume any point a_0 in the surface as first extremity, and inscribe a n'gon whose last extremity we may represent by a_n; draw the plane D_1 which contains the line $\alpha_{-n}\alpha_0$ and the point a_0; draw the plane D_2 which contains the $\alpha_n \alpha_0$ and the point a_n; in the lines $\alpha_0 \alpha_{-n}$, $\alpha_0 \alpha_n$ find the points m_1, m_2 such that

$$\frac{\alpha_{-n} m_1}{\alpha_0 m_1} = \frac{\alpha_n m_2}{\alpha_0 m_2} = +\sqrt{\frac{\alpha_{-n} D_1 \,.\, \alpha_n D_2}{\alpha_0 D_1 \,.\, \alpha_0 D_2}};$$

and in the same lines find the points h_1, h_2 such that

$$\frac{\alpha_{-n} h_1}{\alpha_0 h_1} = \frac{\alpha_n h_2}{\alpha_0 h_2} = -\sqrt{\frac{\alpha_{-n} D_1 \,.\, \alpha_n D_2}{\alpha_0 D_1 \,.\, \alpha_0 D_2}}.$$

Put $\Sigma_{\prime}$, $\Sigma_{\prime\prime}$ to represent the homographic figures in which the first and final extremities of inscribable n'gons are corresponding points. Regard m_2 and h_2 as points in $\Sigma_{\prime}$, and find their correspondents m_3, h_3 in $\Sigma_{\prime\prime}$; draw the planes $m_1 m_2 m_3$, $h_1 h_2 h_3$, and find their line of intersection xx and its reciprocal ii. Then will the line xx pierce the quadric in the only points (real or imaginary as may be) which are first extremities of inscribable closed n'gons, and the line ii will pierce in first extremities of closed $2n$'gons.

Moreover the planes $m_1 m_2 m_3$, $h_1 h_2 h_3$ are the only two double planes of the figures $\Sigma_{\prime}$, $\Sigma_{\prime\prime}$ which are non-tangential to the quadric.

N.B. When the points α_{-n}, α_n are coincident, the inscription of the closed n'gons is partially porismatic, and one of the two points which divide $\alpha_{-n}\alpha_n$ and the diameter coincident with it harmonically is the point of concurrence of the closing chords of all inscribable open n'gons, and the polar plane of which passes through the other, &c.

When the centre of the quadric is a double point, then according as all the closing chords are parallels or pass through the centre, so will the locus of the first extremities of the closed n'gons be the trace of a diametral plane or of a plane at infinity.

When the number n *of given points is* even.

Assume three points a_1, b_1, c_1 on the surface, no two of which are on

the same generator; and, making them first extremities, proceed to inscribe $2n$'gons.

(1) If these assumed points be found to be first extremities of closed n'gons, the problem is fully porismatic, and every point in the surface will be the first extremity of an inscribable closed n'gon.

(2) If two of the points be found to be first extremities of closed n'gons and the other not, then the line xx through these two points, and the line ii reciprocal to xx, pierce the quadric in four points (the punctures made by ii being real or imaginary as may be) which constitute the first extremities of all the inscribable closed n'gons.

(3) If one of the points be the first extremity of a closed n'gon, and another of them be first extremity of a closed $2n$'gon. Draw the closing chord of the two open n'gons composing the closed $2n$'gon to pierce the tangent-plane at the first extremity of the closed n'gon in the point ρ; in the closing chord find the point μ which is conjugate to the point of puncture ρ; through μ and the first extremity of the closed n'gon draw the line xx, and find the line ii reciprocal to xx. Then will the lines xx and ii pierce the quadric in the four points which constitute the first extremities of all the inscribable closed n'gons.

(4) If two of the assumed points be found to be first extremities of closed $2n$'gons, we may find the first extremities of the inscribable closed n'gons by either of the four following methods:—

First method.—Draw the two closing chords of the open n'gons composing the $2n$'gons; and, if these chords intersect in a point ρ, draw the line xx which is polar of ρ in respect to the trace of the plane of the two chords; find the line ii reciprocal to xx. Then will xx and ii pierce the quadric in the first extremities of the closed n'gons. But if the chords do not intersect, draw tangent-planes at their extremities; find the two pairs of points (one pair in each chord) which divide these closing chords and the segments intercepted by the tangent-planes harmonically; draw the line xx through the two of these points which divide the chords internally; draw the line ii through the two points which divide the chords externally. Then will xx and ii be reciprocal lines piercing the quadric in four points which are first extremities of the inscribable closed n'gons.

Second method.—Find a_1a_2, b_1b_2, c_1c_2 the closing chords of three open n'gons. If any two of these intersect, proceed as in the last method; but if not, proceed as follows:—In the chord a_1a_2 find the point m which corresponds to infinity (on the same line) in one of the homographic figures in which the extremities of all inscribable n'gons are corresponding interchangeable points; in the same chord a_1a_2 find the two points x and i such that $mx = mi = \sqrt{ma_1 \,.\, ma_2}$; through x draw the line xx which cuts the two non-planar chords b_1b_2, c_1c_2; through i draw the line ii which cuts the same two non-planar chords b_1b_2, c_1c_2. Then will xx and ii be reciprocal lines piercing the quadric in the four points which are the first extremities of closed n'gons.

Third method.—Draw a_1a_2, b_1b_2 the closing chords of the open n'gons composing the closed $2n$'gons. If these two lines intersect in a point ρ, draw the line xx through the points in a_1a_2, b_1b_2 which are harmonic conjugates to ρ in respect to the segments a_1a_2, b_1b_2; find the line ii reciprocal to xx. Then will xx and ii pierce the quadric in the four points which are first extremities of closed n'gons. But if a_1a_2, b_1b_2 do not intersect, then find the final extremity c_2 of the n'gon having the point c_1 as first extremity; and if the line c_1c_2 cuts either a_1a_2 or b_1b_2, the lines xx and ii can be found in the manner just indicated. If c_1c_2 do not cut either of the lines a_1a_2, b_1b_2, then find the points α_1, α_2 in which a_1a_2 pierces the planes $b_1b_2c_1$, $b_1b_2c_2$; find the points β_1, β_2 in which b_1b_2 pierces the planes $a_1a_2c_1$, $a_1a_2c_2$; find the points h_1, h_2 which divide the segments a_1a_2, $\alpha_1\alpha_2$ harmonically; find the points k_1, k_2 which divide the segments b_1b_2, $\beta_1\beta_2$ harmonically; through the two points h_1, k_1, cutting a_1a_2, b_1b_2 internally, draw the line xx; through the two points h_2, k_2, cutting a_1a_2, b_1b_2 externally, draw the line ii. Then will xx and ii be reciprocal lines piercing the quadric in the four points which are first extremities of closed n'gons.

Fourth method.—Put $\Sigma_{,}$ and $\Sigma_{,,}$ to represent the homographic figures in which the first and final extremities of all inscribable n'gons are corresponding points; find the point α_1 which corresponds in either of the figures Σ, $\Sigma_{,}$ to the centre α_0 of the quadric regarded as belonging to the other figure; find the diameter d_1d_2 which contains the points α_0, α_1; find the points p, q which divide the segments d_1d_2 and $\alpha_0\alpha_1$ harmonically; draw the plane P which is polar to the point p which lies outside the quadric; find the point a_2 which is final extremity of a n'gon whose first extremity is in the trace of the plane P; draw xx the diameter of the trace of P which bisects a_1a_2 (the point a_2 will be in the trace of P); find the line ii reciprocal to xx. Then will xx and ii pierce in first extremities of the four inscribable closed n'gons. But if the centre α_0 of the quadric be a double point of the figures $\Sigma_{,}$, $\Sigma_{,,}$, proceed as follows:—Inscribe any n'gon in the quadric, and draw the diameter xx which bisects its closing chord. Then will the diameter xx and its reciprocal at infinity pierce the quadric in the four points which are first extremities of closed n'gons.

(5) When we can inscribe an open $2n$'gon the problem is always nonporismatic, and we can find the lines xx, ii which pierce the quadric in first extremities of the four closed n'gons by either of the four following methods:—

First method.—Put $\Sigma_{,}$ and $\Sigma_{,,}$ to represent the homographic figures in which the first and final extremities of all inscribable n'gons are corresponding points. In the figures $\Sigma_{,}$ and $\Sigma_{,,}$ find the points o_1 and o_2 which are the correspondents of the centre o of the quadric regarded as belonging to the figures $\Sigma_{,}$ and $\Sigma_{,,}$; draw the diameter ror which bisects o_1o_2; find the line $r_1o_1r_1$ in $\Sigma_{,}$ which corresponds to ror in $\Sigma_{,,}$; bind the line $r'r'$ reciprocal to r_1r_1; through the centre o draw the diametral plane K which bisects all chords parallel to ror; find the points ρ_1, ρ' in which the reciprocal lines r_1r_1,

$r'r'$ pierce the plane K; draw any plane P parallel to K, and through t[he] points μ_1, μ' in which it cuts r_1r_1 and $r'r'$ draw lines parallel to the diamet[er] rr to pierce the plane K in points ϕ_1, ϕ'; through the centre o draw (s[ee] 'Section of Ratio' of Apollonius) the two lines ox_1x', oi_1i' so that

$$\rho_1x_1 : \rho'x' :: \rho_1i_1 : \rho'i' :: \rho_1\phi_1, \rho'\phi';$$

through x_1 and x' draw lines parallel to rr to cut r_1r_1 and $r'r'$ in points and x; through i_1 and i' draw lines parallel to rr to cut r_1r_1 and $r'r'$ in and i. Then will the lines xx and ii be reciprocals piercing the quadric i[n] the four points which are first extremities of the inscribable closed n'gons.

Second method.—In the closing chord c_1c_2 of any inscribed open n'go[n] assume any point p, and for the moment regard it as $n+1$th point of series having the n fixed points as first n points; choose four points b_1, d e_1, f_1 in the surface so that no two of the five c_1, b_1, d_1, e_1, f_1 lie on one ge[-] nerator; find the final points b_2, d_2, e_2, f_2 of inscribed $(n+1)$'gons who[se] sides pass in order through the $n+1$ points, and which have b_1, d_1, e_1, f_1 first extremities. Then (representing tangent-planes at points by capit[al] letters of like names, and subscript numbers as the small ones representin[g] the points of contact) in the chord d_1d_2 find the point d_3 such that

$$\frac{d_1d_3}{p_2d_3} = \frac{d_1B_1}{d_2B_2} : \frac{c_1B_1}{c_1B_2};$$

in the chord e_1e_2 find the point e_3 such that

$$\frac{e_1e_3}{e_2e_3} = \frac{e_1B_1}{e_2B_2} : \frac{c_1B_1}{c_1B_2};$$

in the chord f_1f_2 find the point f_3 such that

$$\frac{f_1f_3}{f_2f_3} = \frac{f_1B_1}{f_2B_2} : \frac{c_1B_1}{c_1B_2}$$

(the points d_3, e_3, f_3 must be so determined as that a *real* tangent-plane ca[n] pass through either of them); draw the plane $d_3e_3f_3$ and it will touch th[e] quadric in a point a_1; draw the line c_1a_1, and find the point q in it which i[s] conjugate to p. Now if we regard the n given fixed points and the point as the $n+1$ points of a series, every point in the surface will be the firs[t] extremity of a closed $2(n+1)$'gon. Find (by last case) the two reciproca[l] lines yy, zz which pierce the quadric in first extremities of close[d] $(n+1)$'gons whose sides pass in order through these $n+1$ points; find th[e] line $p'q'$ reciprocal to pq; draw the lines xx, ii, each of which cuts the fou[r] non-planar lines yy zz, pq, $p'q'$. Then will xx and ii be reciprocal line[s] piercing in four points which are the first extremities of the inscribable closed n'gons.

Third method.—On the closing chord c_1c_2 of any inscribed open n'gon assume any point p, and regard it for the moment as the $n+1$th point of a series to which the n fixed points belong. Assume three points b_1, d_1, e_1 in the surface so that no two of the points c_1, b_1, d_1, e_1 are on one generator, and find the final extremities b_2, d_2, e_2 of inscribed n'gons having b_1, d_1, e_1 as first extremities; draw the tangent-plane C_1 at the point c_1; put D_1, D_2,

E_1, E_2 to represent the four planes $d_1b_1c_1$, $d_2b_2c_1$, $e_1b_1c_1$, $e_2b_2c_1$ respectively; through the line of intersection of the planes D_1, D_2 draw the plane P the distances of any point in which from D_1 and D_2 have to each other the ratio of $\frac{b_1D_1}{b_2D_2}$ to $\frac{b_1C_1}{b_2C_1}$; through the line of intersection of the planes E_1, E_2 draw the plane Q which is such that the ratio of the distances of any point in it from E_1 and E_2 is the same as that of $\frac{b_1E_1}{b_2E_2}$ to $\frac{b_1C_1}{b_1C_1}$; find the line of intersection *mm* of the planes P, Q (this line *mm* will be a tangent to the quadric), and the point a_1 in which it touches the quadric; in the line c_1a_1 find the point q conjugate to p. Then if we regard the n given fixed points and the point q as $n+1$ points of a series, any point in the surface will be first extremity of a closed $2(n+1)$'gon. Find, by the preceding case, the two reciprocal lines *yy*, *zz* which pierce the quadric in first extremities of closed $(n+1)$'gons whose sides pass in order through these $n+1$ points; find the line $p'q'$ reciprocal to pq; draw the lines *xx* and *ii* each of which cuts the four non-planar lines *yy*, *zz*, pq, $p'q'$. Then will *xx* and *ii* be reciprocals piercing in first extremities of the four inscribable closed n'gons.

Fourth method.—The following method is applicable in all cases in which n is even. Omit temporarily the nth point o_n of the given n points, and find the line U which pierces the quadric in the two first extremities of inscribable closed $(n-1)$'gons whose sides pass in order through the $n-1$ points; find the point q in the line U which is conjugate to the omitted nth point o_n. Then if we regard the $n-1$ given points and the point q as forming the n points of a new series, any point in the surface will be the first extremity of a closed $2n$'gon. Find the two reciprocal lines *yy*, *zz* which pierce the quadric in first extremities of closed n'gons whose sides pass in order through the new series of n points; find the line $p'q'$ reciprocal to o_nq; draw the lines *xx*, *ii* each of which cuts the four non-planar lines *yy*, *zz*, o_nq, $p'q'$. Then will the lines *xx* and *ii* be reciprocals piercing the quadric in first extremities of the four inscribable closed n'gons whose sides pass in order through the n given points. But if the inscription of the closed $(n-1)$'gons be partially porismatic, and ρ the point of concurrence of the closing chords of the inscribable open $(n-1)$'gons, then will the line *xx* through o_n and ρ, and the line *ii* reciprocal to *xx* pierce the quadric in the first extremities of the inscribable closed n'gons.

N.B. And if o_n be in such case coincident with ρ, then the problem is fully porismatic, and every point in the surface is the first extremity of a closed n'gon.

N.B. We may also observe that when the inscription of the closed $(n-1)$'gons is non-porismatic, and that the point o_n is situated in the line U, then, by conceiving q coincident with o_n the lines *yy*, *zz* will be identical with *xx* and *ii*.

I may observe that the general problem can be completely solved by

"methods of reduction," amongst which the following is perhaps the mos obvious and simple:—

Let S be the quadric, and o_1 o_2, o_3, o_4, ... o_n the n given points. Put x for the line through o_1 and o_2. Instead of o_2 and o_1 we can substitute th point p_2 in which the line xx is cut by the plane $o_3o_4o_5$, and another poin p_1 determinable in the same line xx. Then instead of the four planar point p_2, o_3, o_4, o_5 we can (see theorem 38) substitute two other points p_3, p_4 i the same plane; and therefore instead of the series of n points, we can sub stitute the series of $n-2$ points p_1, p_3, p_4, o_6, o_7, ... o_n and the inscribabl $(n-2)$'gons, closed and open as may be, whose sides pass in order throug these points will have extremities identical with the extremities of inscri bable n'gons. And thus step by step we can reduce the number of sides until at length we find three points or four points, according as n is odd o even, such that the extremities of all inscribed 3'gons or 4'gons whose side pass in order through such points are identical with extremities of inscri bable n'gons whose sides pass through the original n points; and therefor to solve the problem all we have to do is to inscribe the closed 3'gons o closed 4'gons as may be.

And in respect to this method we may observe,—

(1) If any four consecutive points of any of the series be colinear an such as to render the inscription of closed 4'gons *real,* we may omit suc points altogether from the series.

(2) When n is *odd,* and that we reduce the problem to the inscriptio of closed 3'gons whose sides pass through three known points, then shoul such points be colinear or form a conjugate triad, the problem will be par tially porismatic.

(3) In the case in which n is *odd,* it is easy to perceive how the pro blem can be reduced to the drawing of a line through a known point to cu two reciprocal lines (which *point* will be on *one* of the lines when the pro blem is partially porismatic). And when n is *even,* it is easy to see ho the problem can be reduced to the drawing of the two lines which cut tw pair of (determinable) reciprocal lines.

(4) The following method of finding the line in the plane of four point which pierces in first extremities of closed 4'gons is obvious:—Let o_1, o_2 o_3, o_4 be the four planar points.

Find p the point of intersection of the lines o_1o_2, o_3o_4; in the line o_1o find the point m such that o_1o_2, mp, and the pair of (real or imaginary points in which o_1o_2 pierces the quadric, will form an involution; in the lin o o_4 find the point n such that the pairs of points o_3o_4, pn, and the points i which o_3o_4 pieces the quadric, form an involution. Then will the line m pierce in first extremities of closed 4'gons.

May 28, 1868.

Lieut.-General SABINE, President, in the Chair.

The following communications were read :—

I. "A Comparison of the Kew and Lisbon Magnetic Curves during the Magnetic Storm of February 20–25, 1866." By Senhor I. Brito Capello, of the Lisbon Observatory. Communicated by B. Stewart. Received April 18, 1868.

During the 20th, 21st, 23rd, 24th, and 25th of February 1866, large magnetic disturbances were recorded by the magnetographs at the Lisbon and Kew Observatories.

As these indicate several appreciable deviations from the normal types, I trust a description of them may be not without interest to the Royal Society. Dr. Stewart, Director of the Kew Observatory, has kindly sent me copies of the Kew magnetic curves during these disturbances.

In order thoroughly to ascertain the laws governing the forces which disturb the ordinary magnetic condition of the globe, we should reduce in a systematic manner, such as General Sabine has so ably pursued, the observations made at a number of stations, and then classify and discuss the valuable results so obtained.

Nevertheless the present communication relative to the disturbances observed at two stations offers some interest, on account of the apparent variability of the forces which are in action during the same disturbance, and also the apparently variable relations between these forces at Lisbon and the same forces at Kew.

In a former comparison made between the magnetic curves of Kew and Lisbon (Proceedings of the Royal Society, No. 60), it was established that at Lisbon, during disturbances, the vertical force and the declination curves were *invariably* opposed to each other, *i. e.* a *concave* wave of one of the curves always corresponded with a similar *convex* one in the other; or, in other words, an augmentation of the vertical force agreed with an horizontal movement of the north pole of the bar towards the east, and a diminution of the same component to a movement of the north pole to the west.

This general law applied both to the large and slow movements (waves), and the short and rapid ones (peaks and hollows).

However, there were some very rare instances in which this law did not altogether hold good. In these cases, although the peaks and waves were reproduced in the two curves in inverted order, yet the whole of the one curve for some period did not assume the inverted form of the other curve.

The periods of disturbance which are the objects of this discussion belong to these *abnormal* types.

In the paper quoted above, the authors have also shown,—

1st. That all the small peaks in the Kew curves are simultaneously reproduced in the Lisbon curves in the same sense in the declination and bifilar, but in a contrary direction in the vertical force.

2nd. That, generally, all the waves of the declination and bifilar at Kew were reproduced in the corresponding Lisbon curves, sometimes more or less disfigured.

3rd. That in the generality of cases, with the exception of peaks and hollows, in which they are opposed, the vertical-force curves of the two stations do not resemble each other.

Let us now see if these laws are confirmed in these disturbances.

First Disturbance.—This series of disturbances commenced at Lisbon, February 20th 16^h 12^m G. M. T., by a sudden increase of the declination, and an enormous diminution of the vertical force. The horizontal force likewise decreased rapidly, but 13^m later (16^h 25^m): this is noteworthy. However, the three elements, and especially the horizontal force, had been somewhat disturbed since 10^h 27^m G. M. T.

It is noticeable, in the large diminution of vertical force, that although the curve in descending ran off the edge of the paper, we can fix the point of minimum (approximately), which gives us a little more than 0·1 (English units) as the value of the disturbance. The increase in declination was nearly a degree (59′·3), and the diminution of horizontal force 0·052 English units.

This last diminution commenced, as we have before stated, 13^m later than the other disturbances; and the time of minimum is also 6^m to 8^m after the corresponding points of declination and vertical force.

In other respects the remaining waves of the horizontal force do not agree with those of the declination and vertical force.

The Kew curves agree tolerably well with the Lisbon curves, up to the time of the large and rapid movements. Here it appears that the large movements of the three instruments were of the same nature as the Lisbon ones; and it is very possible that the large variations were more considerable and rapid, since they failed to record their traces on the paper.

These large movements seem to have begun in the three instruments at Kew at the same time (16^h 30^m), at which time the Lisbon declination and vertical force had deviated to half their full extent.

The small peaks are reproduced at the two stations at the same absolute time, the two vertical forces being always in opposite directions.

The first *period* of this disturbance seems, therefore, to be of the same nature in the two stations, *i. e.* the disturbing forces have acted on the three instruments in a similar manner.

Second Disturbance.—Let us now pass to the second period.

The large movements have ceased, but the horizontal force remains too low, and in continual vibration. Towards 2^h and 3^h of the 21st of February

we may take as the recommencement of a second period, which terminated about 7 P.M.

In the Lisbon declination we see at the beginning and end of this period two similar undulations, each about half an hour in duration, and almost regular. In the middle there are smaller waves, interrupted by peaks and hollows. The diminution of declination is not remarkable.

In the vertical force we have the same *waves* in a contrary direction to the declination; but the ascending branch is in one instance greater than the corresponding descent in the declination. A similar difference may be noticed in the last wave but one, by which it happens that the vertical-force curve shows an increase of force during two hours and a half, without a corresponding decrease of the declination.

The horizontal force retained its position below the mean until $3^h\ 20^m$, when it descended further till 4^h; then it ascended successively till 6^h, where it stopped very nearly in its normal position.

We will now discuss the Kew curves. The declination has been greatly disturbed by large deviations above and below its normal position, a general decrease of declination, however, taking place during more than three hours of great disturbance.

The needle has oscillated 40′ in arc, while the Lisbon oscillations have not exceeded 8′. Some movements seem to agree with Lisbon ones, others, on the contrary, disagree entirely; and even in those movements which correspond, some differences of time are appreciable, which cannot be due to error in the time-scale.

The vertical force at Kew increases rapidly from $2^h\ 35^m$ to $3^h\ 50^m$; a period of fluctuations then follows up to 5^h, after which the curve rapidly descends irregularly to the end.

The Kew vertical-force curve only agrees with the Lisbon curve in the general aspect of the disturbances, the period of greatest increase lasting but $1^h\ 15^m$, while at Lisbon it was $2^h\ 10^m$.

It should be also remarked that the vertical force does not agree in the slightest with the horizontal force at Kew.

The horizontal force at Kew seems to follow the inverse direction of that of Lisbon, and its general form resembles that of Lisbon inverted. After $5^h\ 30^m$ the waves appear to agree.

Thus we see, in the same disturbance, two periods at an interval of some hours, which show their relations at the two stations to be of an entirely different nature.

In the first period the three instruments agree; in the second, the horizontal components differ, and all similarity is wanting in the vertical force and declination during the greater part of the total duration.

A long calm period, 46^h in duration, followed these large disturbances; during which the series of small peaks and hollows were reproduced in the three curves, chiefly in the morning (17^h to 21^h) of the 21st and 22nd days.

Third Disturbance.—Another disturbance commenced about 6^h on the

23rd of February, which lasted up till 12^h 30^m. The horizontal force was in motion from 3^h 50^m.

The general appearance of this disturbance at Lisbon is a large decrease of declination and horizontal force, and an increase of vertical force for some hours.

It is noteworthy that the waves in the Lisbon curves are clean and rounded in the declination and vertical force. The Kew curves show also fewer peaks and hollows than during the former disturbances.

At Lisbon this general rule is found to exist: the declination disturbance is opposite in direction to the vertical force. The declination curve agrees very well with the Kew curve; the variations of the latter are larger, as is usual. There is one remarkable circumstance, the first *minimum* (6^h 33^m) happens 6^m or 7^m before Kew, the other *maxima* and *minima* agree, with the exception of very small differences, which may be attributed to the difficulty of determining precisely the extreme points of the Lisbon curve, on account of their roundness.

The vertical-force curves show a general similarity, but the connexion between the different phases is not seen. It is remarkable that the general form of the Kew vertical-force curve has a great likeness to the Lisbon horizontal force, but in an inverted order, although the extreme opposite points of maxima and minima do not occur at the same time.

The horizontal-force curves agree very well up to 8^h; after this time it is easily seen that the Kew curve agrees almost exactly with the Lisbon vertical force.

Fourth Disturbance.—Two less important periods follow this period, which terminate about 15^h on the 24th: after ten hours of comparative calm the magnets are again set in motion at Lisbon, by a deviation of the horizontal force and declination and a depression in the vertical force, about 1^h 45^m on the 25th. This disturbance is composed of three large *waves*, much agitated, and full of *peaks* and hollows, or serrated.

At the first glance we see immediately that the general trait of the disturbance is identical at Kew and Lisbon, *i. e.* the different phases of the three instruments agree with one another.

The Kew curve generally agrees with the Lisbon one, although several periods are more developed, particularly some waves between 5^h 30^m and 9^h more developed at Kew. The horizontal-force curves also agree; but it must be remarked that the *waves* (between 5^h 30^m and 9^h), which are most developed at Kew in the declination, are less developed in the horizontal force than at Lisbon.

The two vertical-force curves generally agree; but the phases at Kew are in advance of those at Lisbon. The small *peaks* (those which can be identified) are inverted and simultaneous. It is also noteworthy that the first vertical-force movement at Kew is opposite to that at Lisbon.

The vertical-force Lisbon curve is greater in its movements than the declination, and consequently deviates from the general law.

Thus we see that the same periods of certain disturbances are manifested very differently in two stations so near each other as Kew and Lisbon. The modification is the greatest, particularly in the periods which depart from the general rule at Lisbon, and are doubtless also abnormal at Kew.

From the examples here quoted, it is evident that a great value would be attached to the curves from another intermediate station; for the little vertical-force *peaks* and *hollows*, being opposed at Lisbon and Kew, it would be very interesting to see if these peaks would be wholly or nearly absent at some intermediate station.

With a certain number of these magnetographs very discreetly placed, we may one day analyze the different forces acting on the needle in the different places on the earth—a manifest desirability.

II. "On Supersaturated Saline Solutions." By Charles Tomlinson, F.R.S. Received April 21, 1868.

(Abstract.)

This memoir is divided into six parts. The *first* part contains a definition of the subject; the *second* an historical sketch; the *third* is on the action of *nuclei* in inducing crystallization, and the effect of low temperatures on a number of supersaturated solutions contained in chemically clean vessels; the *fourth* is on the formation of a modified salt, as in the case of zinco-sulphate and sodic sulphate; the *fifth* contains an inquiry as to whether anhydrous salts form supersaturated solutions; and the *sixth* and last part is a summary with a classified list of the salts examined.

1. *Definition.*—When water at a high temperature is saturated with a salt, and, on being left to cool in a closed vessel, retains in solution a larger quantity of the salt than it could take up at the reduced temperature, the solution is said to be supersaturated.

2. *History.*—During many years the phenomena of supersaturation were studied with reference to solutions of Glauber's salt. In 1809, Ziz of Mayence* showed that the sudden crystallization of these solutions is not due to agitation; that the vessels containing the solutions do not require to be hermetically sealed; but if put under a bell-glass, or loosely covered as with a capsule, they can be preserved during a long time; that solids brought into contact with the solutions act as *nuclei* and produce instant crystallization, but that such solids act best as nuclei when dry; if wet or boiled up with the solution they become *inactive*. The most efficient nucleus is a crystal of the salt itself. Air, if artificially dried, ceases to be a nucleus. Three varieties of the sodic sulphate are noticed, *i. e.* the *anhydrous* and the ordinary 10-*atom hydrate*, and also a peculiar salt formed when supersaturated solutions in closed vessels are left to cool down. This

* Schweigger, 'Journal,' 1815, vol. xv.

x salt, as it is termed, contains less water of crystallization than the or
nary salt, and is more soluble. If the vessel in which it is formed be su
denly opened, or the mother-liquor touched with a nucleus, the moth
liquor instantly solidifies into the 10-atom hydrate, and the *x* crystals b
come opaque, like the boiled white of an egg.

In 1819 Gay-Lussac* referred the state of supersaturation to the inert
of the saline molecules, the molecular condition of the sides of the vess
and other causes. He also showed that solutions of some other salts exh
bit the phenomena of supersaturation. In 1832 the number of su
salts was shown by Dr. Ogden† to be not less that twenty-one.

In 1825 Faraday‡ published some experiments on the supersaturat
solutions of Glauber's salt. Graham §, Turner ||, Ure, and others also co
tributed new facts; but the most elaborate inquiry was by M. Löw
between the years 1850 and 1857, the results of which are contained
six memoirs¶. According to this writer, the ordinary 10-atom sodic su
phate increases in solubility from 32° to 93°·2 F., at which latter temper
ture it begins to fuse in its water of crystallization, and to deposit the a
hydrous salt. This salt follows an inverse order of solubility as compare
with the 10-atom hydrate, its solubility diminishing as the temperatu
rises; or, what is the same thing, from 218°, the boiling-point of a sat
rated solution, down to 64° the solubility increases; but at 64° the solutio
undergoes a new molecular modification, and begins to form crystals of th
7-atom hydrate (the *x* salt of Ziz). This salt is much more soluble at o
dinary temperatures than the 10-atom hydrate, its maximum solubility bein
at 80°·6. Thus the sodic sulphate has three maxima of solubility; vi
93°·2 when it is under the molecular constitution of the 10-atom hydrat
78°·8 to 80°·6 when it is under the molecular constitution of the 7-ato
hydrate, and 62°·6 to 64°·4 when it is under the molecular constitution
the anhydrous salt. At these three maxima the saturated solutions a
about equally rich in salt. The 7-atom hydrate and the anhydrous sa
can only maintain their molecular constitution when in contact with th
mother-liquor in closed vessels, in which they are sheltered from the ai
and from other bodies that act upon them as nuclei. No sooner are the
exposed to the air than they become opaque and warm, and assume the mo
lecular constitution of the 10-atom hydrate as well as its solubility. Henc
the conclusion is that supersaturated solutions of the sodic sulphate are no
really so, since they hold a salt of much greater solubility at ordinary tem
peratures than the normal 10-atom salt. Löwel extends his inquiry t
sodic carbonate and magnesia sulphate, and endeavours to show that in thei
supersaturated solutions salts of a lower degree of hydration and of greate
solubility than the normal salts are formed; and his general conclusion i

* Annales de Chimie et de Physique, 2nd ser. vol. xi. † Edinb. New Phil. Jour
‡ Quarterly Journal of Science, vol. xix. § Trans. Roy. Soc. Edinb
|| Elements of Chemistry.
¶ Annales de Chimie et de Physique, 3rd ser. vols. xxix., xxxiii., xxxvii., xliii., xliv., xli

that all cases of supersaturation are in appearance only, and not in fact. As to the function of nuclei and the inner sides of the flasks in determining crystallization, he regards it as the effect of one of those mysterious contact actions known as *catalytic*, of which science has not yet been able to give a satisfactory explanation. Bodies that appear to be active in inducing crystallization are designated as *catalytic* or *dynamic*, while bodies that are apparently inactive are termed *non-catalytic* or *adynamic*. "It appears certain," he says, "that but for the mysterious action which the air and other bodies exert on supersaturated solutions, we should obtain sulphate of soda only in the modified state; that is, crystallized with seven equivalents of water, and possessing at ordinary temperatures of the air a much greater solubility than that of the normal 10-atom salt."

Later inquirers have endeavoured to explain the nature of the force exerted by nuclei in inducing crystallization under certain conditions, and their passivity under others. Gernez* tried no less than 220 solids, and of these he selected 39 that were active in inducing crystallization: 18 were insoluble; these were carefully washed in distilled water, and dried out of contact with air. When dry they were found to be without action on the solutions they had previously caused to crystallize. The 21 soluble substances were purified by recrystallization, and they all became inactive. Hence it is concluded that sulphate of soda is the only nucleus for solutions of the same salt. That is to say, whenever a glass rod or other body acts as a nucleus, it is contaminated with minute portions of the salt itself, which M. Gernez believes to exist in the air, not only of towns, but in the country. According to this view, the supersaturated solution of any other salt can only be crystallized by a saline nucleus of its own kind. But, as M. Jeannel† has pointed out, if this theory be true, we must have floating in the air specimens of all kinds of salts that form supersaturated solutions, and crystallize by the introduction of a solid nucleus; whereas there are some such salts which cannot exist in the presence of the oxygen or of the ammonia of the air. M. Jeannel shows that a few drops of an ordinary solution of a salt will induce crystallization in a supersaturated solution of the same salt without contact of air.

3. *On the Action of Nuclei.*—With respect to the action of nuclei on saline supersaturated solutions generally, the author refers to a theory of his ‡ which seems to account for the liberation of gases from their supersaturated solutions (soda-water, seltzer-water, champagne, &c) when a solid nucleus that had been exposed to the air is immersed in them; while such nucleus becomes inactive if kept long in water, or passed through flame &c., and dried or cooled out of contact with air. The action of nuclei is referred to adhesion. Nuclei are active in inducing crystallization,

* Comptes Rendus, vol. lx. p. 833. A similar method was adopted by Schiff, Ann. der Chem. und Pharm. vol. cxi. p. 68.

† Ann. de Chim. et de Phys. 4th ser. vol. vi. p. 166; and Comptes Rendus, Jan. 2, 1866.

‡ Phil. Mag. July and August 1867.

or they are inactive, according to the state of chemical purity of their su faces. In the case of a supersaturated saline solution, the sides of t vessel may act as nuclei, or any solid, and some liquid, bodies brought in contact with it. Now suppose the inner surface of the vessel to be ma chemically clean, either by well washing it with strong sulphuric acid, caustic alkali, or spirits of wine, or, as often happens, by boiling the sali solution in the vessel in which it is intended to be kept. In such ca there is perfect adhesion between the sides and the solution, and no salt w be liberated; the sides may in fact be regarded as merely a continuation the liquid itself, and no salt can be formed there any more than in the ce tral parts of the liquid. But suppose the sides to be not chemically clea to be more or less dirty in fact; in such cases adhesion is diminished destroyed, and the surface of the liquid next to such sides is virtually as fr as its upper surface. Salt will be deposited there, other circumstanc being favourable, really from want of adhesion between the side and t liquid that holds the salt in solution. Now apply this to the case of so-called "adynamic," "non-catalytic," or "inactive" glass rod, or coin, fragment of glass or of flint, &c. A glass rod placed in the solution do nothing more than form new sides, as it were, to the vessel, and its effe is merely that of the sides. If chemically clean, the rod will form crystals about it, and hence it is "inactive" because its adhesion is pe fect. If dirty, the surface of the solution in contact with it will be as fre or almost so, as the upper surface. It requires special means to produce chemically clean surface; and when produced, it is not easy to maintain A short exposure to the air, or a mere touch, will suffice to cover it with organic film, or with motes or dust that prevent or lessen adhesion betwe it and the aqueous part of the solution, and apparently render an inacti solid active. When a glass rod &c. has been kept in water or pass through flame and dried, or cooled out of contact with the air, it is more less chemically clean, and remains so while being sheltered. When Ziz fou a knitting-needle active on one solution, and by passing it through the co which confined a similar solution it became inactive, he simply made t wire chemically clean by the friction. Air is not a nucleus, and when appears to act as such, it is simply as a carrier of some solid particle n chemically clean. Hence narrow-necked flasks when opened retain the solutions liquid longer than wide-necked ones, as the former are less like to catch motes &c. from the air than the latter. Supersaturated solutio are best preserved by plugging the necks of the flasks &c. with cotton-woo since in cooling down the air is filtered in passing through the plug, a motes and dust are thus kept back.

Tubes made chemically clean by the action of strong sulphuric acid m be filled with a strong solution of sodic sulphate, and when cold the tub may be placed in a freezing-mixture at 10° F. without any separation the salt. Hence the author differs from M. Löwel's theory, which suppos a molecular change to take place when strong solutions of the salt a

cooled down below 60°. Supersaturated solutions of various salts were cooled down to various temperatures from 32° to 0° F. without crystallizing. Sodic acetate, for example, was kept for some hours at 14°, when on touching it with a wire it became solid, and the temperature rose to 104°. Sodic arseniate, sodic succinate, sodic borate, sodio-potassic tartrate, potash alum, and other saline solutions were treated in this way. Some of these solutions become viscid at a low temperature, and do not immediately crystallize on removing the cotton-wool plug. If they be touched, or the side of the flask scratched with a chemically clean wire, there is no action; but if the wire be not chemically clean, the scratches immediately become chalky white by being covered with minute crystals of the salt, and the action then spreads until the solution becomes solid.

Some salts that are not very soluble in water, such as the plumbic acetate, form highly charged supersaturated solutions, and retain their liquid state below ordinary atmospheric temperatures. When at a certain point they suddenly solidify. Other solutions merely deposit the excess of salt above the condition of supersaturation, leaving the mother-liquor saturated; the cupric sulphate is an example of this.

The memoir contains a number of details respecting the action of nuclei, whether derived from the air, from the flask, from the salt itself, from the filter, or the cotton-wool used in closing the vessels. If the solution touch the wool, crystallization immediately sets in; or if the upper part of a chemically clean tube be touched with a finger slightly greasy before filtering into it the hot solution, the latter will cool down to the temperature of the air without crystallizing, nor will there be any effect if the tube be inclined so as to touch the clean portions of the inner surface; but the moment the solution comes upon the edge of the finger-mark, crystallization sets in, and the solution becomes solid. Solutions not filtered that begin to crystallize at above 100° in open vessels, or even in closed flasks, may by filtration be freed from nuclei, and so cooled down in the latter to low temperatures without any separation of the salt.

4. *On the formation of a modified salt.*—The readiness with which sodic sulphate parts with its water of crystallization, and two or three other considerations, make it more than probable that a solution of sodic sulphate at high temperatures is really a solution of the anhydrous salt. But M. Löwel supposes that a supersaturated solution in cooling down below 60° assumes a new molecular constitution, viz. that of the more soluble 7-atom hydrate which it then holds in solution. The author gives an experiment to show that such cannot be the case, but that the solution continues to hold the anhydrous salt until a portion of it actually separates. If a boiling solution of two parts salt to one part water be filtered into vessels made chemically clean by being washed out with spirits of wine instead of sulphuric acid, and if these vessels, when cold, be placed in water at 32°, or from that to 40°, a few octohedral crystals of the anhydrous salt will be thrown down. The temperature will slightly rise; and if the tube be now

set aside in a moderately warm air, the anhydrous salt will enter into solution, forming a dense lower substratum, from which the 7-atom hydrate will be produced in small quantity, there not being sufficient water present to form the ordinary 10-atom salt. The rest of the solution is still supersaturated, and if the plug be removed from the vessel, crystallization will set in from the surface and proceed rapidly downwards, carrying down enough water to convert the whole solution, as well as the 7-atom, into the 10-atom hydrate.

This process may be conveniently watched in the case of the zinc sulphate. When a saturated solution of this salt cools down from the boiling-point to about 70°, the monohydrated salt is thrown down in quantity, and, as the solution cools, a portion of this dissolves and a crop of acicular crystals is produced which readily melt down at about 100°. On removing the cotton-wool from the tube, crystallization sets in from the surface, and the ordinary 6-atom hydrate is produced.

The author examines M. Löwel's experiments on solutions of the sodic carbonate in which two modified hydrates are pointed out, viz. the 7 H O *a* and the 7H O *b*, which differ in solubility from each other and from the 10-atom salt; but as M. Löwel attaches great importance to the peculiar catalytic properties of the sides of his vessels in determining the formation of these salts, the author cannot help thinking that M. Löwel's results were due to portions of the sides of his vessel, not chemically clean, acting as nuclei. In chemically clean vessels M. Löwel's results have not been reproduced; for on reducing the temperature to a certain point depending on the strength of the solution, the whole became suddenly solid, with a rise in temperature of 35° or 40°. M. Löwel also points out two modifications produced from supersaturated solutions of the magnesia sulphate. The author has placed boiling saturated solutions, when cold, in freezing-mixtures at 10° without producing any separation of the salt.

The ammonia phosphate throws down from its supersaturated solution an anhydrous powder, which, again entering into solution, forms a dense lower stratum in which a modified transparent crystallized salt is formed in small quantity.

The strontic nitrate also deposits an anhydrous salt in cooling down to about 62°; but as this salt is not soluble in the solution, the modified salt is not formed.

Some solutions on being cooled down in freezing-mixtures suddenly become solid; others freeze and sometimes thaw again without any separation of the salt, as in the case of the cupric sulphate; but if a boiling saturated solution of this salt be prepared with strict attention to chemical purity, it may be cooled down to near 0° F. without any separation of the salt.

5. *Anhydrous Salts.*—The method adopted to ascertain whether an anhydrous salt forms a supersaturated solution was to make a solution of known strength, as indicated by some good Table of solubilities, raise it to

the boiling-point, and then note whether salt began to be thrown down when the solution cooled down to the temperature indicated by the Table. For example, according to Poggiale's Table, 100 parts of water at 158° will dissolve 129·6 of sodic nitrate. This is the same thing as 622·22 grains of the salt in 1 ounce water. Such a solution on cooling down from the boiling-point began to deposit salt at 160°.

In like manner, according to Gay-Lussac's Table, 100 parts of water at 150° F. contain 125 of potassic nitrate. A solution of 125 parts salt to 100 of water began to deposit salt at about 149°. The deposit first began to be made on the side nearest the window, or the coldest side, when the flask was suspended in air; but if the flask were placed on metal, or any other good conductor, a ring of salt was first formed at the bottom, some 6° or 8° earlier than if the flask stood on a block of wood.

It has been frequently stated that the potassic bichromate forms a supersaturated solution. According to Kremer, 200 of water at 140° F. dissolve 100 parts of the salt. Such a solution, on cooling from the boiling-point began to throw down crystalline flakes at 138°. The remarkable deepening in colour of this solution under the influence of heat is pointed out.

Sal-ammoniac, potassic chlorate, and some other salts were also examined, the conclusion being that anhydrous salts do not form supersaturated solutions.

6. *Conclusion and Summary.*—The author refers to the prevailing theory that supersaturation exists in appearance only and not in fact, since it is supposed to be the modified and more soluble salt that is in solution. If this were true, it ought to apply to all cases of supersaturation, and it has only been claimed in the case of a very few salts, and in them much importance has been attached to the active or the inactive condition of the sides of the vessels containing the solutions.

The author, while admitting, in the case of a very few solutions, that a modified salt may be deposited, denies that it is due to any molecular change that takes place in the solution, either from reduction of temperature or any catalytic property of the sides of the vessel. His theory is that when these modified salts are formed, it is the anhydrous salt that is held in solution, a portion of which is thrown down as the temperature falls; and this anhydrous deposit, entering again into solution, forms a dense substratum containing less water than the upper portions, so that when the modified salt forms in it, it is out of the reach of sufficient water to form the normal salt. When, on the contrary, under the influence of a nucleus, crystallization sets in from the surface, the normal salt is formed, and the crystals carry down sufficient water to convert the whole into the ordinary hydrated salt.

As to the action of nuclei or the sides of the vessel, when chemically clean the solution adheres to them as a whole, and there is no separation of the salt; when not chemically clean there is a stronger adhesion between the salt and the nucleus than between the salt and the solvent, and there

is a separation of salt; and the action of separation once begun, may rapidly propagated throughout the whole solution. Boiling saturated sol tions may be cooled down in chemically clean vessels and kept for any leng of time, not because they undergo any molecular change or hold a salt greater solubility than the normal salt in solution, but they retain th fluid form simply from the absence of a nucleus.

The salts examined in this memoir are arranged into five groups accor ing to their behaviour.

I. Salts of which the supersaturated solutions remain liquid at l temperatures.

Examples:—Sodic sulphate.
Sodic acetate.
Sodic arseniate.
Sodic succinate.
Sodic borate.
Sodio-potassic tartrate.
Potash alum.
Magnesia sulphate.
Baric acetate.
Calcic chloride.
Cupric sulphate.

II. Salts of which the supersaturated solutions suddenly solidify at l temperatures.

Examples:—Sodic carbonate.
Sodic phosphate.
Plumbic acetate.
Sodic hyposulphite.
Strontic chloride.

III. Salts of which the supersaturated solutions deposit their excess salt at low temperatures or under the action of a nucleus, leaving the mothe liquor saturated.

Examples:—Zinco-acetate.
Cupric sulphate.
Baric chloride.
Potassic arseniate.
Antimonio-potassic tartrate.
Citric acid.

IV. Salts of which the supersaturated solutions form modified salts of lower degree of hydration.

Examples:—Zinco-sulphate.
Sodic sulphate.
Magnesia sulphate.
Ammonia phosphate.

It will be seen that the sodic sulphate and the magnesia sulphate also occupy a place in Class I.

V. Anhydrous salts examined in this memoir that do not form supersaturated solutions:—

Potassic nitrate.
Potassic bichromate.
Sal-ammoniac.
Sodic nitrate.
Potassic chlorate.
Potassic ferrocyanide.
Baric nitrate.
Plumbic nitrate.
Ammonium nitrate.

III. "On the Impact of Compressible Bodies, considered with reference to the Theory of Pressure." By R. Moon, M.A., Honorary Fellow of Queen's College, Cambridge. Communicated by Prof. J. J. Sylvester. Received April 22, 1868.

(Abstract.)

Suppose that we have two *rigid* cylinders of equal dimensions, which have their axes in the same straight line; suppose, also, that one of the cylinders is at rest while the other moves towards the first with the velocity V in a direction parallel to both the axes; the consequence of the collision which under such circumstances must take place, will manifestly be that half the momentum of the moving cylinder will be withdrawn from it, and will be transferred to the cylinder which originally was at rest.

The mode in which velocity or momentum will thus be collected from the different parts of the one cylinder, and distributed amongst those of the other, is obvious. Exactly the same amount will be withrawn from the velocity of each particle of the impinging cylinder, and exactly the same amount of velocity will be impressed on each particle of the cylinder struck.

And the reason of this is equally obvious; since, if such were not the case, the particles of each cylinder would *contract*—a supposition which is forbidden by the very definition of rigidity.

But if, instead of being perfectly rigid, each cylinder is in the slightest degree compressible, a variation in the effect will occur.

As before, momentum of finite amount will be transferred from the one cylinder to the other, but the mode of collection of the velocity withdrawn from the one, and the mode of distribution of that injected into the other, will no longer be the same as before.

In order that the moving cylinder may not be reduced to absolute r
by the collision, it is obvious that the cylinder originally at rest, o
portion of it, must be moved out of the way, so as to allow of the c
tinuance, even in a modified degree, of the other's motion; and this
only be effected on the terms of a transference of velocity or moment
taking place from the one cylinder, or part of it, to the other cylinder,
part of it.

But when the cylinders are compressible, we are freed from two con
tions which obtain when the cylinders are rigid.

In the first place, it is no longer necessary to suppose, neither should
be justified in assuming, that the velocity abstracted from each particle
the impinging cylinder, or transferred to each particle of the cylin
struck, is the same; on the contrary, all experience tells us that, in bod
susceptible to compression, compression is always produced by collisio
in other words, that variation of velocity, in the parts about which t
collision takes place, is the immediate and invariable concomitant
collision.

In the second place, when the cylinders are compressible, it is no long
essential to suppose that the effect of the collision will be to withdr
velocity from every particle of the impinging cylinder, and to impart ve
city to every particle of the cylinder struck. Undoubtedly such may
the case if the cylinders are short, if they are possessed of only a moder
degree of rigidity, and if the velocity before impact of the impingi
cylinder is considerable. But if the cylinders be long, while the veloci
of the impinging cylinder is of moderate amount, the contrary m
occur. The condition that the cylinder originally at rest shall not oppo
an immediate insurmountable barrier even to the modified motion
the other may, obviously, be sufficiently satisfied if a motion of contra
tion is imparted by the collision to a definite portion of the secon
cylinder.

But when the cylinders are compressible, equally as when they are rigi
the collision must cause the instantaneous abstraction of velocity or m
mentum, either from the whole of the impinging cylinder, or from a def
nite part of it, and the instantaneous communication of the velocity
withdrawn, either to the whole of the cylinder struck, or to a definite pa
of it.

We have hitherto assumed the velocity of each particle of the impingi
cylinder to have been originally uniform. Let us now suppose, howeve
that immediately before impact a counter velocity of variable amount
impressed on the different parts of the impinging body, so that, at t
instant of impact, *before taking account of the effect of collision,* the vel
city at any point of the impinging body may be expressed by $V-V$
where V is constant, but V_1 has the value zero at the surface of collisio
and thence gradually increases as we recede towards the other extremi
of the cylinder, so that $V-V_1$, which expresses the velocity of the im

pinging cylinder before impact, has its greatest value at the surface of collision, and diminishes as we recede therefrom.

It is clear that, in the case we are now considering, the collective momentum abstracted from the impinging cylinder by the collision will be less, and finitely less, than that which was abstracted by the collision in the former case, in which the velocity of each particle of the impinging cylinder was supposed uniform and equal to V.

For, if M be the momentum lost by collision when the velocity before impact is uniform and equal to V, it is clear that when the velocity before impact is represented by $V-V_1$, the quantity V_1 may be such that the momentum *before impact* may be finitely less than M; from which it follows inevitably that the amount of momentum lost by collision in this latter case must be less than M.

Let us now vary the *data* by supposing that the velocity before impact *increases* instead of diminishes as we recede from the surface of collision; so that at the moment of impact, before taking account of the effects of collision, the velocity at any point of the impinging cylinder is represented by $V+V_1$ instead of $V-V_1$.

It is clear that the momentum abstracted by the collision in this latter case will be *greater*, and finitely greater, than in the case where the velocity before impact is uniform and equal to V. Let the additional momentum abstracted in this case be M_1, the whole momentum so abstracted being represented by $M+M_1$.

Let us now make a final variation in the conditions of the problem, by supposing that at the moment of impact, and irrespective of the impact, a velocity equal and opposite to V is communicated to each particle of the impinging cylinder, so that at that instant, without taking account of any action of the one cylinder upon the other, the velocities of the two cylinders along their surfaces of contact will be equal, or, rather, will be alike zero; at the same time that at every other point of the impinging cylinder there will be a variable velocity V_1 increasing in amount as we recede from the surface of contact.

In estimating the effect of the cylinders being in contact under the circumstances last described, it is clear that the abstraction from each particle of the impinging body of the velocity V can only be regarded as preventing the transference to the second cylinder of so much of the momentum $M+M_1$ as that velocity, if it had constituted the entire velocity before impact of the impinging body, would have given rise to, viz. M; and that the momentum M_1, whose appearance in the expression $M+M_1$ is due to the fact of the first cylinder having been originally endowed with the variable velocity V_1 in addition to the constant velocity V, will still continue to be transmitted to the second cylinder from the first.

We are thus led to this singular and, doubtless, pregnant conclusion, that in a continuous material system in which there is neither discontinuity of velocity nor discontinuity of density, all the consequences of

collision may occur, viz. the instantaneous transmission of a finite amo of momentum from one part of the system to another, provided we h discontinuity in the *tendency to compression* in the different parts of system.

The author has endeavoured, in former communications to the Royal ciety, to show that when the velocity in a fluid diminishes in the direct to which the motion tends, the slower particles will offer a resistance to motion of the faster particles, which the received theory fails to take i account. The foregoing speculation goes to prove that the circumsta of the surfaces of contact of contiguous elements of the fluid having same velocity, constitutes no objection to the reality of such resistance

IV. "On the Tides of Bombay and Kurrachee." By Willi Parkes, M.Inst. C.E. Communicated by G. B. Airy, Ast nomer Royal. Received May 5, 1865.

(Abstract.)

The object of this paper is to exhibit the phenomena of diurnal inequa in the tides on the coasts of India, and describe the mode adopted by author for obtaining formulæ based on astronomical elements for pred ing them. It is accompanied by the following records of observations gi in a diagram form :—

Kurrachee,	1857–8,	December to March.
„	1865,	March to August.
„	1867,	The whole year.
Bombay	1867,	February to May.

The height and times predicted by the author for 1867, and publisl by the India Office, are given on the diagrams for that year, so that tl may be compared with actual observation.

The continuous curves of the height of the water taken at Bombay, every ten minutes for the four months above named, are also given.

By the rotation of the earth every meridian-line is brought twice a under the influences which ultimately result in the well-known semidi nal tidal movements—once when in the position nearest to the attract body, and once when in that furthest from it. But the actual point that meridian which is in the centre of those influences will be alternat north and south of the equator, to the extent of the declination of attracting body. This alternation of the position of the centre of attr tion from the northern to the southern hemisphere produces a diur tide, and that diurnal tide produces a diurnal inequality in the semidiu tide

The character of the diurnal tide and the highly complex conditions under which its constantly varying solar and lunar component parts are combined are then traced. Being entirely dependent on the declinations of the sun and moon, the solar element vanishes twice a year, and the lunar element twice a month, each reappearing after the solar or lunar equinox, with its times of high and low water reversed.

The diurnal tide produces a diurnal inequality in height and time of high and low water, affecting simultaneously respectively high-water time and low-water height, and high-water height and low-water time. In particular cases, the actual values of height and time of diurnal tide may be directly deduced from the values of diurnal inequality. From these it was found that diurnal tide follows the moon's movements at a much shorter interval than semidiurnal, the retard of the former being from two to three hours only, while that of the latter is from thirty-four to thirty-six hours.

The mode adopted for identifying the varying values of diurnal inequality with their physical causes was then explained. A hypothetical series of diurnal tides, based on the varying values of the declination of the sun and moon, was calculated, the necessary local constants being deduced from the particular cases in which their values could be directly obtained. These hypothetical diurnal tides being combined with a series of semidiurnal tides deduced from the diagram of observations, the diurnal inequalities so obtained were compared with the actual diurnal inequalities. It was then found that a further element was wanting, which was approximately and provisionally obtained by the introduction of a second empirical diurnal lunar tide of twelve inches maximum half-range at Bombay, and six inches at Kurrachee. This tide was assumed, like the first and principal diurnal tide, to be dependent on the moon's declination, but to vanish at intervals of two or three days, before the moon crossed the equator. The author expresses an opinion that this empirical correction might probably be superseded by one more consistent with physical causes, if more extended and more correct observations were subjected to investigation.

Lastly, the comparison of calculated heights and times with the records of observations for four months at Bombay and eight months at Kurrachee were given. This showed that three calculated tides out of four were correct within three inches in height and fifteen minutes in time, the errors of the remainder ranging up to nine inches in height, and thirty minutes in time.

Since receiving the observations made at Bombay and Kurrachee in the year 1867 the author has subjected them to another process for obtaining the actual times and heights of diurnal tide, which has been more successful than that described in the paper.

The only data made use of were the diurnal inequalities in *height* at high and low water, the range of semidiurnal tide and the diurnal ine-

quality in time, which were necessary to the previous process, being altogether disregarded.

The diurnal inequalities in height were obtained by measuring the w of the brown spaces where they were crossed by the vertical lines r senting noon on successive days. The two daily values thus obtaine respectively the sine and cosine of an angle which represents the diffe in time between semidiurnal and diurnal tide. Dividing the low-wate the high-water value gives the cotangent of that angle, and thenc angle itself. Thus the time of actual diurnal tide (first in relation t time of semidiurnal low water, and then in relation to solar time) obtained.

The actual range of diurnal tide was obtained by adding togethe squares of the high-water and low-water values (sine and cosine), taking the square root of the sum.

With these two series of results as ordinates, curves were drawn re senting times and ranges of actual diurnal tide, which were thus prese in a convenient form for comparison with the diurnal tide which had previously calculated.

The comparison confirmed the previous conclusion that the tide b on the simple declination theory was insufficient, and the empirical co tion which had been adopted seemed to provide an approximation to required addition to it, both in time and height. But it appeared th better coincidence in time would have been obtained by assuming the d nal tide at Kurrachee to be forty minutes earlier. This supposition tested by treating the observations of 1865 in a similar manner, and by recalculating a portion of the tides of 1867 with the earlier diu tide. In both cases the supposition was confirmed, a better agreem being obtained.

On treating the Bombay observations in the same manner, a fair gen coincidence with the calculated diurnal tides was found to exist; bu was further found, on comparing together the Kurrachee and Bom curves of actual diurnal tide (thus for the first time recorded for the sa period), that the times were nearly identical at the two ports, and range at Bombay about one-tenth greater than that at Kurrachee.

The tables for the four months over which the Bombay observations tend were recalculated with the diurnal tides which had been calcula for Kurrachee (but made forty minutes earlier, and increased in range one-tenth), and the result was quite as good as that shown by the origi tables. This fact would seem to point to the possibility that the diur tide is a vertical undulation, acting simultaneously, or nearly so, ove large area.

V. "Observations of the Spectra of some of the Southern Nebulæ." By Lieut. JOHN HERSCHEL, R.E. Communicated by W. HUGGINS, Esq. Received May 20, 1868.

[Lieut. Herschel, to whom the observations of the solar eclipse of August next have been entrusted by the Royal Society, has already employed the instruments, placed in his hands by the Royal Society for the observations of the eclipse, to good account by commencing an examination of some of the brightest of the nebulæ of the southern heavens. The first results of this examination, which are contained in the present paper, were obtained at Bangalore, Madras Presidency, during the months of March and April 1868. The instruments consist of an equatorially mounted telescope of 5 inches aperture, driven by a clock furnished with a pendulum-governor by Cooke and Sons, and a spectroscope by Messrs. Simms furnished with one dense prism of flint glass, and with a micrometer-screw and photographic scale for measuring the lines. The nebulæ No. 4390 and No. 2102 have been described by me, Phil. Trans. 1864, p. 439, and Phil. Trans. 1866, p. 383. —W. H.]

No. 3531. [Æ $13^{h}\ 19^{m}$: N.P.D. 136° 37′ : !!; globular cluster of stars; ω Centauri.]

March 25. A large cluster visible to the naked eye, oval-shaped, brighter towards the central part. *Spectrum an indefinable haze; no lines.*

No. 2197. [Æ $10^{h}\ 40^{m}$: N.P.D. 148° 57′ : great nebula in Argus.]

Spectrum: *lines distinctly visible*, but not clear enough to be separated; approximate position $D + 1{\cdot}8 \pm {\cdot}3$ ($D=2{\cdot}30$, $E=3{\cdot}68$, $b=3{\cdot}97$, $F=5{\cdot}03$). An unsatisfactory observation: to be looked for again.

No. 2017. [Æ $10^{h}\ 1^{m}$: N.P.D. 129° 47′ : !!; planetary nebula, very bright, very large, little extended, * 9M.]

March 31. Found with difficulty in the spectroscope. After a minute or two's examination the tube was accidentally disturbed, and before direction could be again obtained, clouds had gathered and work was stopped. Appearance in telescope: a nebulous-looking star; under a higher power a nebulous-looking object with a much brighter nucleus or centre. In spectroscope: *a continuous streak with a blotch of light nearly in the middle of its length*, two-fifths, by estimation, from the red end; slit quite wide.

No. 2581. [Æ $11^{h}\ 44^{m}$: N.P.D. 146° 27′ : a planetary nebula, small, round; blue = * 7M.]

April 2. *A pretty well-defined and bright short line* was distinctly visible in the spectroscope, accompanied by a considerably fainter and more refrangible companion. Principal line measured with the wires and found to be $D+2{\cdot}1=4{\cdot}4$ ($b=3{\cdot}97$, $F=5{\cdot}03$), i. e. $b+0{\cdot}4$.

No. 4083. [Æ $15^{h}\ 12^{m}$: N.P.D. 87° 25′ : !!; globular cluster, very bright, large, extremely compressed in the middle.]

April 5. Seen in telescope as a slightly oval nebulous ball, easily seen not very bright (perhaps owing to moon, nearly full); found with some culty in spectroscope; *a faint continuous spectrum* of considerable wi *no trace or suspicion of lines.*

(No. 4173. Seen easily in telescope; but looked for in spectros for two hours in vain.)

No. 4390. [Æ 18^h 6^m: N.P.D. 83° 10′: planetary nebula; bright, very small, little hazy.]

April 6. Scarcely recognized as a nebula in the telescope. Seen in troscope: *a short bright line* with a fainter one on the more refran side, and a third strongly suspected. (Knowing so well the relative tions of the "usual" lines, it is impossible that an unprejudiced com rative opinion can be offered on such slight foundation as I have.) very slight extension laterally was given in this instance with the c drical lens.

No. 2102. [Æ 10^h 18^m: N.P.D. 107° 59′: !!; planetary nebula, bright, little extended.]

April 9. Seen at once in telescope with low power; and seen disti in the spectroscope *as a bright and a faint line* (the third line not se principal line measured with wires and found $=D+\left\{\begin{matrix}2{\cdot}14\\2{\cdot}16\end{matrix}=2{\cdot}15\right\}$ $b+0{\cdot}48$, $F=b+1{\cdot}06$.

No. 1179. Nebula in Orion. Examined for comparison. The spect of this nebula *shows the three lines distinctly, and three only*; they measured (with wires), and the results were:—

$$D+\left\{\begin{matrix}2{\cdot}17\\2{\cdot}20=2{\cdot}19\\2{\cdot}21\end{matrix}\right\}=b+0{\cdot}52,$$

$$D+\left\{\begin{matrix}2{\cdot}31\\2{\cdot}40\end{matrix}=2{\cdot}36\right\}=b+0{\cdot}69,$$

and

$$D+\quad 2{\cdot}78 \qquad =b+1{\cdot}11.$$

The places and descriptions of the objects enclosed within brackets taken from Sir John Herschel's "General Catalogue of Nebulæ" in Phil. Trans. for 1864.

P.S.—The other day a storm passed over us. As there was a good of lightning, I took the opportunity to examine its spectrum. I saw, expected, numerous bright lines; the blue nitrogen one, I suppose, m the brightest. A suspicion also of the red hydrogen-line C. I was m surprised at the brightness of the continuous spectrum, in which all principal prismatic colours were brilliant.

June 4, 1868.

The Annual Meeting for the election of Fellows was held this day.

Lieut.-General SABINE, President, in the Chair.

The Statutes relating to the Election of Fellows having been read, Mr. Hulke and Capt. Richards were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the Lists.

The votes of the Fellows present having been collected, the following Candidates were declared to be duly elected into the Society.

John Ball, Esq., M.A.
Henry Charlton Bastian, M.D.
Lieut.-Colonel John Cameron, R.E.
Prof. R. Bellamy Clifton, M.A.
Morgan William Crofton, Esq., B.A.
Joseph Barnard Davis, M.D.
P. Martin Duncan, M.B.
Peter Griess, Esq.
Augustus George Vernon Harcourt, Esq.
Rear-Admiral Astley Cooper Key, C.B.
Rear-Admiral Erasmus Ommanney, C.B.
James Bell Pettigrew, M.D.
Edward James Stone, Esq., M.A.
Rev. Henry Baker Tristram, M.A.
William Sandys Wright Vaux, Esq., M.A.

Thanks were voted to the Scrutators.

June 11, 1868.

Lieut.-General SABINE, President, in the Chair.

Capt. Sir Leopold M^cClintock, R.N., and Rear-Admiral Ommanney, C.B., were admitted into the Society.

The following communications were read :—

I. "On the Combustion of Hydrogen and Carbonic Oxide in Oxygen under great pressure." By E. FRANKLAND, F.R.S., Professor of Chemistry in the Royal Institution and in the Royal School of Mines. Received June 11, 1868.

In a former communication to the Royal Society* I described some researches on the effect of a diminution of pressure on some of the phenomena of combustion, and deduced therefrom the law that *the diminution in illuminating-power is directly proportional to the diminution in atmospheric pressure.*

Further experiments, made more than a year ago, on the nature of the luminous agent in a coal-gas flame†, led me to doubt the correctness of the commonly received theory first propounded by Sir Humphry Davy‡, that the light of a gas-flame and of luminous flames in general is due to

* Phil. Trans. vol. cli. p. 629 (1861).

† Lectures on Coal-gas, delivered at the Royal Institution in March 1867. Journal of Gas-lighting.

‡ Phil. Trans. for 1817, p. 75.

the presence of solid particles. In reference to gas- and candle-flames, it is now well known that the fuliginous matter produced when a piece of wire-gauze is depressed upon such flames, and the sooty deposit which coats a piece of white porcelain placed in a similar position, are not pure carbon, but contain hydrogen, which is only completely got rid of by prolonged exposure to a white heat in an atmosphere of chlorine. On pursuing the subject further, I found that there are many flames possessing a high degree of luminosity which cannot possibly contain solid particles. Thus the flame of metallic arsenic burning in oxygen emits a remarkably intense white light; and as metallic arsenic volatilizes at 180° C., and its product of combustion (arsenious anhydride) at 218° C., whilst the temperature of incandescence of solids is at least 500° C., it is obviously impossible here to assume the presence of ignited solid particles in the flame. Again, if carbonic disulphide vapour be made to burn in oxygen, or oxygen in carbonic disulphide vapour, an almost insupportably brilliant light is the result. Now fuliginous matter is never present in any part of this flame, and the boiling-point of sulphur (440° C.) is below the temperature of incandescence, so that the assumption of solid particles in the flame is here also inadmissible. If the last experiment be varied by the substitution of nitric oxide gas for oxygen, the result is still the same; and the dazzling light produced by the combustion of these compounds is also so rich in the more refrangible rays, that it has been employed in taking instantaneous photographs, and for exhibiting the phenomena of fluorescence.

Many other similar cases of the production of brilliant light from incandescent, gaseous, or vaporous matter might be cited; but I will mention only one other. Amongst the chemical reactions celebrated for the production of dazzling light, there are few which surpass the active combustion of phosphorus in oxygen. Now phosphoric anhydride, the product of this combustion, is volatile at a red heat; and it is therefore manifestly impossible that this substance should exist in the solid form at the temperature of the phosphorus-flame, which far transcends the melting-point of platinum. For these reasons, and for others stated in the lectures above quoted, I consider that incandescent particles of carbon are not the source of light in gas- and candle-flames, but that the luminosity of these flames is due to radiations from dense but transparent hydrocarbon vapours. As a further generalization from the experiment above mentioned, I was led to the conclusion that dense gases and vapours become luminous at much lower temperatures than aëriform fluids of comparatively low specific gravity, and that this result is to a great extent, if not altogether, independent of the nature of the gas or vapour, inasmuch as I found that gases of low density, which are not luminous at a given temperature when burnt under common atmospheric pressure, become so when they are simultaneously compressed. Thus mixtures of hydrogen and carbonic oxide with oxygen emit but little light when they are burnt or exploded in free air, but exhibit intense luminosity when exploded in closed glass vessels, so as to prevent their expansion at the moment of combustion.

I have recently extended these experiments to the combustion of jets of hydrogen and carbonic oxide in oxygen under a pressure gradually increasing to twenty atmospheres. These experiments were conducted in a strong iron vessel, furnished with a thick plate of glass of sufficient size to permit of the optical examination of the flame. The results are so remarkable that, although still far from being complete, I venture to communicate them to the Royal Society before the close of the Session. The appearance of a jet of hydrogen burning in oxygen under the ordinary atmospheric pressure is too well known to need description. On increasing the pressure to two atmospheres, the previously feeble luminosity is very visibly augmented, whilst at ten atmospheres' pressure the light emitted by a jet about 1 inch long is amply sufficient to enable the observer to read a newspaper at a distance of 2 feet from the flame, and this without any reflecting surface behind the flame. Examined by the spectroscope, *the spectrum of this flame is bright and perfectly continuous from red to violet.*

With a higher initial luminosity, the flame of carbonic oxide in oxygen becomes much more luminous at a pressure of ten atmospheres than a flame of hydrogen of the same size and burning under the same pressure. The spectrum of carbonic oxide burning in air is well known to be continuous; burnt in oxygen under a pressure of fourteen atmospheres, the spectrum of the flame is very brilliant, and perfectly continuous.

If it be true that dense gases emit more light than rare ones when ignited, the passage of the electric spark through different gases ought to produce an amount of light varying with the density of the gas; and this is in fact the case, for electric sparks passed as nearly as possible under similar conditions through hydrogen, oxygen, chlorine, and sulphurous anhydride emit light the intensity of which is very slight in the case of hydrogen, considerable in that of oxygen, and very great in the case of chlorine and sulphurous anhydride. When liquefied sulphurous anhydride is sealed up in a strong tube furnished with platinum wires, and the temperature then allowed to rise until the internal pressure amounts to three or four atmospheres, the passage of induction-sparks through the enclosed gas is attended with very brilliant flashes of light. Further, if a stream of induction-sparks be passed through air confined in a glass tube connected with a condensing syringe, and the pressure of the air be gradually augmented to two or three atmospheres, a very marked increase in the luminosity of the sparks is observed, whilst on allowing the condensed air to escape, the same phenomena are observed in the reverse order.

The electric arc from fifty cells of Grove's battery is incomparably more brilliant when mercury vapour, instead of atmospheric air, is interposed in the path of the discharge between the carbon points. The gases and vapours just mentioned have the following relative densities:—

Hydrogen	1·0
Air	14·5
Oxygen	16·0

Sulphurous anhydride	32·0
Chlorine	35·5
Mercury	100·0

It is obvious that the above results have a very direct bearing upon the views now generally held regarding the constitution of the sun, stars, and nebulæ; but I refrain from making any such application of them until I have the honour of laying before the Royal Society a complete account of these experiments.

II. "On the Occlusion of Hydrogen Gas by Metals." By Thomas Graham, F.R.S., Master of the Mint. Received May 22, 1868.

In my experiments, already published, on the occlusion of hydrogen by the metals palladium, platinum, and iron, the absorption of the gas was observed to be of uncertain occurrence at low temperatures, but was ensured by heating the metal, whether in the form of sponge or aggregated by hammering, and allowing it to cool slowly and completely in a hydrogen atmosphere. This fact was referred to the condition of absolute purity of the metallic surface being essential to the first absorbing action, as it is to the action of platinum-foil or wire in determining the combustion of the gaseous mixture of oxygen and hydrogen, as observed by Faraday. A new method of charging the metals with hydrogen at low temperatures has lately presented itself, which is not without interest.

When a plate of zinc is placed in dilute sulphuric acid, hydrogen is freely evolved from the surface of the metal, but no hydrogen is occluded and retained at the same time. A negative result was indeed to be expected from the crystalline structure of zinc. But a thin plate of palladium immersed in the same acid, and brought into metallic contact with the zinc, soon becomes largely charged with the hydrogen, which is then transferred to its surface. The charge taken up in an hour by a palladium plate, rather thick, at 12° amounted to 173 times its volume.

The absorption of hydrogen was still more obvious when the palladium plate was constituted the negative electrode in acidulated water to a Bunsen battery of six cells. The evolution of oxygen gas at the positive electrode continuing copious, the effervescence at the negative electrode was entirely suspended for the first twenty seconds, in consequence of the hydrogen being occluded by the palladium. The final absorption amounted to 200·4 volumes, and was greater in amount than the volume of hydrogen occluded by the same plate heated and cooled in an atmosphere of the gas, which did not exceed 90 volumes.

It is worthy of remark that, although the hydrogen enters the palladium and no doubt pervades the whole mass of the metal in such circumstances, the gas exhibits no disposition to leave the metal and escape into a vacuum, at the temperature of its absorption. Thus a thin plate of palladium, charged with hydrogen in the manner described, was washed, dried by a cloth, and then sealed up in an exhausted glass tube. On

breaking the tube under mercury after two months, the vacuum was found perfect. No hydrogen had vaporized in the cold (about 12°); but on the application afterwards of a heat of 100° and upwards, 333 volumes of gas were evolved from the metal.

A similar result was obtained on making a hollow palladium cylinder, of which the length was 115 millimetres, diameter 12 millimetres, and thickness 1 millimetre, the negative electrode in an acid fluid, while the closed cavity of the cylinder was kept exhausted by means of a Sprengel aspirator. No hydrogen whatever passed through into the vacuous cavity in several hours, although the gas was no doubt abundantly absorbed by the outer surface of the cylinder and pervaded the metal throughout.

It appears, then, that when hydrogen is absorbed by palladium the volatility of the gas may be entirely suppressed; and hydrogen may be largely present in metals without exhibiting any sensible tension at low temperatures. Occluded hydrogen is certainly no longer a gas, whatever may be thought of its physical condition. The same conclusion was indicated by another series of experiments, in which it was found that, to be occluded by palladium, and even by iron, hydrogen does not require to be applied under much pressure, but, on the contrary, when highly rarefied is still freely absorbed by these metals.

The occluded hydrogen is readily extracted from palladium by reversing the position of the latter in the decomposing cell of the battery, so as to cause oxygen to be evolved on the surface of the metal. The hydrogen is then drawn out as rapidly as it had previously entered the palladium, and the metal is exhausted in a complete manner by such treatment. When palladium charged with hydrogen is left exposed to the atmosphere, the metal is apt to become suddenly hot, and to lose its gas entirely by spontaneous oxidation.

Platinum may be charged with hydrogen by voltaic action, as well as palladium, but with the usual inferior proportion of gas. The charge of hydrogen taken up in a decomposing voltaic cell by old platinum in the form of a tube, of the thickness of a small crucible, was 2·19 volumes. This absorbed gas was also readily withdrawn from the platinum and oxidized on reversing the place of the metal in the decomposing cell. The platinum acquired its well-known polarizing-power in virtue of the occluded hydrogen. This power was retained by the metal after being washed with pure water and wiped with a cloth, and was brought into action on placing the metal in dilute acid. The temperature required to expel the hydrogen so absorbed by platinum was found to be little short of a red heat, although the gas had entered the metal at a low temperature.

Soft iron, left some time in a dilute acid, occluded 0·57 volume of hydrogen. This charge of gas was also retained at low temperatures, and did not escape into a vacuum till the temperature was raised nearly to redness. This proves that, like platinum, iron is not penetrated through in the cold by hydrogen, the temperature of emission being elevated considerably *.

* In M. Cailletet's experiment of exposing a thin sheet of iron to an acid, the metal

While hydrogen was absorbed freely by palladium and platinum negative plates, no oxygen whatever was absorbed by plates of the sa metals in the position of positive electrodes. Oxygen gas was disenga freely on the surface of the latter without being condensed. A platin plate which had acted for several hours as a positive electrode, gave aft wards, when submitted to heat with exhaustion, a small trace of carbo acid but no oxygen.

The familiar igniting-power of platinum sponge (or clean plate) upo jet of hydrogen in the air seems to depend solely upon the influence of metal upon its occluded hydrogen. The hydrogen appears to be polariz and to have its attraction for oxygen greatly heightened. I beg to o the following representation of this phenomenon, with an apology for purely speculative character of the explanation. The gaseous molecul hydrogen being assumed to be an association of two atoms, a hydrid hydrogen, it would follow that it is the attraction of platinum for negative or "chlorylous" atom of the hydrogen molecule which attac the latter to the metal. The tendency, imperfectly satisfied, is to the mation of a hydride of platinum. The hydrogen molecule is accordin polarized, *orienté*, with its positive or "basylous" side turned outwar and having its affinity for oxygen greatly enlivened. It is true that two atoms of a molecule of hydrogen are considered to be inseparable; this may not be inconsistent with the replacement of such hydrogen ato as are withdrawn, on combining with oxygen, by other hydrogen ato from the adjoining molecules. It is only necessary to suppose that a p of contiguous hydrogen molecules act together upon a single molecul the external oxygen. They would form water, and still leave a pai atoms, or a single molecule of hydrogen, attached to the platinum.

The oxidation of alcohol, ether, and similar hydrocarbons, through agency of platinum, likewise appears to be always an immediate con quence of a similar polarization of the hydrogen of those substances, or some other oxidable constituent.

As has already been remarked, it does not follow that, because a is occluded by a metal, under the pressure of the atmosphere, at a l temperature, the gas will also escape from the metal into a vacuum the same temperature, a much higher temperature being often required the expulsion of the gas than for its first absorption. This is particula true of carbonic oxide occluded by iron. Cast iron is much too porous such experiments, and allows carbonic oxide, equally with other gases, pass through abundantly by the agency of gaseous diffusion. Even w malleable iron there is a difficulty in observing, owing to the long ti during which that metal continues to discharge carbonic oxide from own store of that gas. But a malleable iron tube, first thoroughly prived of its natural gas, was found to allow carbonic oxide to pass throu

is no doubt penetrated through by hydrogen in the cold, but apparently from penetrating agency of the acid which is insinuating itself into the metal at the sa time.—*Comptes Rendus*, 4 Mai 1868.

it into a vacuum very slowly compared with hydrogen, although the volume of carbonic oxide which the metal is capable of absorbing is very sensible, amounting to 4 volumes, and more considerable than the volume of hydrogen which the same metal can occlude. Carbonic oxide did not sensibly pass through iron of 1·7 millimetre in thickness till the temperature was greatly elevated; and then the passage of gas was, in a minute—

Of carbonic oxide, at a full red heat, 0·284 cub. centim. per square metre of surface.

Of hydrogen oxide, at a full red heat, 76·5 cub. centims. per square metre of surface.

The condition of hydrogen as occluded by a colloidal metal may be studied with most advantage in its union with palladium, where the proportion of gas held is considerable. In the pulverulent spongy state, palladium took up 655 volumes of hydrogen; and so charged it gave off no gas *in vacuo* at the ordinary temperature, nor till its temperature was raised to nearly 100°. Hammered palladium foil has been observed to take up quite as much gas. But the condition in which palladium appears to be most absorptive is when precipitated from a solution of about 1·6 per cent. of the chloride, by the action of a voltaic battery, in the form of a compact metal. Palladium is not one of the metals readily thus precipitated; but it may be thrown down upon a thin platinum wire, in brilliant laminæ, by the action of a large single cell. The palladium after a time detaches itself from the wire, exhibiting a bright white metallic surface where it had been in contact with the platinum, and a dull surface, suggesting metallic arsenic, on the side exposed to the acid. As so prepared, it does not contain any occluded hydrogen. But the metallic films, when heated to 100° in hydrogen, and allowed to cool slowly for an hour in the same gas, were found to occlude 982·14 volumes of gas, measured with thermometer at 11°, and barometer at 756 millimetres. This is the largest absorption of hydrogen observed. From palladium so charged there was a slight indication of the escape of hydrogen into a vacuum, with extreme slowness in the cold. This charged palladium is represented by weight as

Palladium 1·0020 grm.	99·277
Hydrogen 0·0073 grm.....................	·723
	100·000

It is in the proportion of one equivalent of palladium to 0·772 equivalent of hydrogen*, or there is an approximation to single equivalents Pd H. But the idea of definite chemical combination is opposed by various considerations. No visible change is occasioned to the metallic palladium by its association with the hydrogen. Hydrides of certain metals are known, as the hydride of copper (Wurtz) and the hydride of iron (Wanklyn); but they are brown pulverulent substances with no me-

* H=1, Pd=106·5.

tallic characters. Indeed a hydride of palladium itself can be formed, not preserved, on account of its great instability. Following the pro of M. Wurtz for the hydride of copper, nitrate of palladium was bo with sulphuric acid, and the sulphate of palladium (a red crystalline salt) pared. A solution of this salt, with an excess of sulphuric acid, was prec tated by the hypophosphite of soda; a black powder fell, which speedily derwent decomposition at 0°, evolving copious volumes of hydrogen gas. final residue appeared to be pure palladium, of its usual black amorph appearance, and with no trace of crystallization. It is singular that palladium precipitate contained no occluded hydrogen; and even w heated, and afterwards exposed to an atmosphere of hydrogen in the u manner, the palladium black so prepared condensed no sensible quan of that gas.

I am inclined to conclude that the passage of hydrogen through a p of metal is always preceded by the condensation or occlusion of the But it must be admitted that the rapidity of penetration is not in pro tion to the volume of gas occluded; otherwise palladium would be m more permeable at a low than at a high temperature. A plate of metal was sensibly exhausted of hydrogen gas at 267°, but continued meable, and in fact increased greatly in permeability at still higher t peratures, and without becoming permeable to other gases at the s time. In a striking experiment, a mixture of equal volumes of hydro and carbonic acid was carried through a small palladium tube, of wh the internal diameter was 3 millimetres, and the thickness of the 0·3 millimetre. From the outer surface of this tube gas escaped in vacuum, at a red heat, with the enormous velocity of 1017·54 cub. cent per minute for a square metre of surface. This gas did not disturb bar water. It was pure hydrogen.

A still more rapid passage of hydrogen was observed through the s stance of a hollow cylinder of palladium 1 millimetre in thickness, higher temperature, approaching the melting-point of gold. The pa dium cylinder being enclosed in a porcelain tube charged with hydrogen, was exhausted as usual, and gave 105·8 cub. centims of ga five minutes; measured with bar. 753 millims., therm. 10°. As the exte surface of the palladium tube amounted to 0·0053 square metre, the pas of gas was

3992·22 cub. centims. from a square metre of surface, per minute

The rate of penetration of hydrogen through the same palladium t at the lower temperature of 265° C., was previously observed to be

327 cub. centims. from a square metre of surface, per minute.

The velocity of penetration thus appears to increase in a rapid ratio the temperature.

When carbonic acid was substituted for hydrogen, at the same temperature, a very minute penetration was perceived, amounting to

1·86 cub. centim. from a square metre of surface, per minute.

This gives for carbonic acid one twenty-thousandth part of the rate of hydrogen. Whether it is a penetration of the same sort, although greatly less in degree, or rather the consequence of a sensible porosity in the palladium (of which it would become the measure), remains uncertain.

The quantity of hydrogen held by the metal at these high temperatures may become too small to be appreciated; but I presume it is still present, and travels through the metal by a kind of rapid cementation. This extreme mobility is a singular property of hydrogen, which was involved in the fundamental discovery, by MM. H. Sainte-Claire Deville and Troost, of the passage of that gas through plates of iron and platinum at high temperatures.

The marked rapidity of the passage of the same gas through a thin sheet of caoutchouc appears to be more capable of explanation on known principles. Caoutchouc of less than 0·1 millimetre in thickness, if impregnated with hydrogen, loses its gas entirely by the most momentary exposure to the air. A tube of 2 millimetres in thickness, through which hydrogen and carbonic acid were singly passed, each for an hour, was found to retain—

Of hydrogen	0·0113 volume.
Of carbonic acid	0·2200 „

The absorption, then, is in the proportion of 1 hydrogen to 20 carbonic acid; but the comparative rate of penetration of the two gases through a sheet of caoutchouc is as 1 hydrogen to $2\frac{1}{2}$ carbonic acid; or the hydrogen moves eight times as rapidly as the density of its solution would indicate. But these gases differ in diffusibility as carbonic acid 1 to hydrogen 4·7. The rapid passage of hydrogen through caoutchouc is thus partly explained by the rapid manner in which that gas is brought to one surface of the sheet and conveyed away from the other by gaseous diffusion. Again, both substances travel through the substance of the caoutchouc by their diffusibility *as liquids*. Suppose hydrogen in that form to be nearly as much more diffusive than the other substance as it is when both are gaseous, then the observed rapid passage of hydrogen through caoutchouc would appear to be fully accounted for.

Liquid diffusion has also a bearing upon the rapid dissemination of hydrogen through a soft colloid metal, like palladium or platinum, at a high temperature. The liquid diffusion of salts in water is known to be six times as rapid at 100° as at 0°. If the diffusion of liquid hydrogen increases with temperature in an equal ratio, it must become a very rapid movement at a red heat. Although the quantity absorbed may be reduced (or the channel narrowed), the flow of liquid may thus be increased in velocity. The whole phenomena appear to be consistent with the solution of liquid hydrogen in the colloid metal. The "solution affinity" of metals appears to be nearly confined to hydrogen and carbonic oxide, so that metals are not sensibly penetrated by other gases than these.

III. "On the Osteology of the Solitaire or Didine Bird of the Isl of Rodriguez, *Pezophaps solitaria* (Gmel.)." By Alfr Newton, M.A., Professor of Zoology and Comparative A tomy in the University of Cambridge, and Edward Newt M.A., Auditor-General of Mauritius. Communicated by P. Sclater, Esq., M.A., Ph.D. Received May 6, 1868.

(Abstract.)

The Solitaire of Rodriguez was first satisfactorily shown to be disti from the Dodo of Mauritius (*Didus ineptus*) by Strickland in 1844, fr a renewed examination of the evidence respecting it, consisting of the count given by Leguat in 1708, and of the remains sent to France Great Britain. Strickland, in 1848, further proved it to be generica distinct from the Dodo. The remains existing in Europe in 1852 w eighteen bones, of which five were at Paris, six at Glasgow, five in p session of the Zoological Society (since transferred to the British Museun and two in that of Strickland, who, at the date last mentioned, describ them as belonging to *two* species, the second of which he named *Pe phaps minor*, from the great difference observable in the size of t specimens. In 1864 one of the authors visited Rodriguez, and there fou in a cave two more bones, while a third was picked up by a gentleman wi him. All these bones have been described, and most of them figured, the publications of the Zoological Society, and in the large work of Stric land and Dr. Melville *.

Encouraged by his former success, that one of the authors of the prese paper who had before been to Rodriguez urged Mr. George Jenner, t magistrate of the island, to make a more thorough search in its caves ; and 1865 this gentleman sent no less than *eighty-one* specimens to Mauritiu These were forthwith transmitted to London, and exhibited at a meeting the Zoological Society in that year, when it appeared that the notion pr viously entertained of there having been two species of *Pezophaps* w erroneous, and that probably the difference in size of the specimens w sexual.

News of this last discovery reached England during the meeting of t British Association at Birmingham, and, prompted by Mr. P. L. Sclate that body made a liberal grant to aid further researches. Owing several causes, the scarcity of labourers in Rodriguez being the chief, near a year elapsed before these could be begun. But in 1866, son coolies having been expressly sent thither to dig in the caves, a very larg collection of the bones of this bird, amounting to nearly *two thousand* sp cimens, was obtained. These specimens include almost all the most in portant parts of the skeleton, and furnish the authors with the material f the present paper.

* The Dodo and its Kindred. London: 1848, 4to.

This vast series of specimens shows that there was a very great amount of individual variability in the bird, so much so as to render the task of describing them minutely, and yet generally, a very difficult one. Yet, in consequence of this wealth of material, the authors have greater confidence in the opinions they declare. Professor Owen, having lately published a very detailed account of the osteology of the Dodo *, the present paper follows as closely as possible the mode of treatment he therein adopted, the authors thinking that they are so consulting the convenience of those who may wish to compare the structure of the two allied birds. Thanks to him, also, they have been able themselves to examine the very specimens which he described; and they are further indebted to many others—Mr. George Clark of Mauritius, Professors Reinhardt, Fritsch, and Alphonse Milne-Edwards, Sir William Jardine, and Mr. Flower, for valuable assistance in the shape of models or other additional material. To Mr. J. W. Clark they also mention their obligations for reconstructing from specimens in their possession the skeletons of the Dodo and of two Solitaires now exhibited.

The description of the latter follows in much detail, the amount of individual variability to which each bone was subject being specially dwelt on, and the whole compared bone by bone with that of the Dodo and also of *Didunculus*. *Pezophaps* differs from *Didunculus* quite as much as *Didus* does, but it is nearly allied to the latter. Still there are important differences. The neck was much longer than in *Didus*, and the vertebræ, on the whole, larger. The ribs also possess perhaps somewhat thicker heads and articular tubercles. The pelvis is much more rounded, and approaches that of the normal Pigeons much more than that of *Didus* does; but in its posterior portion it differs very remarkably from that of any known bird; for the pubis in *Didus* has not yet been discovered. In the sternum *Pezophaps* generally agrees with *Didus*, but has some distinctive features. This bone shows articular surfaces for four sternal ribs only, instead of five, which seems to be the normal number in *Didus*; and the posterior extremity, so far as can be judged from the imperfect condition of the specimens, is very unlike what it is in that bird; but the characters deducible from this last portion in birds generally are shown to be very inconstant. The "scapular arch" differs from that of *Didus*, its constituent portions having been apparently never anchylosed as is the normal state there, and consequently resembling in this respect those of the generality of birds. The angle made by the junction of the coracoid and scapula cannot be accurately determined, but would appear to have been not much less than what it is in *Didus*. The scapula is of very peculiar form, unlike, so far as known to the authors, that of any bird, being inclined somewhat forward, and only pointing backward at its extremity, where it becomes spatulate in shape. The coracoid exhibits, as usual in this very significant bone, some good diagnostic cha-

* "On the Osteology of the Dodo (*Didus ineptus*, Linn.)," Trans. Zool. Soc. vol. vi. pp. 49–85.

racters. It is much stouter than it is in *Didus*—a fact not so surpr when the exceedingly abnormal form it there assumes is taken into c deration. At its sternal end it differs from that of most other birds, in extension and rounding off of the outer border. Other peculiarities are also described, one of which appears to be sexual. This is the surfa which the scapula is articulated, and which in the large individuals (presu to be males) is roughly quadrate, while in the smaller ones (the supp females) it is triangular. In *Pezophaps* the bones of the wing are massive and smoother than in *Didus*, judging from such remains of latter as exist. The most remarkable thing about them, however, is presence of a bony knob on the radial side of the metacarpal, unlike wh found in any other bird. It is large in some of the specimens, supp to have belonged to old males, but very little developed in the presu females. It is more or less spherical, pedunculate, and consists of a ca like mass with a roughened surface, exceedingly like that of diseased b and was probably covered by a horny integument. It is situated mediately beyond the proximal end and the index, which last would pear to be thrust away by it to some extent. It answers most accura and most unexpectedly to Leguat's description of it:—"L'os de l'ail grosait à l'extrémité, et forme sous la plume une petite masse ro comme une balle de mousquet." A description of its structure, as as tained microscopically by Mr. J. Gedge, is added. The extremity of wing is wanting. The leg-bones of *Pezophaps*, when compared with th of *Didus*, show more strongly developed ridges and muscular impressi just the converse of what is observable in those of the wing; but the bones having been minutely and correctly described by prior authors, i unnecessary here to say much of them. Part of the skull, too, had b already described; but the only specimen then known was so incrus with stalagmite that not much could be made of it. The present rema show that it was very markedly different in many respects from that *Didus*. The cranium is narrower and longer, and without the pecu frontal protuberance of *Didus*, being nearly flat at the top, with the f and hind part elevated into two bony ridges of cancellous structure. T upper mandible also presents a remarkable difference from that of *Did* where the axes of the nasal process and the maxillary converge, wher in *Pezophaps* they diverge. The maxilla also was relatively very sma and the mandible differed by being much straighter above, showing salient angle on its lower edge (which is very inconsiderable in *Didus*), a being much more solid posteriorly. In the quadrate the two birds more alike. The rest of the bones of the head are wanting.

A comparison of the entire skeleton shows that *Pezophaps* is in so degree, and perhaps on the whole, intermediate between *Didus* and normal Columbæ, while it has some features, such as the armature of wing, quite peculiar. It has no very near affinity to *Didunculus*; indeed th form must be considered the type of a separate family, though not so ab

rant as the *Dididæ*, which must be looked upon as the most remotely connected of the order Columbæ. Strickland was amply justified in arriving at the conclusion that the Solitaire of Rodriguez was generically distinct from the Dodo; but it seems expedient to define his genus *Pezophaps* more precisely. Accordingly the following characters are assigned to it:—

Rostrum mediocre, curvatum, processu nasali et ramis maxillaribus antice divergentibus. Frons plana, porcâ osseo-cancellatâ circumdata. Ossa coracoidea robusta. Alæ breves, involatiles. Manus singulis bullis osseo-callosis armatæ. Collum et pedes longiores.

In like manner the genus *Didus* may be defined:—

Rostrum magnum, aduncum, processu nasali et ramis maxillaribusa ntice convergentibus. Frons tumida, in umbonem hypoconicum osseo-cancellatum surgens. Ossa coracoidea attenuata, scapulas obtuse attingentia. Alæ breves, involatiles. Manus inermes. Collum et pedes breviores.

The account given by Leguat of his Solitaire is then quoted in full, as also that of d'Heguerty, the latter from Strickland, and the authors proceed to remark upon the different causes of extinction of species within historic time. This, when effected by man's agency, is seldom done by man's will; and various cases are cited to support this opinion. In extirpating species man generally acts indirectly; and they succumb to forces set in motion indeed by him, but without a thought on his part of their effect. In the case of the extinction of the Solitaire of Rodriguez, the cause usually suggested seems inadequate; and the authors consider it was probably effected by feral Swine, and quote a remarkable passage from an old French Voyage, showing the extraordinary abundance of these creatures in Mauritius, where, in or about the year 1708, above *fifteen hundred* had been slain in one day. It is plain that where these abounded inactive birds could not long survive. It is supposed that the case was the same in Rodriguez as in Mauritius; for in every country newly discovered by Europeans, it has been an almost universal custom to liberate Pigs, and there is no reason to believe that the island first named was an exception thereto.

The extraordinary fidelity of Leguat's account of the Solitaire is next considered. It is borne out in every point save one, perhaps, by a study of the remains. The rugose surface at the base of the maxilla, the convexity of the pelvis, the somewhat lighter weight of the Solitaire than of the Dodo, its capacity for running, and, above all, the extraordinary knob on the wing, all agree with the description he has given us. The authors attempt also to account for the origin of this last by observing that its appearance is so exactly that of diseased bone, that it may have been first of all occasioned by injuries received by the birds in such combats with one another as Leguat mentions, and aggravated by the continuance of their pugnacity. The authors remark, also, that it is the habit of Pigeons to fight by buffeting with their pinions.

The particular in which Leguat may have erred is in the assertion, or perhaps rather inference, as to the monogamous habits of the Solitaire; and

the cause of the error (if such it be) may be ascribed, without derogating from his truthfulness, to his anxiety to point a moral, which may have led him to imagine he saw what he wished to see. He especially mentions that one sex would not fight with the other, which is just what takes place among polygamous birds. The case of a very well-known bird (*Otis tarda*), about which much has been written, is then cited, to show that even now, after centuries of observation, it is doubtful whether it be monogamous or polygamous. Leguat, therefore, may easily have been mistaken in his opinion, even setting aside his evident leaning on the matter. The notion of *Pezophaps* having been polygamous was before entertained by one of the authors, and arises from a consideration of the great difference in the size of the two sexes, which in birds is generally accompanied by polygamous habits; but the question is now not likely to be solved.

The amount of variability which every bone of the skeleton of this species presents, warrants the conclusion that as much was displayed in those parts of its structure which have perished, letting alone Leguat's direct evidence as to the individual difference in the plumage of the females. If such a process, therefore, as has been termed "Natural Selection," or "Survival of the Fittest," exists, there would have been abundant room for it to operate; and there having been only one species of *Pezophaps* might, at first sight, seem an argument against the belief in such a process. A little reflection, however, will show that such an argument is unsound. Confined in a space so restricted as one small island, every individual of the species must have been subject to conditions essentially identical in all cases. Whatever power such a process might possess, there would be neither occasion nor opportunity for its operation, so long as no change took place in the physical character of the island. But if we venture to indulge our fancy, and consider what would have been the inevitable result of a gradual upheaval of the island, and a corresponding extension of its area until it became vastly increased and its original low rounded hills were exalted into mountains, it is plain that a great variety of physical conditions would be thereby incurred. One side of the island would be exposed to the full force and direct influence of the trade-winds, the other side would be completely sheltered from them. The climate of these two portions would accordingly differ, and a great difference would be speedily wrought in the character of their vegetation, while that of the elevated central part would undergo a corresponding modification. After some longer or shorter period, we can conceive the island itself being broken up into two portions, separated from one another by a strait, such as divides the North and Middle Islands of New Zealand. This rupture would certainly tend still more to affect the existing fauna and flora; and at the end of another epoch there can be little doubt but the animals and plants of each portion, exposed to different influences, would present a decidedly different appearance, and the eastern and western islands (supposing the separation to have taken place in the direction of the meridian) might each possess its

own special form of Solitaire, as the islands composing New Zealand have their peculiar species of *Apteryx*.

But it is only in such a case as has just been imagined that considerable modifications would be likely to be effected. It therefore seems to be no argument against the existence of such a process as that of "Natural Selection," to find a small oceanic island tenanted by a *single* species which was subject to great individual variability. Indeed a believer in this theory would be inclined to predicate that it would be just under such circumstances that the greatest amount of variability would be certain to occur. In its original state, attacked by no enemies, the increase of the species would only be dependent on the supply of food, which, one year with another, would not vary much, and the form would continue without any predisposing cause to change, and thus no advantage would be taken of the variability of structure presented by its individuals.

On the other hand, we may reflect on what certainly has taken place. Of the other terrestrial members of the avifauna of Rodriguez but few now remain. A small Finch and a Warbler, both endemic (the first belonging to a group almost entirely confined to Madagascar and its satellites, the second to a genus extending from Africa to Australia), are the only two land-birds of its original fauna now known to exist. The Guinea-fowl and Love-bird have in all probability been introduced from Madagascar; but the Parrots and Pigeons of which Leguat speaks have vanished. The remains of one of the first, and the description of the last, leave little room to doubt but they also were closely allied to the forms found in Madagascar and the other Mascarene islands; and thus it is certainly clear that *four* out of the *six* indigenous species had their natural allies in other species belonging to the same zoological province. It seems impossible on any other reasonable supposition than that of a common ancestry to account for this fact. The authors are compelled to the belief that there was once a time when Rodriguez, Mauritius, Bourbon, Madagascar, and probably the Seychelles were connected by dry land, and that that time is sufficiently remote to have permitted the descendants of the original inhabitants of this now submerged continent to become modified into the many different representative forms which are now known. Whether this result can have been effected by the process of "Natural Selection" must remain an open question; but that the Solitaire of Rodriguez, and the Dodo of Mauritius, much as they eventually came to differ, sprang from one and the same parent stock, seems a deduction so obvious, that the authors can no more conceive any one fully acquainted with the facts of the case hesitating about its adoption than that he can doubt the existence of the Power by whom these species were thus formed.

IV. "Description of the great Melbourne Telescope." By T. R. Robinson, D.D., F.R.S., and Thomas Grubb, Esq., F.R.S. Received June 11, 1868.

(Abstract.)

The authorities of the colony of Victoria formed (in 1862) the wish to establish, in connexion with their new observatory at Melbourne, a powerful telescope for observing the Southern Nebulæ, and applied, through the Duke of Newcastle, to the President and Council of the Royal Society for encouragement and advice. That body had in former years given much attention to this subject, and had received a report from a Committee on it, in the hope of inducing the British Government to take such a step. The same Committee was consulted, and made another report almost identical with the first. In consequence of it, the Legislature of Victoria ordered the construction of the telescope in 1865, which was undertaken early in the following year by Mr. Grubb, under the direction of a Committee consisting of the late Earl of Rosse, Dr. Robinson, and Mr. W. De La Rue. After Lord Rosse's death, his son, the present Earl, was appointed in his stead by the President. It has been very successfully completed; and it is thought that some account of it, and the notable things that occurred in its progress, may deserve a record. As an introduction to this account, a notice of the reasons which guided the Committee in some of their decisions may be useful.

1. They chose a four-feet Reflector, because, from the experience gained with Lord Rosse's and Mr. Lassell's telescope, they were satisfied that this aperture was sufficient for the work proposed, and also because it had not been proved that a larger one could be equatorially mounted with the requisite firmness and ease of motion. This last, however, has been convincingly disposed of by the present experiment.

2. They chose the Reflector in preference to the Achromatic, because it is not probable that one of the latter will ever be made which shall equal a four-feet in light, and because, if it were, the cost of it would be tremendous. One of the Committee got from a great continental optician an estimate for one of 30 inches, which would have amounted, complete, to £20,000, and been far inferior in power. Absurd exaggeration is not uncommon in comparing these two kinds of telescopes, but is easily refuted. A speculum reflects, after years of use, 0·64 of the incident light, which is the intensity of Herschel's front view. In the Newtonian the coefficient is 0·401; in the Achromatic light is lost from two causes:—first, by reflection at each of the four surfaces of its lenses. This can be calculated from Fresnel's formula, and gives the coefficient of intensity 0·81, whence the equivalent to a four-feet Newtonian is not less than 33 inches. Secondly, it must be greater than this; for no glass is perfectly transparent, and therefore absorbs light according to a law depending on its thickness and a certain constant. Dr. Robinson discusses this, and comes to the con-

clusion that the equivalent Achromatic will certainly not be less than 3 feet, and may be much more. He adduces experimental facts in support of this, and points out the conditions which are necessary for a fair comparison of the two kinds.

3. The tube was to be metal lattice-work. This plan, originally proposed by Sir J. Herschel, had been tried by Lord Rosse, Mr. Lassell, and Mr. W. De la Rue, with marked advantage; its main purpose is to diminish the evils caused by currents of unequally heated air eddying in the tube.

4. They decided that the speculum should be metal, and not silvered glass. It seemed imprudent to risk the success of the undertaking by venturing on an experiment whose success was not assured; it was not known whether the silver could be uniformly deposited on so large a scale; some facts appear to show that glass is more liable to irregular action than speculum metal; and the intensity of the light in these telescopes is not as great as had been expected.

5. The telescope was to be a Cassegrain, not a Newtonian; and this was the result of long discussion. This form is little known, for Newton's hostility to it has created a prejudice against it. Its chief defect is the difficulty of getting a magnifying power so low that the eye can only take in the whole pencil, as the first image is magnified from five to six times by the second speculum. This requires a huge eyepiece, of which the lenses are costly, and, by their thickness, intercept light. One defect attributed to it by Newton proves to be an advantage; he thought that the reflection of metal is like that of glass, faintest at perpendicular incidence, but increasing in intensity with the obliquity; therefore brighter in his telescope at 45° than in the other nearly perpendicular. But it is now known that the law is different, so that the reflection at 45° is $\frac{1}{20}$ less powerful than the other. The chief advantage of the Cassegrain, and that which influenced the Committee, is its extreme convenience to the observer; he is near the ground, and has to move through a small space to command the whole sky, instead of standing on a structure nearly 40 feet high, which cannot be used without fatigue and even danger.

Then formulæ are given for finding the constants of the telescope; the foci of its specula are 366 and 75 inches, its lowest power 240, and its extreme field of view $14'{\cdot}3$.

The composition of the specula is Lord Rosse's, four equivalents of copper and one of tin, respecting which much detail is given; and still more of the process of casting the first speculum, which took place July 3, 1866. This was managed according to the method of Lord Rosse, with some modification, caused by the necessity of having a projecting band round the edge of the speculum, and an aperture in its centre. It was conducted without accident, and safely transferred to the annealing-oven, in which a thermocouple of platinum and iron was inserted, which, at the end of twenty-four days, showed that it was completely cooled. It came

out perfectly sound, but was a little "in winding," and had Lord Rosse's "Crowsfeet" on part of its surface. These might have been ground out, but Mr. Grubb preferred recasting it. It was broken by an iron ram of 70 lbs. falling four feet. When the blows were applied through a bar of wood, they did nothing; but the first one through iron broke it into four equal pieces. These, when put together, fitted exactly, showing that there was no unequal shrinkage in cooling. Some changes were made before the second casting, on Sept. 22. The hoop-bed of the mould was much strengthened; the core, which formed the central aperture, and seemed to interfere with the regular flow of the metal, was raised $1\frac{1}{2}$ inch above the mould; and this last set on a strong cast-iron frame, which could be inclined at pleasure. It was sloped, so that at the first part of the pouring the melted alloy filled it from its lower edge to its middle; it was then lowered rapidly to be horizontal while the pouring was completed. When it came from the oven, the central disk, which was under the core, was cut out. This, as well as the third casting, which was made November 24, came out perfect.

A description is given of the polishing machine, which is remarkable for its smooth and equable action, and not less so for the facility it gives of testing the figure of the speculum during the polishing. Removing the polisher (which is built up of wood, so as to combine great strength and lightness), one man, by turning a winch, sets the speculum upright. Doors are opened, and by an eyepiece properly placed a dial or artificial star at a suitable distance is examined. The specula were ground flat at their back, and true at their edges; and their focal lengths differ only $1\frac{1}{2}$ inch.

Not less important than the perfect figure of a speculum is its being so supported both at the back and edge that it may be subjected to no irregular pressure, for to such it is almost inconceivably sensitive. The arrangements to secure this are very elegant, and have proved entirely successful.

The lattice of the tube consists of steel bars $\frac{1}{3}$ thick, and, on average, $2\frac{1}{2}$ broad, forming openings of 17 by 9; it is very light, and so stiff that 1 cwt. produced only a deflection $\frac{1}{200}$ inch at 20 feet. The mode of supporting the small specula is described, and the mechanism by which the moving of them for focal adjustment is made easy to the observer. One of these specula is peculiar; it is an achromatized lens, whose coincident surfaces are cemented; its front one so curved that there can be no false image formed by it, its fourth coated pretty thickly with silver. This will be more permanent than the metal, and is expected to give more light.

The equatorial is worthy of the telescope. By inverting the usual arrangement of the polar axis, the eyepiece, circles, and centre of gravity of the whole are brought near the ground; each axis is provided with three sets of counterpoises; the polar with two, which relieve its pivots of lateral pressure; the third lightens the end pressure of the lower one. Those of the declination axes act, two in its plane parallel and perpendicular

to it, the third parallel to the polar axis. They are so effective, that 5 lbs. at a leverage of 20 feet turns the polar axis; 12½ the declination one. A man can raise the telescope from the horizon to the zenith in 20 seconds; two (as both axes must be turned) can reverse it from the east side of the pier to the west in 45 seconds.

The telescope is moved in right ascension by a sector and screw driven by a very effective clock. The regulator of this is so powerful, that an addition of 2 cwt. to its driving-weight only makes it gain six seconds in the hour.

The micrometer has an original mode of illuminating its lines in a dark field, which has been found very suitable for nebulæ.

The spectroscope is on the usual plan, but with special provision for the permanence of its adjustments.

The instrument is also provided with a photographic apparatus, nearly like Mr. De la Rue's celebrated one, which (the small speculum being removed) is placed at the focus of the great speculum. A few trials made with an extemporized one gave pictures which that gentleman considered to be of great promise.

A high opinion is expressed both of the optical and astronomical powers of the instrument.

June 18, 1868.

Lieut.-General SABINE, President, in the Chair.

Prof. Clifton, Dr. J. Barnard Davis, Dr. Duncan, Dr. Pettigrew, Mr. Stone and Mr. Vaux, were admitted into the Society.

The following communications were read:—

I. "A Contribution to the Knowledge of Persulphide of Hydrogen." By A. W. Hofmann, LL.D., F.R.S. Received May 25, 1868.

This remarkable body was first observed by Scheele, and subsequently examined by Berthollet; our knowledge of this substance is, however, more especially due to Thenard, who, soon after the discovery of peroxide of hydrogen, was led also to investigate what was believed to be the corresponding sulphur-compound *. The composition of persulphide of hydrogen has nevertheless remained doubtful. Thenard points out that the specimens analyzed by him, contained variable quantities of sulphur, but always more than would have been met with in a sulphur-compound corresponding to peroxide of hydrogen †.

* Ann. de Chim. et de Phys. vol. xlviii. p. 79.

† Thenard states that all his analyses yielded more than 4 atoms of sulphur for 1 molecule of sulphuretted hydrogen.

If, nevertheless, several modern authors have not hesitated to represent the composition of persulphide of hydrogen by the formula

$$H_2 S_2,$$

with or without a sign of interrogation, their statements are no longer based upon the secure foundation of experience.

Of late peculiar circumstances have again directed the attention of chemists to this remarkable compound. Among the technico-chemical aspirations which the Paris Exhibition has brought to light, none have been received with more satisfaction than the efforts, manifested in a variety of forms, of resuscitating for the purposes of industry the quantities of sulphur buried in the mountains of soda-waste, which accumulate in the neighbourhood of our factories. Chemists have more especially admired the processes by which M. Schaffner on the one hand, and Messrs P. W. Hofmann and P. Buquet on the other, have endeavoured to solve this problem. In certain phases of the reactions utilized for this purpose, enormous quantities of persulphide of hydrogen are frequently produced; and only lately, when visiting the chemical works at Dieuze, where the sulphur is regenerated on a colossal scale, the author of this Note has had an opportunity of experimenting with many kilogrammes of this interesting sulphur-compound.

Under these circumstances the author has examined with great interest a compound which he discovered by accident, and the analysis of which appears to throw some light on the composition of the persulphide.

On adding a cold saturated solution of strychnine in strong alcohol to an alcoholic solution of polysulphide of ammonium, brilliant crystalline spangles soon begin to appear in the liquid, and after twelve hours, the walls of the vessel are covered with beautiful orange-red needles, frequently attaining the length of a centimetre, which, after the removal of the mother-liquor, have to be washed only once or twice with cold alcohol, in order to render them perfectly pure. The crystals are insoluble in water, alcohol, and ether, also in bisulphide of carbon; indeed I have not yet found a solvent from which they could have been recrystallized.

Analysis has led to the formula

$$C_{21} H_{24} N_2 O_2 S_3 = C_{21} H_{22} N_2 O_2, H_2 S_3.$$

Hence the crystals are a compound of 1 mol. of strychnine with 1 mol. of a persulphide of hydrogen, of the composition

$$H_2 S_3.$$

Indeed the strychnine-compound splits up in the sense of the above conception. In contact with concentrated sulphuric acid the orange-red crystals are decolorized, and on addition of a small quantity of water, colourless, transparent, oily droplets of persulphide of hydrogen are separated, sulphate of strychnine remaining in solution. The oily drops remain unaltered for some time, but are ultimately decomposed into sulphur and sulphuretted hydrogen.

The investigation of this sharply defined strychnine-compound, which can be preserved for months without undergoing any decomposition, goes far to prove the existence of a persulphide of hydrogen,

$$H_2S_3;$$

it is, however, by no means improbable that compounds of hydrogen and sulphur in several proportions may exist.

The formation of the strychnine-compound which I have described, and which I have often prepared with the same result, could not fail to lead to an examination of several other alkaloids in a similar direction. Quinine, cinchonine, brucine, and several other vegetal bases were repeatedly submitted to the action of an alcoholic solution of polysulphide of ammonium, but in no case were similar phenomena observed.

The compound of strychnine with persulphide of hydrogen is remarkable for its insolubility. An alcoholic solution containing 2·03 grs. of strychnine, when mixed with an alcoholic solution of polysulphide of ammonium and allowed to stand for twelve hours, was found to have deposited 2·287 grs. of the red crystals, *i. e.* 87·2 per cent. of the theoretical amount. It deserves to be examined, whether the property possessed by strychnine, of forming so insoluble a compound with persulphide of hydrogen, could not be utilized for the purpose of preparing this alkaloid, and in certain cases even for its detection and separation from other substances with which it might be mixed.

II. "Note on the Anatomy of the Blood-vessel System of the Retina of the Hedgehog." By J. W. Hulke, F.R.S. Received May 26, 1868.

(Abstract.)

This retina is very remarkable for the fact that all the arteries and veins lie upon the inner surface of the membrana limitans interna retinæ, in intimate relation with the membrana hyaloidea; while capillaries only traverse the limitans, receiving a sheath from it, and penetrate the inner layers of the retina. The hedgehog's retina is in this respect a link between the non-vascular retina of fish, amphibia, reptiles and birds, and the vascular retinæ of most mammals.

III. "Researches on Refraction-Equivalents." By J. H. Gladstone, Ph.D., F.R.S. Received May 29, 1868.

Since the paper of the Rev. T. Pelham Dale and myself "On the Refraction, Dispersion, and Sensitiveness of Liquids"*, our researches have been continued from time to time, and a good deal of attention has been paid to the subject in Germany. The permanence of the specific refractive

* Philosophical Transactions, 1863, p. 317.

. . .gest generalization arrived at is
a compound is the sum of the refraction-
This has been sufficiently proved in a mu
compounds of carbon, hydrogen, and oxyg
assumed to be the case, in the combinati
bodies*.

My more recent researches have branchec
especial attempt has been made to answer t
any of the elements more than one defini
what are the refraction-equivalents of the
mass of observations bearing on these points
and more or less collated, but it is yet imperf
rather to indicate the principal method of i
actual results.

As the metals are opaque, their refractive in
in a direct manner as those of gaseous hydrog
tallized carbon, and other transparent element
must therefore be made to determine their ef
examining their compounds; but their crystall
doubly refracting, owing to some peculiarities
where they give only one spectrum, there are
the experiment that are not encountered in
The solutions of these salts have only one refra
that they might afford an easy means of detern
lents, first, of the compounds themselves, and
and other constituents. In practice, many
themselves, all of which tell upon the ulti...
tated improved ...

[redacted]

and the refractive index and density of the solution were taken. From these was reckoned the refraction-equivalent, and subtracting from this *n* times the refraction-equivalent of water for the solar line A, there remained the refraction-equivalent of the dissolved salt for that part of the spectrum. That this fairly represents the action exerted on light by the chemical compound itself is supported by several considerations. 1st. In the few cases, such as chloride of sodium and sugar, where the refraction-equivalent of the substance has been obtained, both in the solid and dissolved condition, it is found to be the same. 2nd. Solutions of several organic substances, such as formic and citric acids, give the theoretically correct equivalent for these substances. 3rd. The refraction-equivalent of a salt seems to be the same, whether it be dissolved in water or in alcohol. 4th. The refraction-equivalent of a salt in solution is not affected by altering the amount of water in which it is dissolved. 5th. The numbers reckoned for these salts in solution bear such a remarkable relation to one another as to force the conviction that they are made up of two components, the one depending on the metal, the other on the substance combined with it. To exhibit the nature and force of this argument, it would be necessary to tabulate a long series of results; but for the present I shall confine myself to the salts of potassium and sodium with the corresponding hydrogen compounds.

Dissolved compound.	Common formula.	Refraction-equivalents.			Difference between potassium and sodium compounds.	Difference between potassium and hydrogen compounds.
		Potassium compound.	Sodium compound.	Hydrogen compound.		
Chloride	MCl	18·44	15·11	14 44	3·3	4·0
Bromide	MBr	25·34	21·70	20·63	3·6	4·7
Iodide	MI	35·33	31·59	31·17	3·7	4·2
Cyanide	MNC	17 12	......	......	......	......
Sulphocyanide	MSNC	33·40	......	......	......	......
Nitrate	MNO_3	21 80	18·66	17 24	3 1	4·5
Metaphosphate	MPO_3	....	19·48	18·68	......	......
Hydrate	MHO	12·82	9·21	5·95	3·6	6·8
Alcoholate	MC_2H_5O	27·68	24·28	20·69	3·4	0·8
Formate	$MCHO_2$	19·93	16·03	13 40	3·9	6·5
Acetate	$MC_2H_3O_2$	27·65	24·05	21·20	3·6	0·5
Tartrate	$M_2C_4H_4O_6$	57·60	50·39	45·18	3·6	6·2
Carbonate	M_2CO_3	34·93	28 55	.	3·2	......
Sulphate	M_2SO_4	30·55	26·20*	22·45	2·2	4 1
Bichromate	$M_2Cr_2O_7$	79·9	72·9	......	3·5	......
Hypophosphite	$M_2PH_2O_2$	26·94	20·93	......	3·0	......

From the above numbers several conclusions may be safely drawn. First,

* This number seems to be too high, but it is the mean of fairly accordant results. It is rejected in the calculation of average difference between potassium and sodium.

it is evident that the refraction-equivalents of the compounds of pota differ very widely according to the nature of the electro-negative stituents; again, that the refraction-equivalents of the compoun sodium differ *pari passu* with those of the potassium compounds, always less by a number varying from 3·0 to 3·9. From this it m fairly concluded that the electro-negative constituent has the same effe light, whichever metal it is united with, and that the refraction-equiv of potassium exceeds that of sodium by 3·4, or thereabouts. But do Table afford the data for determining the absolute equivalent of c other of these metals? It was at first thought that this would be a at by a comparison of the metal with hydrogen, the refraction-equival which has hitherto been estimated at 1·3 (or 1·5 in the case of water) the last column shows that the difference between potassium and hyd is not always the same, the differences being greater than can be attril to errors of observation. Indeed the numbers seem to fall int groups: with the mineral acids the differences lie between 4·0 and while with water, alcohol, and the organic acids, they are always up of 6, varying indeed from 6·2 to 6·8, the average being 6·55. But it these last-mentioned compounds that the equivalent of hydrogen is bel to be 1·3. Assuming this, we may reckon the refraction-equivale potassium to be about 6·55+1·3, that is, 7·85. We have, how other means of arriving at an estimation. Chlorine, in such bodi chloroform or tetrachloride of carbon, is represented by 9·8. A cyanogen, from the experiments of Dulong on the gas itself, may be tak 9·2 Sulphur has a refraction of 16·0; hence sulphocyanogen ma reckoned as 16·0+9·2, that is, 25·2. Subtracting these numbers those of the respective potassium salts, we obtain the equivalent o metal. Thus from different sources we may calculate for the val potassium:—

From the chloride	8·6
„ cyanide	7·9
„ sulphocyanide	8·2
„ hydrate	8·3
„ alcoholate	8·1
„ formiate	7·8
„ acetate	7·7
„ tartrate	7·5

These numbers are tolerably close, though the equivalent of potas deduced from its inorganic, would, on the whole, be higher than deduced from its organic compounds. The mean of the first four co tations is 8·2, that of the last four 7·8. Perhaps, pending furthe searches, it will be best to assume the mean of these numbers,

Potassium ....=8·0;

and since a sodium salt has a refraction-equivalent generally 3·4 lower than the corresponding potassium salt, we may reckon

Sodium...................... $=4{\cdot}6$*.

If instead of taking the refraction-equivalent $P\,\frac{\mu-1}{d}$, we reckon the specific refractive energy $\frac{\mu-1}{d}$, we obtain the following values:—

Potassium	0·205
Sodium	0·200

This implies that equal quantities of these two analogous metals exert very nearly, if not precisely, the same effect on the velocity of the rays of light.

Another deduction from the above Table is that already alluded to in regard to hydrogen; while in the organic acids it probably has the known refraction-equivalent 1·3, it would seem that in the others, viz. hydrochloric, hydrobromic, hydriodic, nitric, metaphosphoric, and sulphuric acids, it has a very much higher refraction-equivalent, one in fact which is little less than sodium, and falls short of potassium by only about 4·3. Hence we may we deduce—

Hydrogen in organic compounds	$=1{\cdot}3$
Hydrogen in mineral acids..........	$=3{\cdot}7$

How far this conclusion may hold good throughout, and whether one number should be an exact multiple of the other number, must remain to be determined by future observations. It appears, however, to answer in the affirmative the question whether an element can have more than one definite refraction-equivalent.

Of course, from the Table given above, it would be easy to deduce values for each of the electro-negative constituents; but it would be safer to generalize from a larger number of instances.

The series of observations on potasisum and sodium salts are the most complete and the most carefully revised of any which have yet been made; but if their refraction-equivalents are fixed, it becomes a much simpler matter to determine those of most other metals. Thus, of lead the nitrate and acetate have alone been examined; but as the refraction-equivalents reckoned for these salts differ from those of the corresponding potassium compounds in each instance by 4·1, the presumption is great that the refraction-equivalent of lead is very near $8{\cdot}0+4{\cdot}1$, or 12·1.

The following are numbers deduced from two or more salts of each metal. They must be looked upon only as approximately true, and any subsequent modification of the value 8·0 for potassium, must lead to a corresponding modification of the whole series.

* Haagen, from the crystalline chloride, determined the refraction-equivalent of sodium for the hydrogen line *d* at 4·89.

Metal.	Chemical equivalent.	Refraction-equivalent.	Specific refractive energy.
Potassium	39	8·0	0·205
Sodium	23	4·6	0·200
Lithium........................	7	3·9	0·557
Magnesium	12	3·7	0·308
Barium	68·5	7·8	0·114
Strontium	43·8	6·5	0·148
Calcium........................	20	5·2	0·260
Zinc	32·6	4·8	0·147
Nickel	29·5	5·1	0·173
Cobalt	29·4	5·2	0·177
Lead	103·5	12·1	0·117
Mercury	100	9·8	0·098
Ammonium	18	11·4	0·633

These numbers are suggestive in many ways; but I will only rem the very high refractive energy of lithium, the practical identity of nic and cobalt, and the remarkable fact that the specific refractive energy the metals are (with one or two exceptions) in the inverse order of th atomic weights.

IV. "A Third Memoir on Skew Surfaces, otherwise Scrolls." Prof. Cayley. Received May 30, 1868.

(Abstract.)

The present Memoir is supplementary to my "Second Memoir on Sk Surfaces, otherwise Scrolls," Phil. Trans. vol. cliv. (1864) pp. 559–5 and relates also to the theory of skew surfaces of the fourth order, or quar scrolls. It was pointed out to me by Herr Schwarz, in a letter dated Hal June 1, 1867, that in the enumeration contained in my Second Mem I have given only a particular case of the quartic scrolls, which have directrix skew cubic; viz. my eighth species, S $(1, 3^2)$, where there is al a directrix line. And this led me to observe that I had in like mann mentioned only a particular case of the quartic scrolls with a triple dire trix line; viz. my third species, S $(1_3, 1, 4)$, where there is also a simp directrix line. The omitted species, say, *ninth species*, S (1_3), *with a trip directrix line*, and tenth species, S (3^2), with a directrix skew cubic, a considered in the present Memoir; and in reference to them I develope theory of the reciprocal relations of these scrolls, which has some ve interesting analytical features.

The paragraphs of the present Memoir are numbered consecutively wi those of my Second Memoir above referred to.

V. "Transformation of the Aromatic Monamines into Acids richer in Carbon.—III. On Menaphtylamine." By A. W. Hofmann, LL.D., F.R.S. Received June 1, 1868.

The transformation of naphtaline into its carboxylic acid suggests the existence of a large number of compounds which the progress of science cannot fail to realize. It is not my intention to examine in detail this group of substances, the composition and even the properties of which are sufficiently indicated by theory. There are nevertheless several terms of this series which I must not leave unprepared whilst engaged with this question. These are the aldehyde, the alcohol, and the monamine of the series. It is the latter substance of which I beg permission to submit to-day a short account to the Royal Society.

My first attempts to produce the aromatic monamine were anything but successful. Cyanide of naphtyl, when left in contact with zinc and sulphuric acid, even for weeks, was found to yield but trifling quantities of menaphtylamine. The greater portion of the nitrile was left unchanged, while more or less, by the absorption of the elements of water, was converted into menaphtoxylamide and even into menaphtoxylic acid. A slight modification, however, of the process usually adopted has removed these difficulties.

It is well known that M. Mendius, after he had discovered the remarkable property possessed by *nitriles* of fixing two molecules of hydrogen, has submitted also the *amides* to the action of hydrogen *in conditione nascendi*, in the hopes of replacing their oxygen by hydrogen, and of producing also in this manner the primary monamines. These experiments have not been successful. In the presence of the difficulties attending the preparation of menaphtylamine, the idea suggested itself of trying whether the sulphuretted amide of the series into which the nitrile is so easily transformed would not be more readily attacked by nascent hydrogen than the nitrile itself. The result of this experiment was highly satisfactory. On submitting an alcoholic solution of menaphtothiamide to the action of zinc and hydrochloric acid, torrents of sulphuretted hydrogen are at once evolved. The addition of zinc and hydrochloric acid, and sometimes also of a little alcohol, is continued, until, after a day or two, the disengagement of sulphuretted hydrogen almost ceases. The liquid is now mixed with concentrated soda until the precipitate of hydrate of zinc, which is formed in the commencement, is redissolved. An oily layer containing much soda and alcohol is seen to separate and to collect on the surface of the aqueous solution. This layer is removed and heated in the water-bath until the alcohol is volatilized. An aqueous liquid is thus produced, on which a yellow oil is floating. The latter is principally menaphtylamine, which is still mixed with a small quantity of cyanide of naphtyl regenerated from the thio-compound. The oil is treated with dilute hydrochloric acid, the hydro-

chloric liquid separated by filtration from the cyanide, and decompos hydrate of sodium, when the base separates in a state of purity.

Menaphtylamine is a very caustic liquid, boiling between 290° and Freshly distilled it is colourless, but soon acquires a yellow tint. I tracts carbonic acid with such avidity that it is impossible to pour it one vessel into another without a pellicle of the difficultly soluble carb being formed on its surface.

The composition of the base was sufficiently indicated by theory; i peared nevertheless desirable to establish it experimentally by the an of the hydrochlorate and the platinum-salt.

The hydrochlorate crystallizes with the greatest facility in diffi soluble needles, containing

$$C_{11}H_{12}NCl = \left.\begin{matrix} C_{11}H_9 \\ H \\ H \end{matrix}\right\} N,HCl.$$

The yellow crystalline precipitate which is formed by the additi perchloride of platinum to the hydrochlorate has the composition

$$C_{22}H_{24}N_2PtCl_6 = 2[(C_{11}H_9)H_2N, HCl], PtCl_4.$$

The transformation of the thio-compound into menaphtylamine is seen simply to consist in the substitution of 2 atoms of hydrogen for 1 of sulphur:—

$$C_{11}H_9NS + 2HH = C_{11}H_{11}N + H_2S.$$

I have but little to say about the properties of menaphtylamine; ne theless the extraordinary crystalline tendencies of its salts deserve t mentioned. The sulphate and nitrate are likewise difficultly soluble; latter crystallizes in splendid nitrelike prisms. In contact with bisulp of carbon menaphtylamine solidifies at once to a crystalline mass. W treated with alcoholic soda and chloroform, it is converted into the terr smelling *formomenaphtylnitrile,* which I propose to examine somev more in detail.

I have also prepared *benzylamine,* starting from thiobenzamide instea benzonitrile. The experiment is of course likewise successful, the adv tage, however, less conspicuous, since benzonitrile fixes hydrogen with greater facility than the cyanide of naphtyl.

Be this, however, as it may, the facility with which hydrogen *in co tione nascendi* acts upon sulphur-compounds deserves to be noticed. propose to examine in the direction indicated by the above experiments s of the more important sulphur-compounds, more especially the thio-a of the fatty and aromatic series, and the two groups of sulphocyanic eth The investigation of the latter, indeed, has already furnished me result great neatness and precision.

In conclusion, I must not leave unmentioned that, since my first c munication on the menaphtan series, I have had an opportunity of remo the slight doubts respecting the identity of the acid obtained by the ac

of oxalic acid upon naphtylamine with that procured by treating a naphtalin-sulphate with cyanide of potassium. M. V. Merz had found the fusing-point of the latter acid to be 140°, whilst for the former I had observed the fusing-point 160°. M. O. Olshausen has since prepared in my laboratory a quantity of cyanide of naphtyl according to Merz's process. The acid obtained from this cyanide by treatment with an alkali, thrice recrystallized and finally purified by distillation, was likewise found to fuse exactly at 160°. Menaphtoxylamide, procured from the same source, exhibited the fusing-point 203°, while the compound I had formerly examined fused at 204° *. The identity of the acids obtained by the two processes is thus satisfactorily established.

VI. "Account of some recent Observations on Sun-spots, made at the Kew Observatory." By Warren De La Rue, Esq., F.R.S., Balfour Stewart, Esq., F.R.S., and Benjamin Loewy, Esq., F.R.A.S. Received June 2, 1868.

(Abstract.)

The authors, after reviewing briefly the two theories on the nature of suu-spots, which are still subjects of dispute, refer to the stereoscopic views obtained and the results published in their 'Researches on Solar Physics,' and state the reasons which have led them to believe that sun-spots are cavities and at a lower level than the sun's photosphere. Their opinion has been recently strengthened by observations of a sun-spot on the 7th of May, which in disappearing produced in two successive photograms indentations in the west limb.

After proving by the measurements made, which, with the calculations, are appended to their paper, that there can be no doubt about the identity of the heliographical elements of the previously observed spot and the successive indentations, they prove from the established details of the phenomena of sun-spots that such indentations must under all circumstances be very rare occurrences, and state fully the conditions favourable to the recurrence of similar observations, inviting observers to give their particular attention to them.

VII. "The Formation and Early Growth of the Bones of the Human Face." By George W. Callender, Lecturer on Anatomy at St. Bartholomew's Hospital. Communicated by James Paget, F.R.S. Received June 2, 1868.

(Abstract.)

These notes refer to some few points with which we are as yet imper-

* The fusing-point of this substance is, by misprint (Proc. Roy. Soc. vol. xvi. p. 302), stated to be 244°, instead of 204°.—A. W. H.

fectly acquainted, such as the growth of the maxilla, and the formation and subsequent obliteration of the intermaxillary bones. As they are simply a narration of facts, it is almost impossible to give a satisfactory abstract of them.

After brief consideration of the arrangement of the cartilages for the ethmoid and turbinate bones and for the septum of the nose, an account is given of the appearances observed in a human fœtus four-tenths of an inch long, noticing the relations of the maxillary lobes, of the parts which represent the palate, and showing that the opposite sides unite from before backwards to form the palate, the soft palate remaining ununited in a fœtus one inch and five-tenths long. The superior maxilla is described, before ossification has commenced, in a fœtus nine-tenths of an inch long. Although ossification begins in this bone at many distinct points, the rapidity with which the separate ossifications are fused make it undesirable to name each as a distinct centre. The palatal and alveolar portions are formed somewhat later than the remainder of the bone. In a fœtus two inches and three-tenths long the bone consists of a nasal process, deeply grooved on its inner surface, of an incisor process, which has not hitherto been accurately described, of orbital and of palatal-alveolar portions; the infraorbital fissure is distinctly marked, and a deep notch shows the situation of the canine socket.

The principal changes which are noticed during the growth of the bone result from the thickening and marking of the nasal process, the formation of the nasal groove and of the inner and outer walls of the antrum, the variations in the relative size of that cavity, the deepening of the sockets for the teeth, the formation of the septa dividing them, and the changes in the orbital plate. The order and method of these changes are separately narrated.

The ossification of the vomer is shown to commence from its inferior and anterior parts, whence also ossification extends to the intermaxillary bones, which are formed in the membrane bounding the anterior nares. Each of these bones acquires its greatest independent size in a fœtus about four inches and a half long, and then consists of a nasal process, which eventually fuses with the corresponding portion of the superior maxilla, its apex assisting to form the ridge for the turbinate bone, and of plates which strengthen the incisor portion, and form part of the walls of the incisor sockets. In a fœtus six inches and seven-tenths long the apex alone is distinct, and in a fœtus nine inches long the bone is lost in the upper maxilla, although the fissure along its posterior margin still outlines its shape. The bone grows in the fissure between the incisor process and the palate, and is shut off from the face by the first-named and by the nasal process. Distinctly outlined at the close of the fourth month, it is joined to the superior maxilla during the latter part of the fifth or beginning of the sixth month.

Some facts are related respecting the formation of several of the remaining

bones of the face, and that of the inferior maxilla is specially referred to. This bone is beginning to ossify in a fœtus nine-tenths of an inch long, and is distinctly formed in a fœtus one inch and one-tenth long. Each half may be said to grow from four centres, formed (1) by the cartilage which tips the condyloid extremity, (2) by the layer of membrane in front of Meckel's cartilage, (3) by the ossification of the anterior extremity of Meckel's cartilage, (4) by deposits of bone in the perichondrium of the anterior and middle thirds of the same cartilage, from which is derived the plate of bone which forms the base of the dental canal.

After giving the measurements of the angles formed between the ascending ramus and the body of the bone, and after referring to the subdivisions of the groove for the teeth, the growth of the mylo-hyoidean ridge is described, as well as the ossification of the anterior extremity of Meckel's cartilage, the latter forming a triangular block beneath the incisor sockets, to the twist acquired by which the prominence of the front of the maxilla, known as the mentum or chin, appears to be due. In a fœtus four inches and seven-tenths long, the block of bone formed in the anterior extremity of the cartilage of Meckel is still clearly defined.

VIII. "On a Method of making a Direct Comparison of Electrostatic with Electromagnetic Force; with a Note on the Electromagnetic Theory of Light." By J. Clerk Maxwell, F.R.SS.L. & E.

Received June 10, 1868.

(Abstract.)

The experiments described in this paper were made in the laboratory of Mr. Gassiot, who placed his great battery of 2600 cells of bichloride of mercury at the disposal of the author. Mr. Willoughby Smith lent his resistance-coils of 1,102,000 Ohms; Messrs. Forde and Fleeming Jenkin lent a sensitive galvanometer, a set of resistance-coils, a bridge, and a key for double simultaneous contacts; and Mr. C. Hockin undertook the observation of the galvanometer, the adjustment of the resistances, and the testing of the galvanometer, the resistance-coils, and the micrometer-screw. The electrical balance itself was made by Mr. Becker.

The experiments consisted in observing the equilibrium of two forces, one of which was the attraction between two disks, kept at a certain difference of potential, and the other was the repulsion between two circular coils, through which a certain current passed in opposite directions. For this purpose one of the disks, with one of the coils attached to its hinder surface, was suspended on one arm of a torsion-balance, while the other disk, with the other coil behind it, was placed at a certain distance, which was measured by a micrometer-screw. The suspended disk, which was smaller than the fixed disk, was adjusted so that in its position of equilibrium its surface was in the same plane with that of a "guard-ring," as

in Sir W. Thomson's electrometers, and its position was observed by means of a microscope directed on a graduated glass scale attached to the disk. In this way its position could be adjusted to the thousandth of an inch, while a motion of much smaller extent was easily detected.

An exactly similar coil was placed at the other end of the torsion-balance, so as to get rid of the effects of terrestrial magnetism.

It was found that though the suspended disk and coil weighed about half a pound, a very slight want of equality between the opposing forces could be detected, and remedied by means of the micrometer.

The difference of potential between the disks was maintained by means of Mr. Gassiot's great battery. To measure this difference of potential, it was made to produce a current through Mr. Willoughby Smith's resistance-coil, and the primary coil of the galvanometer shunted with a variable resistance.

The current in the coils was maintained by a Grove's battery, and was led through the secondary coil of the galvanometer.

One observer, by means of the micrometer-screw, altered the distance of the disks till the suspended disk was in equilibrium at zero. At the same time the other observer altered the shunt, till the galvanometer-needle was also in equilibrium. The micrometer reading and the resistance of the shunt were then set down as the results of the experiment.

The mean of twelve satisfactory experiments, at distances varying from ·25 to ·5 inch, gave for the ratio of the electromagnetic to the electrostatic unit of electricity—

$$\begin{aligned} v &= 27{\cdot}79 \text{ Ohms, or B. A. units.} \\ &= 277{,}900{,}000 \text{ metres per second.} \\ &= 174{,}800 \text{ statute miles per second.} \end{aligned}$$

This value is considerably lower than that found by MM. Weber and Kohlrausch by a different method, which was 310,740,000 metres per second. Its correctness depends on that of the B. A. unit of resistance, which, however, cannot be very far from the truth, as it agrees so well with Dr. Joule's thermal experiments.

It is also decidedly less than any estimate of the velocity of light, of which the lowest, that of M. Foucault, is 298,000,000 metres per second.

In a note to this paper the author gave his reasons, in as simple a form as he could, for believing that the ratio of the electrical units, and the velocity of light, are one and the same physical quantity, pointing out the difference between his theory and those of MM. Riemann and Lorenz, which appear to lead to the same conclusion.

IX. "Results of Examination of Southern Nebulæ with the Spectroscope." By Lieut. J. HERSCHEL. Communicated by Prof. G. G. STOKES, Sec. R.S. Received June 1, 1868.

No. 1179. April 9, 1868. [Great nebula in Orion.] All three lines seen remarkably well, and measured as follows:—

$$\left.\begin{matrix}4{\cdot}63\\4{\cdot}66\\4{\cdot}67\end{matrix}\right\}4{\cdot}65\,;\ \begin{matrix}4{\cdot}77\\4{\cdot}88\end{matrix}\left.\vphantom{\begin{matrix}4\\4\\4\end{matrix}}\right\}4{\cdot}82;\ 5{\cdot}24;\ D=2{\cdot}46.$$

[*Note.*—$b=D+1{\cdot}67$, $F=b+1{\cdot}06$.]

No. 1225. April 9. ["Planetary; pretty bright; very small; very little extended; barely resolvable?"] Not found.

April 4. Seen; faint, but unmistakeable. Not seen in spectroscope, though most carefully placed and intently looked for.

No. 1565. March 30. ["Planetary; pretty bright; pretty small; extremely little extended; barely resolvable; $3''{\cdot}75$ diam."] Not seen.

April 18. Seen, but not considered bright enough: some doubt about identity.

April 23. What was seen on the 18th looks more than ever doubtful: a faint haze in a cluster.

No. 1567. April 23. ["Planetary; considerably bright; not very well defined."] Linear spectrum,

$$4{\cdot}66,\quad D=2{\cdot}50$$

No. 1783*. ["Remarkable; planetary; pretty bright; =star 9 mag.; very small; round."] Looked for *three times*, but not seen.

No. 1801. ["Remarkable; planetary; pretty bright; pretty large; round."] Looked for *twice*, but not seen.

No. 2008. April 12. ["Very bright; large; very much extended, 45°."] Well placed, but not seen with spectroscope; focus suspected, but not enough to prevent lines being seen.

April 23. Easily visible in telescope. I am almost certain I must have seen this one if it has a linear spectrum. Continuous spectrum suspected.

No. 2017. March 31. ["Very remarkable; planetary; very bright; very large; little extended; star 9 mag. in the middle; $4''{\cdot}0$ diam."] Slight haze and moonlight. Found with difficulty in spectroscope: examination interrupted. A continuous streak with blotch of light $\frac{2}{5}$ from the red end.

No. 2102. April 4. ["Remarkable; planetary; very bright; little ex-

* 1783. May 4. Seen as a monochromatic light—faint, but beyond doubt. Rough measurement, $D+2{\cdot}0$.

tended; 32ˢ diam.; blue."] Found at once, and seen in finder. Not *very* well seen in spectroscope: third line not seen at all.

$$D+\frac{2\cdot14}{2\cdot16}=D+2\cdot15.$$

No. 2197. March 26. ["η Argus and great nebula."] Spectrum easily visible; did not, however, succeed in separating the lines (low altitude.) Estimated position $D+1\cdot8\pm\cdot3$.

No. 2581. April 2. ["Planetary; remarkable; small; round; blue; equal star 7 mag.; 1ˢ·5 diam."] Well seen in spectroscope: eventually a short sharp bright line was seen, with a fainter companion close by. $D+2\cdot1\pm\cdot1$.

No. 2917. March 26. ["Very bright; large; round; barely resolvable."] Not seen in spectroscope.

April 13. Not seen in spectroscope, though every precaution was taken to secure direction and focus. It is almost impossible that this can have a bright line spectrum, or it must have been seen.

April 23. Seen at once; bright; all three lines easily seen, and a fourth suspected, at intervals 1 ; 3 : 10 ? from the first by estimation.

$$D+\frac{2\cdot10}{2\cdot19}=D+2\cdot15.$$

No. 3021. April 13. ["Very bright; large; round."] Not seen in spectroscope. (See remark of this date for No. 2917.)

No. 3092. April 23. ["Very bright; considerably large; pretty much extended, 63°."] Not seen in spectroscope.

No. 3128. April 2. ["Cluster; large; extremely rich; very compressed; irregularly round; well resolved; star 12 red."] Not seen in spectroscope (adjustments suspected).

April 12. Not found in spectroscope. (Remark essentially the same as for 2917.)

No. 3132. April 12. [Remarkable; very bright; very large; extremely extended, 92°."] No spectrum seen, though carefully placed.

April 26. Continuous spectrum suspected: linear spectrum considered out of the question. (NB. Clear night, and other spectra well seen.)

No. 3525. March 25. ["Very remarkable; very bright; very large; very much extended, 122°; bifid."] An irregularly shaped nebula with dark space across it. No spectrum seen: faint stellar spectrum seen.

March 26. Looked for again: no result.

April 26. A good instance of the extreme difficulty of finding a continuous spectrum object. I was enabled to find this one by the help of a neighbouring star, whose spectrum was

easily found; but even in the field the faint light could only just be recognized.

No. 3531. March 25. ["Very remarkable; cluster; ω Centauri."] A large cluster visible to the eye: spectrum continuous.

No. 4066. April 29. ["Planetary; very small; round; quite sharp.] (April 5, not seen.) Recognized in telescope as a small round planetary nebula: seen without much difficulty in spectroscope as a small luminous point ill defined on the more refrangible side.

$$D = 2{\cdot}55\,;\ (\pm{\cdot}1) + \left.\begin{array}{r} 4{\cdot}60 \\ {\cdot}70 \\ {\cdot}63 \end{array}\right\} = D + 2{\cdot}1 \pm {\cdot}1.$$

No. 4083. April 5. ["Very remarkable; globular cluster; very bright; large; extremely compressed in the middle."] Seen in telescope as a slightly oval nebulous ball not very bright (? moonshine). Found with difficulty in the spectroscope: a faint continuous spectrum of considerable width: no lines.

No. 4173. April 5. ["Very remarkable; globular cluster; very bright; large; well resolved."] Seen easily in telescope: looked for two hours in vain with spectroscope.

April 13. Spectrum continuous; compared its appearance with that of a small star close by to make sure; difference quite marked.

No. 4183. April 13. ["Cluster; well resolved."] Spectrum clearly continuous.

No. 4238. April 13. ["Remarkable; globular cluster; very bright; very large; irregularly round; well resolved."] A faint continuous spectrum certainly seen, but too faint for more than recognition; stellar spectrum seen involved.

No. 4284. April 5. ["Very remarkable; planetary; pretty bright; very small; round."] Not seen in telescope.

April 17. Not seen in spectroscope, though pretty certainly placed.

April 24. Linear spectrum seen, though too faint for measurement, even for absolute *certainty* of its character. The light seen, however, was too strictly LOCAL to belong to a continuous spectrum of so faint an object, but that is the whole of the evidence.

No. 4302. April 17. ["Remarkable; annular nebula; pretty bright; small; round."] (April 6, not seen in telescope.) Not seen in spectroscope, though pretty certainly placed.

April 24. No result. A faint object in telescope (200), but of some size. No spectrum, though satisfactorily placed.

No. 4355. April 24. ["Very remarkable; very bright; very large; trifid; double star involved."] A very large object with a double

2 R 2

star in the central patch (sketched). This star was frequently in the spectroscopic field, but no lines were seen: a haziness suspected.

April 26. Continuous spectrum: readily found owing to the central star.

No. 4361. April 17. ["Very remarkable; very bright; extremely large; extremely irregular figure; with large cluster."] A large nebulous area visible in finder; not examined with telescope; spectrum linear, but feeble. D + 1·98. Some doubt about the reading of D.

No. 4390. April 16. ["Planetary; very bright; very small; round; little hazy."] Seen in spectroscope as a short bright line with a second fainter one; a third suspected. Measurement D + 2·34.

No. 4403. April 26. ["Very remarkable; bright; extremely large; extremely irregular figure."] I should say this is as bright an object as any of the larger nebulæ I have seen. It is a striking object (detailed account of appearance and sketch), and the various parts could be recognized as they were brought on the slit. Measurements, D being read by reflected light from soda-flame:—

$$\left.\begin{array}{l}\left.\begin{array}{l}4{\cdot}53\\4{\cdot}58\end{array}\right\}4{\cdot}56 \quad D=2{\cdot}48\\ \;\;4{\cdot}59 \qquad\quad 4{\cdot}59 \quad\;\; =2{\cdot}44\end{array}\right\}D+2{\cdot}10.$$

No. 4628. April 26. ["Remarkable; planetary; very bright; small; elliptic."] Easily seen in telescope, and *confidently* looked for in spectroscope. Spectrum as bright and distinct as any yet seen; lines measured *by obliteration* with cross-wires in a dark field, D being obtained from a reflected soda-flame.

4·61. 4·81, 5·16, D=2·41,

or D+2·20, +2·40, +2·75.

No. 4510. May 3. ["Planetary; bright; very small; round."] I am rather surprised to find this described as "bright, very small;" I should have expected very bright, pretty large. Its spectrum is the first in which I have suspected a new character. In the first place, there is no trace of a third line, and the second is more uncertain, as though there were more than one fainter companion. The brightness of the principal line is considerable, making measurements by estimation behind the wires in a dark field not very difficult. The following measures are, I believe, trustworthy: $D=\left\{\begin{array}{l}3{\cdot}32\\ \;\;{\cdot}32\end{array}\right\}2{\cdot}32$, Neb. line $=\left.\begin{array}{l}4{\cdot}62\\ \;\;{\cdot}66\end{array}\right\}4{\cdot}64$.

In the second place, therefore, here is a reliable measurement, differing widely from the rest (too widely, as I think,

for an accident), but agreeing closely with one other (4390), in which the third line is only "suspected." Both must of course be remeasured. This is the first planetary nebula I have seen in the "finder." Can it have changed its character since?

[*Note.*—With four exceptions, Nos. 1843, 2076, 1565, 1801 (all between 7^h & 11^h R.A.), the *whole* of the bright planetary nebulæ between 80° and 150° N.P.D. have now been examined.

Abstract of Measurements.

No. 1179	D+2·19	+2·36	+2·78
1567	+2·16		
2102	+2·15		
2197	+1·8		
2581	+2·1		
2917	+2·14		
4066	+2·1		
4361	+1·98		
4390	+2·34 (!)		
4403	+2·10		
4407	+2·08		
4628	+2·20	+2·40	+2·75
4510	+2·32 (!)		
General mean	2·10	2·29	2·67
b=D+	1·67	1·67	1·67
b	+·43	·62	1·00
F=*b*+1·06			

Supplementary List.

No. 4450. May 4. ["Globular cluster; very large; very little extended; well resolved."] A faint continuous spectrum barely visible.

No. 4543. May 4. ["Globular cluster; bright; pretty large; round; partially resolved."] A decided continuous spectrum brighter in the middle. No trace of lines.

No. 4678. May 4. ["Globular cluster; very remarkable; bright; very large; well resolved."] A decided continuous spectrum of visible width. No trace of lines.

May 5, 1868.

The above were observed this morning half an hour before despatch.

[The spectra of the following nebulæ have been described by Mr. Huggins. See Phil. Trans. 1864, p. 439, and 1866, p. 383, and Proceedings of the Royal Society, vol. xiv. p. 40.

No. 2102	No. 4628
No. 4238	No. 4510
No. 4403	No. 4678.—G. G. S.]
No. 4390	

X. "Notes on Variations in the origin of the Long Buccal Branch of the Fifth Cranial Nerve." By Wm. Turner, M.B. (Lond.), Professor of Anatomy, University of Edinburgh. Communicated by Dr. Sharpey. Received June 9, 1868.

In the Journal of Anatomy and Physiology, November 1866, I gave a description of a specimen from the dissecting-room in which the long buccal nerve, instead of proceeding from the third division of the fifth nerve, arose from the superior maxillary trunk in the spheno-maxillary fossa.

This transposition of the origin of the nerve from its proper trunk to one which is sensory in function, seemed to me to be a strong additional argument, and from a new point of view, to those which had previously been urged by various writers on physiological and pathological grounds, in favour of the purely sensory nature of this nerve.

In a subject dissected during the month of March of the present year, variations of an interesting kind in connexion with this nerve were observed, which afford additional proof of its sensory nature.

The occurrence within so short a period of two subjects presenting variations of so important a nature in their bearings on the much disputed question of the physiology of this nerve, leads me to offer these notes to the Royal Society, in the hope that, if inserted in its Proceedings, the attention of anatomists may be more generally directed to the matter, and lead perhaps to further observations of a similar nature.

When the superior maxillary trunk was exposed on the left side of the head in the spheno-maxillary fossa, in addition to the orbital and palatine nerves usually arising from it in this locality, a branch of some size arose, which descended behind the posterior surface of the superior maxilla. In its course it gave origin to two distinct superior dental nerves, which entered foramina in the bone for the supply of the upper teeth. The upper of these dental nerves was joined immediately before entering its foramen by another superior dental branch, arising independently from the superior maxillary trunk. After giving origin to these superior dental nerves, the branch, now considerably diminished in size, passed downwards to the cheek, where some of its fibres pierced the substance of the buccinator muscle at the spot where it is usual for the long buccal nerve to enter it, whilst others formed a distinct anastomosis with branches of the portio dura in the fat over the surface of that muscle. No buccal nerve from the inferior maxillary was observed on this side. This arrangement corresponds almost exactly to the specimen above referred to, as described in the Journal of Anatomy and Physiology, and the two are, I believe, the only examples of the kind up to this time recorded.

On the right side of the same subject a different arrangement was observed. From the superior maxillary trunk in the spheno-maxillary fossa, in addition to its proper branches, two slender nerves arose, which passed obliquely downwards and forwards through the mass of fat beneath the ramus

of the jaw and the anterior border of the masseter to the fat of the cheek, where they anastomosed with branches of the portio dura, and doubtless ended in the integument. These nerves represented, I believe, the branches which the long buccal nerve usually gives off before it enters the buccinator muscle, and were the only parts of that nerve which arose on this side of the head from the second division of the fifth. The remaining part of the long buccal nerve arose from the inferior maxillary trunk and entered the buccinator in the usual locality. When followed into the substance of the muscle, it passed obliquely and gave off branches of great delicacy, many of which were traced into the mucous membrane of the cheek. Followed upwards to its origin, the long buccal nerve was seen to divide at the foramen ovale into two parts, of which one was traced without difficulty directly into the Gasserian ganglion, and must therefore be regarded as sensory; the other, connected with the fasciculus, from which the temporal and masseteric nerves arose, was followed upwards to the motor root of the fifth. Almost immediately after receiving this offshoot from the motor root, the buccal nerve gave origin to the nerve of supply for the external pterygoid muscle, and the fibres of the motor root were to all appearance prolonged directly into this pterygoid branch, whilst the fibres from the sensory ganglion could be distinctly traced into the proper buccal part of the nerve.

There can be no doubt that in this case the entire buccal nerve on the left side was purely sensory. There can also be no doubt that those branches arising from the superior maxillary trunk on the right side, which passed to the surface over the buccinator, were purely sensory. The remaining part of the right nerve also, though connected with the motor root of the fifth, yet parted to all appearance with its motor fibres before it proceeded to its destination.

I may take this opportunity of referring to a case of variation in the origin of the buccal nerve, which, so far as I know, has not yet been referred to by British anatomists, and which gives additional evidence of the sensory nature of the nerve. In the Bulletins de la Soc. Anat. de Paris, 1853, S. 109 (quoted in Krause und Telgmann 'Die Nerven-Varietäten,' Leipzig, 1868), M. Gaillet describes the nerve as arising directly from the Gasserian ganglion, without having any connexion with the motor root, then passing out of the cranium through a special foramen midway between the F. rotundum and F. ovale, and lying between the great wing of the sphenoid and external pterygoid muscle on its course to its distribution.

Variations in the usually described arrangements of the structures in the human body have, as a rule, been studied either from their bearings on questions connected with practical medicine and surgery, or from the light which they throw on the development and morphology of parts and organs, but, as these cases prove, their study is not without interest from the teleological point of view.

XI. "Description of a Pendulum-Electrograph now in use at the Melbourne Observatory." By R. L. J. Ellery, Government Astronomer to the Colony of Victoria. Communicated by Balfour Stewart, Esq., F.R.S. Received May 23, 1868.

(Plate III.)

About eighteen months since, being desirous of making some improvements in our mode of observing atmospheric electricity, I tried a series of experiments with Sir William Thomson's various methods of obtaining observations and measures of the electric state of the atmosphere; the results of these were so very satisfactory that I had a divided-ring reflecting electrometer made, as nearly similar as possible, judging from the descriptions available, to those used by himself. This was attached to a water-dropping collector, and I obtained the deflections of the needle measured by means of a telescope and reflected scale. Prior to this, all observations of atmospheric electricity were made with Quetelet's modification of the Peltier electrometer, where the needle and its little directing magnet are suspended by a cocoon fibre instead of on a point. The use of this, however, was very troublesome, involving its being carried to the highest part of the building at every observation, brought down, and placed on its stand within doors, the needle brought to rest by a magnet, and, after reading off the force of repulsion, the ascertaining of the character of the electricity by a separate operation. After using the divided-ring electrometer for a few weeks, it became apparent that no method of observing atmospheric electricity that was not continuous could possibly afford results that would embrace the numerous and rapid changes which take place. I found also in observing with the divided-ring electrometer that the torsion of the platinum wire was uncertain, requiring very frequent alteration of the zero-point, especially after great disturbances.

I therefore endeavoured to devise an electrograph that should act by gravity instead of torsion; and for this purpose the electrometers of Sir W. Thomson and the photographic registration method adopted in the Kew magnetographs afforded me a sufficient groundwork. In August last I so far succeeded as to obtain regular photographic curves of the electric condition of the air about 18 feet from the earth's surface; experience of the first temporary apparatus suggested modifications; and in November an improved instrument was erected, of which a brief description was read before the Royal Society of Victoria in December. Some defects in the performance of this, however, led to even a further modification; and since December last the improved electrograph has performed most satisfactorily.

Plate III. will explain its construction. It may be generally described as consisting of—

1. Reservoir of electricity.
2. The pendulum.
3. Electrodes.

4. Charge-measurer apparatus.
5. Lifting-cradle.
6. Outer metallic cover.
7. Charging-apparatus.

1. *Reservoir of Electricity.*—On an octagonal slab of slate 1 inch thick and 15 inches from side to side, is fixed an ordinary Leyden jar, A; to the inside bottom of this jar, and in connexion with its inside coating, is fixed a pillar of brass tube reaching to about the level of the mouth of the jar; to the top of this is fixed the support (*b*) for the moveable part with its mirror. Connected with the pillar, and projecting upwards and outwards beyond the top of the jar, is a branch of metal terminating in a ring (*c*), to facilitate charging the Leyden jar.

The support of the moveable part will be best understood by reference to the Plate, where it is marked *d*; it consists of a Z-shaped piece of brass attached to the top of the pillar with a central screw and *three set screws*, by means of which the surface (*e*) upon which the moveable part rests may be set quite level. The surface (*e*) is of polished hardened steel about 1 inch long and ¼ inch broad, dovetailed into the brass Z-piece.

2. *The Pendulum.*—The moveable or sensitive part (P), which may be called the pendulum, has its centre of gravity and point of suspension nearly coincident. It consists, first, of a mirror-frame (*f*) and knife-edge (*g*) with counterpoise (*h*), needle (*i*) with its counterpoise (*j*) and balance-screw (*k*). The mirror, a piece of silvered parallel glass about 1 inch square, is clamped on to its frame by two little clips; the knife-edge (*g*) is of hardened steel, and is fixed as nearly as possible in a plane with the silvered surface of the mirror; the counterpoise brings the centre of gravity of the whole nearly to the centre of the knife-edge.

The needle is a piece of No. 16 aluminium wire fixed to the back of the mirror-frame, projecting vertically upwards for about 4 inches from the level of the knife-edge, and terminating in a piece of thin sheet aluminium (*a*) about 1¼ inch long and ½ inch wide, bent to a curve of 4 inches radius and fixed to the wire at right angles to the plane of the mirror, and with the chord of its curvature at right angles to the wire itself. The counterpoise to this is found at *j*; a small brass wire, screwed, projects downwards from the frame and carries a small weight, which can be adjusted by screwing up and down the wire so as to obtain the required sensitiveness of the pendulum. The balance-screw (*k*) is a piece of small brass wire screwed its whole length, terminating at each end in a little capstan head. It is attached at right angles to the frame and perpendicular to the plane of the mirror by means of a small stud (*l*), through which it can be screwed in order to adjust the whole pendulum to the required verticality.

3. *Electrodes.*—Firmly fixed to the slate base are two stout pillars of brass (*m, m*), ¾-inch diameter and 20 inches high; these are connected at the top by a bridge of stout brass (*n*), to the centre of which a block of ebonite carrying the electrodes (*o, o'*) is firmly screwed. These consist of two seg-

outer metal case (to be described) is a slidin
which passes a steel wire (s) terminating insid
brass 1 inch diameter. This is so arrang
pushed free through the sliding-tube and fi
nite distance from the mica disk; while a fu
made with the sliding-tube and screw, both of
graduated to inches, the tube and screw togeth
The large disk, when withdrawn, rests agains
6 inches from the needle. The screw is a fou
to the inch, and is sufficiently long to allo
sliding-tube.

5. *Lifting-Cradle.*—This consists of a cr
apparatus of a good chemical balance. It
points; one is a V, a prolongation of the k
formed by the arms of the balance-wire; the
of ebonite (t) projecting from a tube (u) whic
which can be moved up and down to the req
passing upwards through the bridge. This s
through the top of the outer case. The cradl
adjusting screws, so that it can be made to lift
trically, and lower it without setting it vibrati

6. *The outer metallic cover.*—x x x is a cy
high and 12 inches diameter, covered at the t
brass at the bottom which is ground flat and
slate slab; three studs find their places in t
keep it in position. In front of the mirro
cylinder, which is closed by a piece of parallel

the top of the cylinder are two tubular apertures lined with plugs of ebonite, through which the external parts of the electrodes pass. The key for lifting the pendulum is also fitted on the top of the case.

7. *The charging-apparatus.*—This consists of an ordinary electrophorus, and of a rod of wire covered with gutta percha or ebonite, terminating at one end in a brass knob, at the other in a projection of uncovered metal for placing in contact with the ring which projects from the Leyden jar pillar.

The air inside the cylinder is kept perfectly dry by two leaden trays, containing lumps of pumice-stone saturated with sulphuric acid*.

The arrangement of light, cylinder for photograph-paper and clockwork, as also of the reading-telescope and scale, are in all respects similar to that adopted for the vertical-force magnetograph of Kew, and described in the Report of the British Association for 1859.

The water-dropping apparatus is of the same kind as described by Sir W. Thomson in Nichol's Cyclopædia (1860, art. "Atmospheric Electricity"). The cistern contains about twenty-eight gallons, which is found to give the requisite stream for about thirty hours. It is a copper vessel, 2 feet square and 1 foot high. Keeping it shallow avoids much alteration of head of water, and consequently secures a more regular stream: this tank rests upon four ebonite insulators indoors. A copper pipe ending in a fine nozzle passes through a hole in the glass of the window, and projects to about four feet beyond the wall of the building; the tank is connected with the electrometer in another chamber by means of a copper wire very thickly covered with gutta percha.

The mode of using the whole apparatus may be thus describod. The Leyden jar or reservoir is first charged by a few good sparks from the electrophorus; to do this the cover of the opening (y) is removed and the charging-rod inserted so that the bare end rests on the ring (c). The sparks from the electrophorus are then passed to the brass knob, which projects a few inches outside the case. The rod is then withdrawn and the opening closed. The act of charging generally sets the pendulum oscillating; it soon comes to rest, however. An hour after charging I have generally found the charge sufficiently permanent for commencing registration. The two electrodes are then connected with the earth; in a minute the reading of the reflected scale will give what may be styled *charge-zero*, which is always different from the *zero-reading before charging* the jar, the latter being the reading of the position of rest of the pendulum (which, by the bye, I make so sensitive as to vibrate about once a second). The charge has now to be measured in terms of the slide and screw. To do this, the wire is first pushed in till the disk is at some definite distance from the pendulum-disk; to admit of this being done precisely and quickly, a

* It is necessary that the sulphuric acid be quite pure, for nitric acid is often an impurity in commercial samples, which soon corrodes all the parts. To avoid this, it is best to heat the sulphuric acid well in a platinum dish and drive off all the nitrous fumes.

mum will be found to have shifted, sligh
and rendering it weak and undefined; a ge
pendulum, however, by means of the *lifting*
right immediately.

The air-electrode (p) is now disconnecte
connected; the needle then assumes a slightly
five minutes is read off by the telescope and
This position becomes also photographed, a
undisturbed line on the sheet. At the end
leading to the water-dropper is attached and t
hours; at the expiration of this period the ta
nutes after the earth-reading taken and entere
trodes are then connected and the charge me
readjusted for the second day's curve, scale-re
again, and so on for the next twenty-four hours

The curves show at the commencement and
straight line (see specimen) already referred to,
mirror's position for the earth-reading; a line d
of these gives the zero or base-line for the day
the jar, which is assumed to be proportional to
for; for the line will be inclined to the edge of
the loss in the twenty-four hours. In the case
charge of the jar being positive, and the front el
nected with the air, a positive charge will raise
and a negative one lower it; so that the curve al
positive, and below it negative electricity.

It now remains to show how th

charge-measuring disk is now approached to the pendulum-disk until a sufficient deflection is caused to alter the scale-reading (say, 10′) from the zero, and the indication of the slide-tube and screw noted as *charge-reading*. The electrodes are now disconnected from the earth and connected with the 12-cell battery, one with one pole and one with the other, and the deflection then read off; the poles are then reversed and a second reading obtained. The *charge-reading* is now obtained again, as well as the *zero-reading*.

The following actual observations will further elucidate this:—

	Scale-reading.	Screw-reading.
Zero	96·5	
Charge	..	940
Battery P	101·6	
Battery N	90·4	
Charge	..	940
Zero	96·2	

The reading for battery in first positions differs from zero by +5·1 divisions, and in the second by −5·8 divisions. A deflection of 5·45 divisions is therefore equal to twelve Daniell's cells when the charge-reading is 940. The charge is now reduced and the same process is gone through. This is repeated for eight or ten degrees of charge; and by this means we obtain a set of readings of the amount of deflection caused by a 12-cell battery for various states of the charge, as well as arbitrary measures of the charge by the screw- and slide-tube.

One set of observations gave the following results:—

When the reading of the charge-measurer (with the disks near enough to produce a deflection of 10′) is	Twelve cells Daniell's battery causes a deflection of pendulum of
1·00	0·06 scale-divisions.
2·00	0·76 ,,
3·00	1·39 ,,
4·00	1·90 ,,
5·00	2·35 ,,
6·00	2·80 ,,
7·00	3·32 ,,
8·00	3·96 ,,
9·00	4·80 ,,
10·00	5·89 ,,
11·00	7·30 ,,
12·00	9·09 ,,

The distances between the disks when the charge-measurer reads 1·00 and 12·00 were 1·16 inch and 3 inches respectively. From this a Table is computed, in which the value of the scale-reading and of the ordinates to

the curves in terms of the Daniell's cells is shown for every value of the charge of the reservoir.

This apparatus has scarcely been in use for a sufficiently extended period to allow of any reliable deductions being made as to the existence of laws in the variations of the force of atmospheric electricity, or of any relations that may exist between this and other meteorological phenomena. Some facts, however, can be already gathered from the curves obtained. These are, first, the periods of maxima and minima, which are most distinctly shown. The former occurs at from $6^h\ 30^m$ to $8^h\ 30^m$ A.M., the latter at from 1 P.M. to 3 P.M. A second maximum at from 9 to 10 P.M., and a second minimum at from midnight to 2 A.M. are also indicated.

The greatest disturbances take place during our northerly winds, especially in dry hot weather, when a very great negative tension often occurs, so strong as to be beyond the measuring-power of the instrument, in which case sparks may be generally obtained from the air-electrode. The usual turning of the wind from N. to S.W. is always accompanied for a short period by an almost equally high positive tension, but during strong and hot northerly winds the high negative tension lasts as long as the wind blows strong and dry.

XII. "Further particulars of the Swedish Polar Expedition." In a Letter addressed to the President, by Prof. A. E. NORDENSKIÖLD. Communicated by the President. Received June 12, 1868.

Stockholm, June 7th, 1868.

SIR,—I had last night the honour of receiving your letter, and hasten to express my gratitude for the offer of some magnetical instruments. As an able *élève* of Prof. Edlund, Dr. Lemström, will join the expedition, exclusively for studying the meteorology and terrestrial magnetism of these remote regions, I hope that these instruments will be often and advantageously employed. But *the expedition will start from Gottenburg the 1st July, or from Tromso the 9th July.* The boxes can be addressed to *Count Ehrenward, Gottenburg,* or *Consul Aagaard, Tromso.* Excepting myself and two officers of the Navy (Capt. Baron v. Otter and Lieutenant Palander), the expedition will consist of—

Doctor Malmgren, " Smitt, " Nyström, Mr. Holmgren,	} Zoologists.
Doctor Fries, " Bergren,	} Botanists.
Doctor Lemström,	"Physiker."

A Geologist, a Conservator, and about 20 Mariners.

Almost all the zoologists and botanists will, however, return in September, with a ship hired for the purpose, and the remaining party will try to go further northward, west, or eastward, from the north-west part of Spitzbergen, where the expedition can obtain a sufficient depôt of English coal. We will also try to employ the brown coal of King's Bay; but I fear this supply is not to be relied upon.

XIII. "An attempt to apply Chemical Principles in explanation of the Action of Poisons." By W. H. Broadbent, M.D. Communicated by Dr. F. Sibson, F.R.S. Received June 18, 1868.

(Abstract.)

The starting-point in the inquiry has been the two following postulates:—

1. That there must be some relation between the substance administered and the animal organism, on which the effects depend.

2. That, so far as the substance is concerned, the basis of the relation can only be its chemical properties, using the term in its widest sense.

From these postulates follow certain corollaries:—

1. That the physiological and therapeutic action of the same substance must be similar in kind.

2. That the action of food, remedies, and poisons must be capable of explanation on the same principles.

3. That substances chemically allied should have similar physiological and therapeutic actions, or any diversity found to exist should be capable of explanation on chemical grounds.

The second of these deductions is taken as a guide in the present inquiry. Something is known as to the uses of the various classes of foods in the economy, and of the mode in which they subserve these uses; this knowledge may be applied in the endeavour to ascertain the mode of action of poisons.

The operations taking place in the animal organism may be divided into two great classes:—(α) for maintenance of structure, (β) for evolution of force. While mutually interdependent, they are distinct, and in character essentially antagonistic, structural and chemical elaboration on the one hand, oxidation or disintegration on the other.

The two great classes of food, organic and mineral, are in close relation with these. The organic foods build up the tissues, but ultimately undergo oxidation and yield force. The inorganic foods take a subordinate part in the composition of the textures; they do not yield force by oxidation, but they influence the nutritive processes. So also the organic remedies and poisons affect the evolution of force, mineral substances the organic processes.

(The action of mineral matters has been noticed elsewhere.)

The force evolved in the animal organism takes the form of heat, motion, and nervous action; but there are very important points of difference between heat on the one hand and nervo-muscular action on the other, both as to the part they take in the vital processes, and in the conditions of their evolution.

It is through their action on the nervous system that the powerful organic poisons destroy life; and in order to understand this action, it is necessary to consider closely the evolution of nerve-force, and to endeavour to realize the chemical conditions implied.

In the first place, the source of nerve-force is oxidation, and the seat of the oxidation is the nervous structures. This is generally admitted, and seems to be conclusively established by an analysis of the phenomena observed in experiments with a prepared frog's leg.

This admitted, it is to be noted—

1. That nerve-action is intermittent and of varying intensity, and that in addition to the presence of the oxygen brought to the nerve-structures by the blood, an impulse from without, or from some other part of the nervous system, is necessary to determine the evolution of the force.

2. Again, there is a storing up of potential energy in the nervous structures; witness the necessity for sleep, &c.

3. A due supply of oxygen is required. The phenomena of asphyxia show that the demand is most urgent in the hemispherical ganglia.

These facts indicate that the constituent of the nervous structures by oxidation of which the force is yielded, possesses what I have ventured to call chemical tension, a property which does not belong to non-nitrogenized matter, or to all nitrogenized matter. It will be further explained later; for the present, it is sufficient to refer to nitroglycerine as an extreme example.

The protagon of Liebreich, and the neurine recently identified by Wurtz, with hydrate of trimethyl-oxethyl-ammonium, have this characteristic in a certain degree.

Turning now to the poisons which kill by their powerful action on the nervous system. They all contain nitrogen, and all possess chemical tension; and these seem to be the only points common to the entire group.

Nitrogen cannot be the poisonous element; it has no great chemical energy, and it is present in large proportion in substances which are inert. It is nevertheless the pivot on which the deadly influence turns. Its affinity for H, O, and especially for C, is only feeble. When, therefore, in a molecule containing C, H, and N, or C, H, N and O, the elements are not so arranged that the mutual affinities of C, H, and O cooperate to maintain the integrity of the molecule; there may be a more or less powerful tendency on the part of C, H, and O to rearrange themselves without regard to the N, or to combine with O or H_2O if presented. This is what is meant by the term chemical tension. In the example given, nitroglycerine $\{C_3H_5(NO_2)_3O_3\}$, the dislocation is of O from N in favour of C and

H. Equally striking examples of dislocation of N from C or H cannot be given, and it is not easy in all cases to point out the source of the tension. A very important method in which the balance of affinities is deranged and the condition of tension brought about, is by departure from a stable type, as, for instance, in the nitrite bases, which are residues derived from ammonium-salts by dehydration. To this class belong morphia, strychnia, brucia, and most poisonous alkaloids. Additional interest is given to these compounds by the fact that Dr. Crum Brown and Dr. Fraser have shown that, by introduction into the molecule of methyl-iodide, carrying back the constitution a step in the direction of the ammonium type, the poisonous effects are greatly diminished, and entirely altered in character.

Let the deduction as to the evolution of nerve-force be accepted, and we have in the introduction into the blood of substances having varying degrees and directions of tension an intelligible method of influencing its manifestations.

Looking now upon nerve-action as a result of oxidation, in the various methods by which this oxidation may be influenced, analogies may be traced with conditions which affect ordinary combustion. These conditions are—

1. The supply of oxygen.

2. The character of the combustible.

3. The presence of products of combustion, or of bodies having a similar influence.

It is of course necessary to bear in mind the peculiarities of the oxidation yielding nerve-force, the differences between combustion and oxidation in the moist state, and the special modifying conditions of the animal organism. For example, while in asphyxia the deprivation of oxygen arrests all nervous action, the respiration of undiluted oxygen does not intensify it, either because the blood will only take up a certain proportion of oxygen, or more probably because the effects of the O are expended in altering the blood, which is thus oxidized instead of being oxygenated.

The analogies to the above conditions found in the action of substances on the nervous system are—

1. The liberation in the nascent state in the nervous structures of C and H, which appropriate the O brought by the blood, and so produce a result equivalent to the exclusion of O. The C and H are set free by the dislocating influence of N, and the example of this mode of action is furnished by prussic acid.

The converse of this, the liberation of O by a similar process, is not likely to occur, as O is never present in an organic body in excess of the proportion which would fully oxidize the other elementary constituents.

2. The analogy to the influence on the energy of combustion by the

character of the combustible, is found in the introduction into the blood of substances having chemical tension, holding different relations to the tension of the nervous matter.

3. The action of anæsthetics on the nervous system furnishes a strict parallel to the influence of CO_2 on combustion.

The rationale here given as to the action of anæsthetics is, for the purposes of the present paper, taken as established by the late Dr. Snow. Objections which have been made to it are capable of removal by experiments and considerations which need not here be adduced.

Considerable importance is attached to the establishment of the explanation here given of the action of prussic acid. Stated more explicitly, this explanation is, that the prussic acid is carried by the blood to the nerve-centres; that under the influence of the affinities thus brought to bear upon it (affinities which normally determine the oxidation by which nerve-force is evolved), its elements are dislocated from each other, and the C and H liberated in the latent condition appropriate the O destined for the evolution of nerve-force which is thus arrested.

This explanation is suggested by the composition of hydrocyanic acid, H Cy. Cyanide of potassium KCy, again, is used as a powerful reducing agent in chemical processes. Its liability to the change which will permit its elements to exercise their individual affinity for O, is indicated by its spontaneous decomposition in water, by its position as a nitrile (formio-nitrile). Corroborative evidence that it is by means of such a change that it acts, is furnished when the elements are held together by some supporting affinity, as in ferro-cyanogen. But the best example is in sulphocyanogen and hydrosulphocyanic acid, which of themselves are poisonous (*i. e.* cannot resist the dislocating influences), but, reinforced by a base, are innocent. [A parallel to this is seen in aniline, which is poisonous, and in sulphate of aniline, which is not. See Lond. Hosp. Reports, Dr. Letheby.]

The phenomena by poisoning by prussic acid are perfectly consistent with this view. All observers have noted their similarity to those of asphyxia.

Still more striking is the fact that artificial respiration, and especially with oxygen, is the great means of neutralizing the effects of this poison.

Probably this chain of facts would be considered conclusive, were it not that the hypothesis as to the mode of death by H Cy is paralysis of the respiratory movements. This hypothesis, however, still leaves unexplained the cause of the paralysis itself, and therefore the real mode of action of the poison. It is, moreover, inconsistent with certain of the phenomena; the respiratory nerve-centres are actually the last to be paralyzed, except those concerned in the action of the heart.

Experiments nevertheless have been made for the purpose of ascertaining whether the previous respiration of undiluted oxygen would retard, or in

any measure prevent the action of prussic acid. The results have been by no means uniform; but instances have occurred (rats being the animals used) in which, after the injection of an overwhelming dose, the fatal effect has been delayed quite beyond the operation of accidental causes; and again, in which a dose fatal to two rats, and barely survived, after a long train of symptoms, by one other, produced comparatively little effect on another after the respiration of oxygen.

It has been found also that a proportion of prussic acid diffused in equal volumes of air and of oxygen, has a decidedly less powerful action on the animal in the latter case.

With frogs the results were most contradictory and embarrassing, till it was discovered that prussic acid injected under the skin had scarcely any action on them. But if they were subsequently placed under a glass shade, or in some other confined atmosphere, into which the acid diffused, it would gradually affect them. It seems probable that the affinities in operation in the nervous structures of the frog are not sufficiently energetic to determine the decomposition of the H Cy, which will then act upon this animal as an anæsthetic.

Nitroglycerine was at first made the subject of experiment, under the idea that possibly oxygen might be evolved from the NO_2, which is substituted for three equivalents of H in the typical molecule. Subsequent reflection showed that this is not likely to occur; but the fact remains, that it is a substance liable to change, and very highly charged with oxygen, as compared with the ordinary constituents of the body.

It is a very powerful poison, having, however, entirely different effects on frogs and rats.

In frogs it very speedily induces powerful tetanic convulsions (a single drop of a solution of one pint of nitroglycerine in four parts of methyl in alcohol placed on the back of a frog is followed in five or eight minutes by stiffness of movement, and in thirteen to sixteen minutes by most violent spasms). In rats an hour or more elapses before any symptom is manifest, and then death is by a gradually increasing feebleness of movement, in two or three hours, without convulsion.

It is unquestionable that this difference in the effects has a relation with the oxygen contained in the nitroglycerine. The contrast with prussic acid in the action on warm and cold-blooded animals is suggested.

A very extended and comprehensive inquiry, both as to the conditions in the nervous system associated with convulsions, tetanus, delirium, &c., and into the relations and constitution of the poisons which give rise to these symptoms, is necessary before the second analogy can be followed out with any confidence. Experiments are being made with substances of known composition and constitution, with a view to elucidate this part of the question.

In conclusion, two points are considered which cannot be passed over in

any attempt to apply the principles of physical and chemical science to the case of poisons.

The first is as to the minuteness of the fatal dose. Any explanation, before it can be accepted, must show that the cause is adequate to produce the effect. This is a difficulty in the path of any rational explanation. It is attempted to meet it by showing, on the one hand, that the equivalency of nerve-force is extremely small, by reference to its analogy with electrical currents, and by other considerations, and that therefore the degree of chemical change involved in its evolution is also small; and, on the other hand, the maximum of force to be obtained from an organic body is through the exercise of the affinities of its individual elements.

The second point is as to the special action of certain poisons on particular nervous centres,—strychnia on the cord, morphia on the brain, &c., the substances being carried by the blood to all alike. It is as necessary to explain why no effect is produced on those centres, or tracts which do not suffer, as to explain the action on the one which does. The explanation is sought in the fact that the difference in the functional activity of the brain and cord, the need for sleep by the brain, not experienced, at any rate in the same degree, by the cord, point to a difference of tension, and therefore of relation with the substances which act as poisons. This consideration will apply where the differences of susceptibility and of tension are not so marked.

But this is only part of a still wider question—the different action of poisons on different classes of animals. The explanation is still the same. Difference in the functional energy or activity of corresponding nerve-centres implies difference of tension.

The following facts bear strikingly on this point:—

1. Anæsthetics affect all classes of animals alike, *i. e.* when the effect is a general arrest of oxidation.

2. Strychnia, which acts on the cord, affects all animals alike. The spinal system is the centre which is most similar in its endowments in all classes of vertebrates.

3. The poisons which have the most diverse action on different animals are such as in man act on the cerebral ganglia.

XIV. "On the Communication of Vibration from a Vibrating Body to a surrounding Gas." By G. G. Stokes, M.A., Sec. R.S., Fellow of Pembroke College, and Lucasian Professor of Mathematics in the University of Cambridge. Received June 18, 1868.

(Abstract.)

In the first volume of the Transactions of the Cambridge Philosophical Society will be found a paper by the late Professor John Leslie, describing

some curious experiments which show the singular incapacity of hydrogen either pure or mixed with air, for receiving and conveying vibrations from a bell rung in the gas. The facts elicited by these experiments seem not hitherto to have received a satisfactory explanation.

It occurred to the author of the present paper that they admitted of a ready explanation as a consequence of the high velocity of propagation of sound in hydrogen gas operating in a peculiar way. When a body is slowly moved to and fro in any gas, the gas behaves almost exactly like an incompressible fluid, and there is merely a local reciprocating motion of the gas from the anterior to the posterior region, and back again in the opposite phase of the body's motion, in which the region that had been anterior becomes posterior. If the rate of alternation of the body's motion be taken greater and greater, or, in other words, the periodic time less and less, the condensation and rarefaction of the gas, which in the first instance was utterly insensible, presently becomes sensible, and sound-waves (or waves of the same nature in case the periodic time be beyond the limits of audibility) are produced, and exist along with the local reciprocating flow. As the periodic time is diminished, more and more of the encroachment of the vibrating body on the gas goes to produce a true sound-wave, less and less a mere local reciprocating flow. For a given periodic time, and given size, form, and mode of vibration of the vibrating body, the gas behaves so much the more nearly like an incompressible fluid as the velocity of propagation of sound in it is greater; and on this account the intensity of the sonorous vibrations excited in air as compared with hydrogen may be vastly greater than corresponds merely with the difference of density of the two gases.

It is only for a few simple geometrical forms of the vibrating body that the solution of the problem of determining the motion produced in the gas can actually be effected. The author has given the solution in the two cases of a vibrating sphere and of an infinite cylinder, the motion in the latter case being supposed to take place in two dimensions. The former is taken as the representative of a bell; the latter is applied to the case of a vibrating string or wire. In the case of the sphere, the numerical results amply establish the adequacy of the cause here considered to account for the results obtained by Leslie. In the case of the cylinder they give an exalted idea of the necessity of sounding-boards in stringed instruments; and the theory is further applied to the explanation of one or two interesting phenomena.

XV. "An Account of certain Experiments, on Aneroid Barometers, made at Kew Observatory, at the expense of the Meteorological Committee." By B. Stewart. Communicated by the Meteorological Committee. Received June 3, 1868.

In judging of the value of an instrument, such as an aneroid, it is not the mere extent of difference between its indications and those of a standard barometer that ought to guide us; but it is rather the constancy of its indications under the various circumstances to which it may be subjected, that determines its value.

An aneroid may differ from a standard barometer at the ordinary pressure, and to a greater extent at other pressures; but provided these differences can be well ascertained and remain constant, such an instrument ought to be regarded as valuable, just as much as a chronometer of known constancy, but of which the rate is wrong.

The circumstances which may be supposed to affect the indications of an aneroid may be classed under three heads, namely:—

(1) Time.
(2) Temperature.
(3) Sudden variations of pressure.

(1) *Time.*—Of the influence of time, I am not able to say much; Captain Henry Toynbee has allowed me to examine the various readings of an aneroid, which he carried about with him for many years in his voyages, and constantly compared with a standard barometer.

This aneroid (which I shall call No. 1) was between 4 and 5 inches in diameter, and was compensated for temperature.

In July 1860, as compared with a standard barometer, it read 0·025 in. too low. In September 1862 it read (at the same temperature) about 0·012 in. too low; while in March 1864 (still at the same temperature) it read about 0·020 in. too low.

This instrument, which was well cared for, and which, being used chiefly on the surface of the ocean, was subjected neither to a very great nor to a very sudden change of pressure, must be allowed to have retained its character with great constancy.

This is the only definite information regarding the effect of time on these instruments which I have received.

(2) *Temperature.*—A good aneroid is generally compensated by its maker for the effects of temperature, and the question to be investigated is, to what extent such compensations are trustworthy. I record the results (obtained at the Kew Observatory) of subjecting six aneroids, each $4\frac{1}{2}$ inches in diameter, made by two different makers, to a very considerable range of temperature.

No. of instrument.	Correction at				
	55° F.	72° F.	78° F.	88° F.	100° F.
2.	—·105	—·135	—·140	—·145	—·145
3.	—·055	—·090	—·095	—·095	—·100
4.	—·095	—·095	—·095	—·080	—·060
5.	—·106	—·106	—·111	—·111	—·111
6.	—·101	—·111	—·111	—·106	—·106
7.	—·061	—·061	—·061	—·061	—·031

These results are, on the whole, very satisfactory, and appear to show that a well-made compensated instrument has its indications comparatively little affected by a very considerable temperature change.

It ought always to be borne in mind that an aneroid is not capable of being read to the same accuracy as a standard barometer, and that the $\frac{1}{100}$ of an inch is a very small quantity. These temperature experiments were made at the ordinary atmospheric pressure.

I am unable to say what effect a change of temperature would have at a diminished pressure.

(3) *Sudden changes of pressure.*—A preliminary investigation, made at the request of Mr. De La Rue, into the behaviour of an aneroid belonging to the Italian Government, seemed to show considerable error at low pressures. For the purpose of investigating the influence of sudden changes of pressure upon the indications of aneroids, I then applied to some of the best known makers of these instruments, for the loan of several, and through their courtesy in lending me a sufficient number, and for a sufficiently long time, I have been enabled to investigate this influence at some length.

In the following experiments, the instruments were, to begin with, suspended vertically, at the usual atmospheric pressure. They were tapped before being read. The pressure was then lowered an inch, and the instrument allowed to remain ten minutes at this pressure before being read, after having again been well tapped.

The pressure was thus reduced an inch every time, being allowed to remain ten minutes at each stage; the instrument was always well tapped before being read, by means of an arrangement contrived for this purpose by Mr. R. Beckley. The exhaustion was carried downwards to 19 inches in the case of those instruments in which the scale was sufficiently great, and the instrument was allowed to remain an hour and a half at its lowest pressure; the air was then admitted an inch at a time, the previous arrangement as to time and tapping being followed.

TABLE I.

No. of aneroid.	Size.	Date of trial.	Correction at											
			30 inches.		29 inches.		28 inches.		27 inches.		26 inches.		25 inches.	
			Down.	Up.	Down.	Up.	Down.	Up.	Down.	Up.	Down.	Up.	Down.	Up.
8.	4½ inches.	June 1867.	*	·00	−·10	+·07	−·12	+·10	−·10	+·14	−·10	+·15	−·06	+·19
9.	4½ inches.	June 1867.		+·03	−·17	+·03	−·17	+·06	−·17	+·10	−·16	+·10	−·18	+·11
8.	2nd trial	July 1867.		+·06	−·16	+·09	−·15	+·10	−·12	+·14	−·12	+·19	−·07	+·22
9.	2nd trial	July 1867.		+·07	−·12	+·07	−·10	+·10	−·07	+·13	−·07	+·17	−·07	+·15
10.	2½ inches.	June 1867.		+·06	−·15	+·09	−·15	+·12	−·12	+·17	−·08	+·19	−·06	+·22
11.	2¾ inches.	July 1867.		−·03	−·20	−·05	−·21	−·02	−·22	−·03	−·22	·00	−·19	+·03
12.	2 inches	July 1867.		·00	−·19	+·02	−·19	+·06	−·18	+·05	−·13	+·12	−·10	+·14
13.	2 inches.	July 1867.		−·19	−·44	−·16	−·41	−·06	−·36	·00	−·42	·00	−·35	·00
14.	4½ inches.	June 1867.		+·06	−·10	+·07	−·08	+·11	−·03	+·15	−·01	+·16	−·01	+·16
15.	4 inches.	August 1867		+·01	−·11	+·03	−·15	·00	−·15	+·01	−·14	+·03	−·12	+·03
16.	2½ inches.	June 1867.		+·04	−·12	+·03	−·10	+·06	−·10	+·07	−·06	+·10	−·02	+·13
17.	2 inches.	June 1867.		+·15	−·10	+·20	−·05	+·29	+·02	+·37	+·08	+·44	+·13	+·46
			24 inches.		23 inches.		22 inches.		21 inches.		20 inches.		19 inches.	
8.	4½ inches.	June 1867.	−·07	+·20	·00	+·23	+·04	+·23	+·07	+·21	+·12	+·21	+·18	+·18
9.	4½ inches.	June 1867	−·17	+·11	−·14	+·09	−·10	+·11	−·04	+·11	+·01	+·10	+·05	+·05
8.	2nd trial	July 1867	−·07	+·24	−·04	+·27	+·01	+·25	+·03	+·26	+·09	+·27	+·16	+·25
9.	2nd trial.	July 1867.	−·08	+·15	−·08	+·14	−·06	+·12	−·03	+·16	·00	+·16	+·05	+·13
10.	2½ inches.	June 1867.	−·13	+·24	−·03	+·20	·00	+·21	+·06	+·20	+·12	+·17	+·15	+·17
11.	2¾ inches.	July 1867.	−·16	+·06	−·15	+·08	−·15	+·11	−·12	+·09	−·09	+·07	−·06	+·11
12.	2 inches.	July 1867.	−·12	+·14	−·11	+·15	−·05	+·24	+·02	+·24	+·08	+·27	+·07	+·26
13.	2 inches.	July 1867.	−·34	·00	−·33	·00	−·29	+·01	−·26	·00	−·18	·00	−·13	−·05
14.	4½ inches.	June 1867.	+·01	+·13	−·02	+·04								
15.	4 inches.	August 1867.	−·05	+·08	+·01	+·09								
16.	2½ inches.	June 1867.	+·01	+·12	+·04	+·10								
17.	2 inches.	June 1867.	+·18	+·49	+·28	+·48								

* Inadvertently in these experiments a single observation in the receiver at the ordinary pressure before exhaustion was not made, but the reading at this pressure was supposed to be represented by the mean of those readings made in the open air for several days before the experiment. As, however, such readings are not strictly comparable with those made in the receiver, they have been omitted.

This Table may be better followed by considering, in the first place, the

down readings, and by supposing each aneroid right to start with (say, right at 29 inches), that is to say, by correcting each instrument for index error. We thus obtain:—

TABLE II.

Correction for the Down readings of the Aneroids of Table I., supposed right at 29 inches.

No. of aneroid.	Size.	Date of trial.	29 inches.	28 inches.	27 inches.	26 inches.	25 inches.	24 inches.	23 inches.	22 inches.	21 inches.	20 inches.	19 inches.
8.	4¼ inches..	June 1867	·00	−·02	·00	·00	+·04	+·03	+·10	+·14	+·17	+·22	+·28
9.	4¼ inches..	June 1867	·00	·00	·00	+·01	−·01	·00	+·03	+·07	+·13	+·18	+·22
8.	2nd trial ..	July 1867	·00	+·01	+·04	+·04	+·09	+·09	+·12	+·17	+·19	+·25	+·32
9.	2nd trial ..	July 1867	·00	+·02	+·05	+·05	+·05	+·04	+·04	+·06	+·09	+·12	+·17
10.	2¼ inches..	June 1867	·00	·00	+·03	+·07	+·09	+·02 ?	+·12	+·15	+·21	+·27	+·30
11.	2¾ inches..	July 1867	·00	−·01	−·02	−·02	+·01	+·04	+·05	+·05	+·08	+·11	+·14
12.	2 inches ..	July 1867	·00	·00	+·01	+·06	+·09	+·07	+·08	+·14	+·21	+·27	+·26
13.	2 inches ..	July 1867	·00	+·03	+·08	+·02	+·09	+·10	+·11	+·15	+·18	+·26	+·31
14.	4½ inches..	June 1867	·00	+·02	+·07	+·09	+·09	+·11	+·08				
15.	4 inches ..	August 1867 ..	·00	−·04	−·04	−·03	−·01	+·06	+·12				
16.	2¼ inches..	June 1867	·00	+·02	+·02	+·06	+·10	+·13	+·16				
17.	2 inches ..	June 1867	·00	+·05	+·12	+·18	+·23	+·28	+·38				

If now we separate the results of Table II. into two sets, one comprising large (4 to 4½ inch) aneroids and the other small instruments, we shall find the mean down correction for large aneroids to be as follows.

	29 in.	28 in.	27 in.	26 in.	25 in.	24 in.	23 in.	22 in.	21 in.	20 in.	19 in.
Mean correction for large aneroids, graduated to 19 in. ..	·00	·00	+·02	+·03	+·04	+·04	+·07	+·11	+·14	+·19	+·25
23 in.	·00	−·02	+·02	+·03	+·04	+·08	+·10				

In like manner we shall find for small aneroids, supposed right at 29 inches, the following mean correction:—

	29 in.	28 in.	27 in.	26 in.	25 in.	24 in.	23 in.	22 in.	21 in.	20 in.	19 in.
Mean correction for small aneroids, graduated to 19 in. ..	·00	+·01	+·02	+·03	+·07	+·07	+·09	+·12	+·17	+·23	+·25
23 in.	·00	+·03	+·07	+·12	+·16	+·20	+·27				

It will be seen that there are two instances in which the same instrument has been twice experimented on. Assuming that the mean of the two experiments represents the true correction for each of these instruments, we find:—

	29 in.	28 in.	27 in.	26 in.	25 in.	24 in.	23 in.	22 in.	21 in.	20 in.	19 in.
No. 8. Mean correction, deduced from two experiments	·00	−·01	+·02	+·02	+·06	+·06	+·11	+·15	+·18	+·23	+·30
Mean *minus* first determination ..	·00	+·01	+·02	+·02	+·02	+·03	+·01	+·01	+·01	+·01	+·02

In like manner:—

	29 in.	28 in.	27 in.	26 in.	25 in.	24 in.	23 in.	22 in.	21 in.	20 in.	19 in.
No. 9. Mean of two experiments	·00	+·01	+·02	+·03	+·02	+·02	+·03	+·06	+·11	+·15	+·20
Mean *minus* first determination ..	·00	+·01	+·02	+·02	+·03	+·02	·00	−·01	−·02	−·03	−·02

We see from these results that if aneroids, right to begin with, be sub-

jected to a decrease of pressure similar to that to which they were subjected in these experiments—

(1) That a well-constructed large aneroid will not go far wrong down to 24 inches, but after that pressure its reading will be considerably lower than that of a standard barometer, so that a large positive correction will have to be applied.

(2) That small aneroids are less trustworthy than large ones, and probably cannot be trusted below 26 inches.

(3) That if previous experiments are made upon an aneroid, we are enabled by this means to obtain a table of corrections which, when applied to future observations with the same instrument, will most probably present us with a much better result than had we not verified our instrument at all, and that by this means we may use our instrument down to 19 inches with very good results.

Let us now consider the up readings of these instruments, and let us suppose that each instrument is right to begin with, that is to say, while remaining an hour and a half at its lowest reading.

These corrections and up readings are exhibited in the following Table:—

TABLE III.

No. of aneroid.	Correction for the up readings of the aneroids of Table I. supposed right with standard at lowest reading.											
	19.	20.	21.	22.	23.	24.	25.	26.	27.	28.	29.	30.
8.	·00	+·03	+·03	+·05	+·05	+·02	+·01	−·03	−·04	−·08	−·11	−·18
9.	·00	+·05	+·06	+·06	+·04	+·06	+·06	+·05	+·05	+·01	−·02	−·02
8.	·00	+·02	+·01	·00	+·02	−·01	−·03	−·06	−·11	−·15	−·16	−·19
9.	·00	+·03	+·03	−·01	+·01	+·02	+·02	+·04	·00	−·03	−·06	−·06
10.	·00	·00	+·03	+·04	+·03	+·07	+·05	+·02	·00	−·05	−·08	−·11
11.	·00	−·04	−·02	·00	−·03	−·05	−·08	−·11	−·14	−·13	−·16	−·14
12.	·00	+·01	−·02	−·02	−·11	−·12	−·12	−·14	−·21	−·20	−·24	−·26
13.	·00	+·05	+·05	+·06	+·05	+·05	+·05	+·05	+·05	−·01	−·11	−·14
14.	...	...	...	...	·00	+·09	+·12	+·12	+·11	+·07	+·03	+·02
15.	...	...	...	...	·00	−·01	−·06	−·06	−·08	−·09	−·06	−·08
16.	...	...	...	...	·00	+·02	+·03	·00	−·03	−·04	−·07	−·06
17.	...	...	...	...	·00	+·01	−·02	−·04	−·11	−·19	−·28	−·33

Hence we find the mean up correction for large aneroids:—

	19 in.	20 in.	21 in.	22 in.	23 in.	24 in.	25 in.	26 in.	27 in.	28 in.	29 in.	30 in.
Supposed right at 19 in..........	·00	+·03	+·03	+·03	+·03	+·02	+·01	·00	−·03	−·06	−·08	−·11
Supposed right at 23 in..........	..	..	..	..	·00	+·04	+·03	+·03	+·01	−·01	−·02	−·03

In like manner for small aneroids we have the following result :—

	19 in.	20 in.	21 in.	22 in.	23 in.	24 in.	25 in.	26 in.	27 in.	28 in.	29 in.	30 in.
Right at 19 in. ..	·00	·00	+·01	+·02	−·01	−·01	−·02	−·04	−·07	−·10	−·15	−·16
Right at 23 in. ..	..	..	..	..	·00	+·01	·00	−·02	−·07	−·11	−·12	−·19

As before, there are two instances in which the same instrument was twice tried; assuming the mean of the two trials to represent the truth, we find—

	19 in.	20 in.	21 in.	22 in.	23 in.	24 in.	25 in.	26 in.	27 in.	28 in.	29 in.	30 in.
No. 8. Mean correction..	·00	+·02	+·02	+·02	+·03	·00	−·01	−·04	−·07	−·11	−·12	−·18
Mean *minus* first determination ..	·00	−·01	−·01	−·03	−·02	−·02	−·02	−·01	−·03	−·03	−·01	·00

In like manner—

	19 in.	20 in.	21 in.	22 in.	23 in.	24 in	25 in.	26 in.	27 in.	28 in.	29 in.	30 in.
No. 9. Mean correction..	·00	+·04	+·04	+·03	+·03	+·04	+·04	+·04	+·02	−·01	−·04	−·04
Mean *minus* first determination ..	·00	−·01	−·02	−·03	−·01	−·02	−·02	−·01	−·03	−·02	−·02	−·02

We may learn from these results that if aneroids which have been subjected for at least one hour and a half to the lowest pressures which they register, have the pressure increased by means of the gradual introduction of air into the receiver, after the manner already described.

(1) That a well-constructed large aneroid will not go far wrong for about 8 inches above the lowest pressure.

(2) That in this respect small aneroids are somewhat less trustworthy than large ones.

(3) That if the instrument read be previously tested and its corrections ascertained, we may consider it trustworthy (making use of these corrections) for up readings throughout a greater range than if it had not been so tested.

I come now to consider whether a rapid change of pressure affects an aneroid after the experiment has been completed.

The following Table will exibit the results obtained in this direction.

TABLE IV.

	1st Ex. 8.	1st Ex. 9.	2nd Ex. 8.	2nd Ex. 9.	10.	11.	12.	13.	14.	16.
Correction before experiment	−·10	−·12	−·13	−·09	−·12	−·11	−·13	−·47	−·04	−·05
Immediately after experiment	·00	+·03	+·06	+·07	+·06	−·03	·00	−·19	+·06	+·04
18 hours after experiment	−·07	−·03	+·04	+·02	..	−·10	−·07	−·34	+·01	+·01
48 hours after experiment	−·08	−·04	..	..	−·03	..	..	−·37	..	..
3 days after experiment	−·08	−·05	..	..	..	..	..	..	−·01	−·01
3 weeks after experiment	−·13	−·10	..	..	−·11	..	..	..	−·07	−·06

It thus appears that if an instrument reads correctly before it is put into the receiver it will read too low immediately afterwards, and that it may be some considerable time before it recovers its previous reading. The instrument cannot, therefore, be safely trusted for absolute determinations if it has been recently exposed to rapid changes of pressure.

The experiments hitherto recorded, in which an inch of pressure has been taken away or added every ten minutes, are perhaps analogous to ascents in a balloon, or descents from a mountain; they are not, however, precisely analogous to mountain ascents, since a longer time than 10 minutes is usually taken to produce a change of pressure equal to 1 inch.

At the suggestion of Mr. Charles Brooke, a couple of aneroids were tested in April 1868, with the view of rendering the experiment more analogous to a mountain ascent.

The pressure was reduced by half an inch at a time and at intervals of 30 minutes, the aneroids being well tapped.

The following corrections were obtained for down readings (instruments supposed right at 30 inches).

TABLE V.

At	No. 8.	No. 9.	At	No. 8.	No. 9.
inches.			inches.		
30	·00	·00	23·5	+·08	−·02
29·5	·00	−·03	23	+·11	−·03
29	·00	−·04	22·5	+·12	−·01
28·5	·00	−·03	22	+·14	·00
28	·00	−·03	21·5	+·16	+·02
27·5	·00	−·02	21	+·17	+·04
27	....	....	20·5	+·20	+·06
26·5	·00	−·02	20	+·22	+·07
26	+·01	−·02	19·5	+·25	+·09
25·5	+·04	−·02	19	+·27	+·11
25	+·05	−·04			
24·5	+·06	−·02			
24	+·05	−·01			

These results, when compared with the previous determinations for these same instruments, would seem to show that a somewhat better result is obtained when the exhaustion is carried on more slowly, and hence that the corrections depend, to a considerable extent, on the nature of the treatment received. No. 8 seems to be more constant under different treatment than No. 9.

From all these experiments we may perhaps conclude as follows :—

(1) A good aneroid of large size may be corrected for temperature by an optician, so that the residual correction shall be very small.

(2 α) If an aneroid correct, to commence with, be used for a balloon or mountain ascent, it will be tolerably correct for a decrease of about 6 inches of pressure.

(2 β) A large aneroid is more likely to be correct than a small one.

(2 γ) The range of correctness of an instrument used for mountain ascents may be increased by a previous verification, a table of corrections being thus obtained.

(3 α) If an aneroid have remained some time at the top of a mountain, and be supposed correct to start with, then it will give good results for about 8 inches of increase of pressure.

(3 β) A large aneroid is more likely to be correct than a small one.

(3 γ) If the aneroid has been previously verified, it is likely to give a better result.

(4) After being subjected to sudden changes of pressure the zero of an aneroid gradually changes, so that under such circumstances it ought only to be used as a differential and not as an absolute instrument, that is to say, used to determine the distance ascended, making it correct to begin with, or to ascertain the distance descended, making it correct to begin with, it being understood that the instrument ought to be quiescent for some time before the change of pressure is made.

Before concluding I ought to mention that most of the experiments herein described were undertaken and executed in a very careful manner by Mr. T. W. Baker.

XVI. "Contributions to Terrestrial Magnetism, No. XI." By General Sabine, R.A., P.R.S., &c. Received June 18, 1868.

(Abstract.)

This number of the Contributions of Terrestrial Magnetism contains the completion of the Magnetical Survey of the South Polar Regions, undertaken by Her Majesty's Government in 1840–1845 at the joint instance of the Royal Society and the British Association for the Advancement of Science. The observations themselves, and their provisional discussion, have already been given in the previous numbers, V., VI., VIII., and X. of the Contributions. The present number contains a general review of

the whole survey, and is accompanied by three maps, which have been prepared, with the permission of the Hydrographer, Captain Richards, R.N., F.R.S., under the careful superintendence of the Assistant Hydrographer, Captain Frederick John Evans, R.N., F.R.S., one map being allotted to each of the three magnetic elements, viz. the Declination, Inclination, and Intensity of the Magnetic Force. In these maps the Isogonic, Isoclinal, and Isodynamic lines have been drawn, by the author of the paper, conformably with the observations around the circumference of the globe between the parallel of 30° S. and the South Pole. The paper also contains Tables, prepared with a view to the revision of the calculations of Gauss's 'Allgemeine Theorie des Erdmagnetismus.' They give the values of each of the three magnetic elements at the intersections of every fifth degree of latitude between 40° of south latitude and the South Pole, and every tenth degree of longitude between 0 and 360°.

"On the Spectrum of Comet II., 1868." By William Huggins, F.R.S. Received July 2, 1868.

(Abstract.)

The author describes the appearance of the comet in the telescope on June 22 to consist of a nearly circular coma, which became rather suddenly brighter towards the centre, where there was a nearly round spot of light. A tail was traced for nearly a degree.

He found the light of the comet, when examined with a spectroscope, furnished with two prisms of 60°, to be resolved into three broad bright bands.

The brightest band commences at about *b*, and extends nearly to F. Another band begins at a distance beyond F, rather greater than half the interval between *b* and F. The third band occurs about midway between D and E. In the two more refrangible of these bands the light was brightest at the less refrangible end, and gradually diminished towards the other limit of the bands. The least refrangible of the three bands did not exhibit a similar gradation of brightness.

These bands could not be resolved into lines, nor was any light seen beyond the bands towards the violet and the red.

The measures of these bands are given, and a diagram of their appearance.

The author found this cometic spectrum to agree exactly with a form of the spectrum of carbon which he had observed and measured in 1864. When an induction spark, with Leyden jars intercalated, is taken in a current of olefiant gas, the highly heated vapour of carbon exhibits a spectrum which is somewhat modified from that which may be regarded as typical of carbon. The light is of the same refrangibilities, but the separate strong lines are not to be distinguished. The shading, composed of

numerous fine lines, which accompanies the lines appears as an unresolved nebulous light.

On June 23 the spectrum of the comet was compared directly in the spectroscope with the spectrum of the induction spark taken in a current of olefiant gas.

The three bands of the comet appeared to coincide with the corresponding bands of the spectrum of carbon. In addition to an apparent identity of position, the bands in the two spectra were very similar in their general characters and in their relative brightness.

These observations were confirmed on June 25.

The remarkably close resemblance of the spectrum of the comet with that of the spectrum of carbon, necessarily suggests the identity of the substances by which in both cases the light was emitted.

The great fixity of carbon seems, indeed, to raise some difficulty in the way of accepting the apparently obvious inference from these prismatic observations. Some comets have approached sufficiently near the sun to acquire a temperature high enough to convert even carbon into vapour.

In the case of other comets, the author suggests that the difficulty is one of degree only, for the conditions are not known under which even a gas permanent at the temperature of the earth could maintain sufficient heat to emit light.

The author states that some phosphorescent substances give spectra which are discontinuous, but he gives reasons which would scarcely permit us to consider cometary light to be of a phosphorescent character.

The spectrum shows that the colour of this comet was bluish green. Considerable difference of colour has been remarked in the parts of some comets. Sir William Herschel described the head of the comet of 1811 to be of a greenish or bluish-green colour, while the central point appeared of a ruddy tint. The same colours have been observed in other comets. If carbon be the substance of some comets, this substance, if incandescent in the solid state, or reflecting, when in a condition of minute division, the light of the sun, would afford a light which, in comparison with that emitted by the luminous vapour of carbon, would appear yellowish or approaching to red.

The author refers to the bearing of these results on certain cometary phenomena, and on the apparent identity of the orbits of the periodical meteors with those of some comets.

[*To face page* 483.]

No	Sex	1. Cleido-occipital.	2. Sterno-cleido-mastoid.	3. / 9. O... / Pectoralis hy... major.	10. Pectoralis minor.	11. Sterno-scapular.	1... Lat... d...
1	M.	B.	B. double clavic. slip.				
2.	M.			B. doubl... belly ... orig... / ...igastric ...			...ed lo
3.	M.	L. st. slip					
4	M						
5.	M.	B.					
6.	M.	B.	B. double clavic origin.	...sed with ...toid.			...l &
7.	M	B. sternal slip.					ta... &
8	M.			...sed with ...toid.			ta...
9.	M			R. cla... of st...	L. to capsular ligament.	B.	B. slip 9th int... tal fa...
10.	M.				B. to glenoid ligament.		l & ta
11.	M.	B.	B. double clavic. origin.				&
12.	M.			...ed with ...oid.	B. to glenoid ligament.	B.	&
13	M.	B. double	B. double clavic. origin.	L. no ... tend...	B. to capsular ligament.		& ta
14.	M.					B.	
15.	M.			R. dou... L. clav...	B. to coraco-acrom. lig.	B.	l & ta...
16.	M.			...ed with ...oid.			&
17.	M.					B.	& ...nd...
18.	M.	B.					
19.	F.						
20.	F			L. clav...ble origi... or.			& ta
21.	F.						& tu...
22.	F.						
23.	F.	B.					
24.	F.						
25.	F	L.		...astric			
26.	F.	B					
27.	F.						&
28.	F.	B.			B. to capsular ligament.		ta...
29.	F				B. to capsular ligament.		L. dorsi epitrocl...
30.	F.	B.	B. double clavic origin.		B. to capsular ligament.	L.	
31.	F					B.	&
32.	F.				R. into clav		B. slip ... fascia.
33.	F.				B. to capsular ligament.	R.	&
34.	F.			R. clav orig...			ta &
35.	F	L.	L. double clavic. origin			B	ta
36.	F.		B. double clavic. origin			R.	
		14	7		10	16	

XVII. "Variations in Human Myology observed during the Winter Session of 1867–68 at King's College, London." By John Wood, F.R.C.S., Examiner in Anatomy to the University of London. Communicated by Dr. Sharpey. Received June 10, 1868.

On giving the results of our observations in this branch of scientific inquiry, my thanks are again due to the zeal and increased experience of my assistants in the dissecting-rooms, and especially to Mr. J. B. Perrin, by whose constant labour and vigilance my work has been most materially aided. Of the 36 subjects dissected, not one has been found totally wanting in departures from the standard descriptions of the muscular system given in anatomical text-books. Some of the more common variations, which are usually mentioned by our best authors, have been included this year with a view of determining their relative frequency. Others have been comprised which seem to show the first evidences of a tendency to the more complete and striking abnormal forms.

These I have deemed of much importance in throwing light upon the origin of the more complete variations. Many of them have been found so often as materially to add to the total number of abnormalities in the Table, and also to the labour of searching out and classifying them.

With few exceptions, the *lines of variation* have been found in the same grooves as in former sessions, as will be seen by comparing the Tables. Our observations have still been confined to the muscles of the head and neck, arms and legs, not noting those of the face, back, or abdomen, except in the case of unusual eccentricity of formation. All the more important formations have been sketched by myself from the subject; and the greater part of those which have been simply noted have also been previously examined by me. The proportion of the two sexes happens this year to have been exactly equal, giving us a fair opportunity of comparing the proportionate frequency of muscular abnormalities in each. For the sake of greater convenience of reference, they have been placed separately in the Table.

In the 36 subjects, 18 *males* and 18 *females*, we have the large number of 558 muscular variations, of which 20 were found in the *head* and *neck* proper, 390 in the *arms* (including those lying in the neck which act upon the upper extremity), and 148 in the *legs*. But although the absolute number of specimens found is so much greater than in former years, the *lines of variation* are not proportionably increased, amounting to 72 as compared with 61 of last year. Of these 13 were found in the *head*- and *neck*-muscles proper, 37 in the *arm*, and 22 in the *leg*—the greatest increase being in the *arm*.

In the first section of the accompanying Table will be found 20 speci-

mens affecting 13 muscles proper to the *head and neck*, and 40 others affecting 6 muscles which, acting upon the clavicle or scapula, are numbered with the muscles of the upper extremity, viz. the cleido-occipital, sterno-cleido-mastoid, omohyoid, trapezius, levator anguli scapulæ, and levator claviculæ.

1. *Cleido-occipital* (*Cephalo-humeral* of the lower animals).—Of this muscle 8 specimens have been found in the *male* subjects, all on both sides—and 6 in the *females*, two of which were on the left side only. On the left side of No. 3, male, and on both sides of No. 7, male, the curious and interesting arrangement was found which is drawn from the former subject in fig. 1. A slip of parallel muscular fibres, three-fourths of an inch wide (*a*), arising from the sternal end of the clavicle, close outside of and in connexion with the sternal origin of the sterno-mastoid, crossed superficially and obliquely the cleido-mastoid (*b*) (which was split into two parts) to join the *cleido-occipital* (*c*) about its middle. The more usual clavicular attachment of the latter muscle was broad, and connected with the centre of the clavicle directly over the posterior triangle, reaching from the origin of the cleido-mastoid as far out as the clavicular attachment of the trapezius (*d*). This arrangement gives a very complex appearance to the whole group of muscles, and strikingly resembles the formation in the Marmot, Polecat, Genette, and striped Hyæna, and, to a less extent, in the Coati. In these animals the cephalo-humeral is only connected with the trapezius close up to the occiput. Towards the clavicle it is continued forward, superficial to the cleido-mastoid, and joins wholly or by a separate slip with the sterno-mastoid (see Cuvier and Laurillard's plates). This resemblance to the *cephalo-humeral* of these animals tends strongly to confirm the view of the homology of the *cleido-occipital* in Man, given by the author in former papers. On the right side of the subject No. 3, the cleido-occipital was large, but presented no slip of connexion with the sterno-mastoid; but it was found on both sides of No. 7. A tendency to this formation was seen also in subject No. 21 of last year's Table.

Fig. 1 (Subject No. 3).

In No. 13 the cleido-occipital was double, and distinct from both the cleido-mastoid and trapezius along its whole length. This homologue of the *cephalo-humeral* of animals was found distinct and very well marked

by Murie in his dissection of a Bushwoman (Journal of Anatomy and Physiology, No. 2, May 1867, p. 198).

2. *Sterno-cleido-mastoid.*—In 4 *male* and 3 *female* subjects out of the 36, the *clavicular* origin of this muscle was double, or divided into two portions. In all but one of these the cleido-occipital was also present.

3. *Omohyoid.*—In 2 male and 2 female subjects the *posterior belly* presented an additional or *clavicular* origin. Three were found on the left side only, and one on the right only. On the opposite side of one male (No. 15), a muscular slip arising from the middle of the clavicle (quasi-clavicular origin of the omohyoid) passed upwards and inwards to join the *sterno-thyroid* at its insertion, the omohyoid itself on that side being normal, with the exception of having *no* median tendon. In No. 9 the omohyoid, otherwise normal, passed through an opening between the normal origin of the *sterno-thyroid* and an abnormal slip to that muscle from the clavicle, at the junction of its middle and outer thirds. In No. 13 also, the *median tendon* of the omohyoid was wanting, a continuity of muscular fibre passing from its origin to its insertion. This is always the case in Monkeys and the lower mammalia. In a male subject (No. 2) the *anterior belly* was double, the superior one fusing with the *hyoglossus* and *middle constrictor* muscles, and receiving, moreover, a slip from the middle of the *sterno-thyroid* (see fig. 2 *h*).

Out of 70 subjects now examined by the author with a view to the abnormalities of the *omohyoid* muscle, viz. 40 *males* and 30 *females*, the *anterior belly* has been found *double* in 4 males, in two on both sides, and in two on the right side only. In one male it was *triple* on both sides. A *clavicular* origin of the *posterior belly* has been found in 2 males on the left side only; and in 3 females, of which 1 was on both sides, 1 on the right, and 1 on the left side only. The *median tendon* has been found absent in 2 males, in 1 on the right, and in the other on the left side only. Thus in 12 out of 70 subjects some irregularity existed in this muscle—a proportion greater than that found by Professor Turner in 373 subjects, viz. 5 or 6 per cent. (Irregularities of the Omohyoid, p. 2). A clavicular origin of the posterior belly was found in 5 subjects out of the 70, or about 7 per cent. Turner found it in 17 out of 373 subjects, *i. e.* rather more than 4 per cent. In 5 out of 70 subjects (also about 7 per cent.) the *anterior belly* was found *double* or *triple*. Turner does not mention this abnormality, but found the anterior belly blended with the sterno-hyoid in 4 out of the 373 cases.

4. *Digastric.*—In 3 subjects, 2 *male* (Nos. 13 & 16) and 1 *female* (No. 34), the anterior belly of this muscle was *double*, in the males on both sides, and in the female on the left side only. In No. 13 the supernumerary head decussated across the median line with its opposite fellow. This variety has been before recorded by Fleischmann (Anat. Wahrnehm. in Erlang. Abhand. Bd. i. S. 26 & 27, Taf. i. fig. i. 1810), and subsequently by Gantzer (Dissert. Anat. Musc. varietatis sistens. 1813), Sœmmerring, Theile, and Henle.

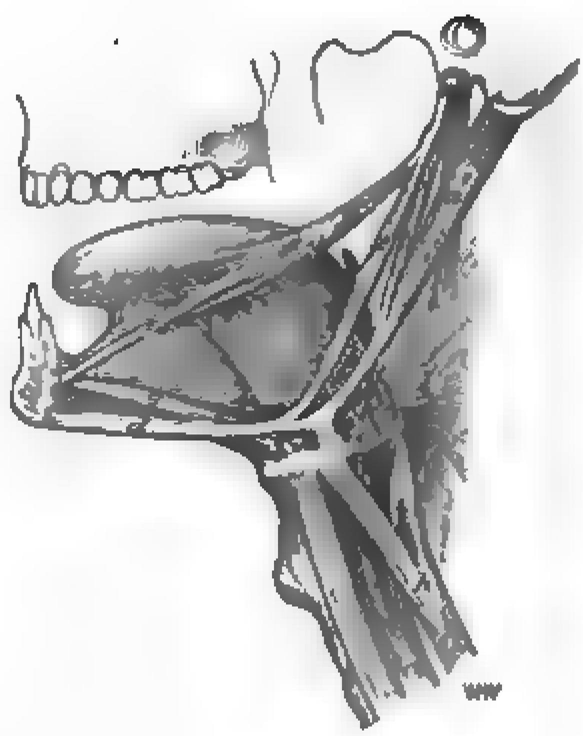
[redacted]

In 102 subjects examined by the author, the anterior belly of this muscle has been found *double* in 5 out of 68 *males*, of which in 2 the supernumerary belly decussated with its fellow across the median line. Out of 34 *females* it was found double once only on the left side.

In the Norway Rat (*Mus decumanus*), the anterior bellies of the digastric are completely blended together, arising without any median fissure from an arched tendinous structure connecting the two median tendons. They are also united in the *Cercopithecus sabæus*, according to Rudolphi (Heusinger's Zeitschrift, Bd. iii. S. 335), and blended so as completely to cover the mylo-hyoids in *Callithrix* and *Papio Mormon* (Cuvier and Laurillard's plates). More or less so they are found in the ruminants.

In the male subject No. 2, a combination of curious varieties led to the sketch of the adjoining figure (fig. 2), taken by the author from the dissected parts. The posterior belly of the digastric (*a*) appeared at first sight to be a triple muscle. The two anterior muscular slips (*b* & *c*), which were implanted upon the median tendon, one behind the hyoid pulley with the true posterior belly, and the other in front of it and nearer to the anterior belly (*d*), were found on closer examination to arise from the *styloid* process, the hinder one (*b*) from the usual site of origin of the *stylo-hyoid* muscle, and the front one (*c*) from the tip of the process with the *stylo-glossus*. Each of these, therefore, represents the two halves of a completely split or divided *stylo-hyoid*, which, instead of uniting to be inserted into the side of the hyoid bone, are implanted separately upon the tendon of the digastric. Coexistent with this were two other abnormalities which may most conveniently be described here. One was a curious well-developed muscular slip, found only on the left side, arising as a muscle one-sixth of an inch wide (*e*), from the lower genial tubercle of the mandible outside the genio-hyoid muscle (*g*). Thence passing upward and backward over the genio-hyo-glossus and hyo-glossus muscles, it finally united its fibres with those of the *stylo-glossus* (*f*) at their insertion into the outer and back part of the tongue. This muscle, which the author has never before met with nor found recorded, is totally different from the *mylo-glossus* described in his last paper. It also differs from that mentioned under this name by Böhmer (Obs. Anat. rar. præfat. viii. note 5), as seen by Vesalius, Riolanus, and Spigelius, and considered

Fig. 2 (Subject No. 2).



by Verheyneus, Haller, Douglass, and others to be that part of the *superior constrictor* which is attached to the side of the tongue, and which was at first called by Winslow the "*mylo-glossus*" (Comp. Myograph. Spec. p. 93). For the sake of distinction the author has given the name of *genio-glossus* to this remarkable slip.

On the same side of the subject was the bifurcation of the anterior belly of the omohyoid before mentioned (*h*), and also of the *sterno-thyroid* (*i*). A considerable portion of the posterior fibres of each of these muscles was split off; and the two slips, uniting together, were partly inserted into the tip of the greater horn of the hyoid bone, but chiefly blended with the fibres of the *hyo-glossus* (*k*) and *middle constrictor* (*l*) muscles.

5. *Trapezius.*—In 2 males the fibres of this muscle reached only so far down the back as the eighth and ninth dorsal spines respectively. (This deficiency has been found by the author in 4 males out of 70 subjects, viz. 40 males and 30 females.) In one (No. 11) the cervical and dorsal portions were completely separate and distinct muscles, one connected with the occipital bone and *ligamentum nuchæ*, and the other with the dorsal spinous processes,—the two joining only at their insertion into the acromion process. Something like this is the normal condition in the Mole, *Ornithorhynchus*, and Echidna, and to a less marked extent in the Agouti, according to Meckel (Anat. Comp.) and Mivart and Murie (Proceedings of the Zoological Society, June 26, 1866). It has been recorded in the human subject by Sœmmerring (De Corp. Hum. Fabrica, t. iii. 1796), Fleischmann, Zagorsky, and Theile. Macalister has also found it ("Notes on Anomalies in Human Anatomy," in Proc. Royal Irish Academy, April 23rd, 1866, p. 21).

In the female (No. 31) the cervical border of the trapezius was attached to the clavicle as far inwards as the origin of the sterno-cleido-mastoid, and so covering the posterior triangle entirely. A foramen or tendinous arch, placed about the middle of the clavicle, allowed the passage of the supraclavicular nerves. This very peculiar formation has been described by Macwhinnie (London Med. Gazette, No. 948, January 30th, 1846, p. 194), and more recently figured by Gruber, of St. Petersburg (Vier Abhandlung. 1847, S. 16, 17), by Hallett (Edin. Med. and Surg. Journal, 1848, No. 174), and by Macalister. The author believes it to be a blending or fusion of the *cleido-occipital* or *cephalo-humeral* with the cervical border of the *trapezius*, of the same kind as the more common blending of the same muscle with the hinder border of the *sterno-cleido-mastoid*.

This view is further supported by the arrangement found in another female (No. 35), on the right side of whom the trapezius detached from the upper part of its cervical border a considerable muscular slip to be inserted into the clavicle, close to the cleido-mastoid. Through a tendinous arch or opening left between this slip and the lower part of the normal clavicular fibres of the trapezius the hinder belly of the omohyoid passed, along with the supraclavicular nerves, as before alluded to with

the abnormalities of that muscle. On this side of the subject no proper cleido-occipital was found; but on the other (left) side, this muscle was large, well formed, and separate, both from the trapezius and sterno-cleido-mastoid.

6. *Levator anguli scapulæ.*—The variations of this muscle have been this year recorded with a view to throw light upon the occasional occurrence in the human subject of the *levator claviculæ* described by the author in former papers.

In 2 males (Nos. 2 & 6) and 2 females (Nos. 20 & 29) this muscle was much divided, forming a distinctly double muscle, one from the two upper, and the other from the third and fourth cervical transverse processes. Such an arrangement is figured by Cuvier and Laurillard in their plate 7 of the Anatomy of a Negro. The upper one is there marked as the representative in Man of the "*omo-trachélien*" (levator claviculæ) of animals.

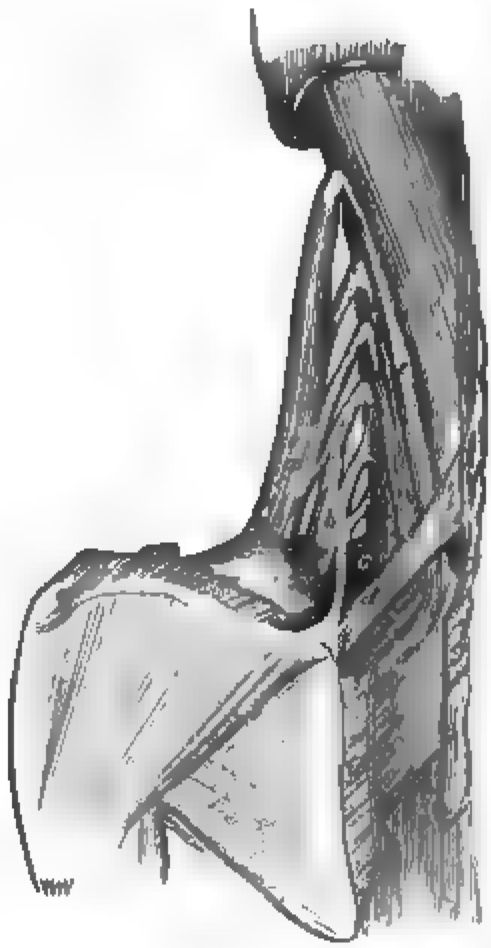
A more perfect homologue of this muscle in the human subject, however, has been recorded by Macwhinnie (*op. cit.* p. 194), and by the author in his former papers—arising from the transverse process of the atlas, and, in some, as low down as that of the third cervical vertebra, and inserted into the middle or outer third of the clavicle.

Such a muscle has been noted and described by the author in 5 male subjects out of 174 of both sexes in whom it has been carefully looked for. Macalister has also found it in a spare female (*op. cit.*). In No. 5 of the subjects noted in column 8 of the Table of the present year, a considerable and long muscular slip, arising by a tendon from the transverse process of the third cervical vertebra, was found lying superficial to the fibres of the levator anguli scapulæ, and inserted into the fascia placed immediately behind the clavicle and covering the axillary surface of the first digitation of the *serratus magnus* muscle. A muscle similar to this was found by Dr. Murie in the Bushwoman (*op. cit.*), and was rightly looked upon by him as an imperfect *levator claviculæ*, the insertion of which had only just failed to reach the clavicle. Kelch records that he saw, in a female subject, a triple division of the levator anguli scapulæ, the middle part sending off a slip to the scapulo-thoracic fascia (Beiträge zur pathologischen Anatomie, 1813, xxv. S. 33). Rosenmüller found a slip from the first cervical transverse process, inserted into the first digitation of the serratus magnus (De nonnullis Musc. &c. Leipzig, 1814, S. 5). These were, no doubt, specimens of the last-mentioned variety. In the male subject (No. 6) the levator anguli scapulæ was divided into six slips along its whole length, arising from the five upper cervical transverse processes, and all inserted into the usual place.

On the left side of a female (No. 20), the subject of fig. 3, a less extensive division of its fibres was found, combined with a double insertion, and a significant fusion of the lower portion with the *serratus magnus* and the *rhomboideus minor.* This arrangement supports and illustrates the homology first pointed out by Meckel (Archiv, viii. S. 585, and Muskellehre, Bd. ii. S.

402) between the levator anguli scapulæ of the human subject and the upper or cervical part of the serratus magnus as found in the lower animals—an homology which was disputed by no less an authority than Cuvier. The muscle arose by five digitations from the hinder tubercles of the five upper cervical transverse processes, and by a sixth from the fibres and fascia of the transversus colli muscle. Those from the three upper and part of the fourth digitations (*a*) are inserted into the usual place at the superior angle of the scapula. The two and a half lower digitations are inserted as a separate muscle (*b*) into the vertebral border of the scapula as low down as the spine, and are intimately connected by their deeper surface with the fibres of insertion of the *serratus magnus.* A muscular slip from its lower border is attached at its origin to the transversalis colli, and through it to the upper cervical transverse processes; at its insertion it is blended with that of the *rhomboideus minor* (*d*). The lower portion of the muscle may be considered as representing the first and second stages of that differentiation and transfer of insertion of some of the fibres of the levator anguli to the spine of the scapula which, in the *omo-trachélien* or *omo-atlanticus* of animals, attains to the acromion process, and in the *levator claviculæ* of the Quadrumana and Man reaches, on the same line of departure, to the clavicle. Macwhinnie records a slip of the levator anguli inserted into the spine of the scapula (*op. cit.* p. 194), a little further advance in the same direction, in the human subject.

Fig. 3 (Subject No. 20).



In two other male subjects (Nos. 9 & 10), also, the levator anguli scapulæ was connected by muscular slips with the hinder surface and upper fibres of the serratus magnus. In No. 10 it also received a slip from the *scalenus medius,* and in the female (No. 29) from the *scalenus posticus* of one side, and the *serratus posticus superior* on the other. In No. 34 it gave an interdigitating slip to the *splenius capitis* high up in the neck.

7. *Rhombo-atloid.*—In 3 male and 2 female subjects was found a distinct muscle arising with the fibres of the *splenius colli* from the transverse process of the atlas, usually about half an inch wide, and forming a muscular band which was placed superficial to the serratus posticus superior, and behind the rhomboideus minor, and inserted either into the upper fibres of the tendon of origin of the *rhomboideus major* or into the verte-

bral aponeurosis of the *serratus posticus superior*. In 1 male and the 2 females it was present on the left side only. One of them was subject 20 (given in fig. 3), where the abnormal muscle (*e*) is seen to pass under the rhomboideus minor (*d*), to be inserted into the serratus tendon (*f*) under the rhomboideus major (*g*), which is divided in order to show it. In subjects 2, 3, and 23 its insertion was entirely into the serratus aponeurosis. In Nos. 7 & 20 it was also connected with the spine of the seventh cervical vertebra. It has been described by Mr. Macalister as the *rhombo-atloid* (*op. cit.* p. 3, pl. 5. fig. 1, *a*). In Haller's 'Disputationum Anatomicarum Selectiorum,' vol. vi. (1733) p. 589, this muscle is described by F. Walther under the name of the "*Musculus singularis splenii accessorius*," or "*Adjutor splenii.*"

8. *Sundries.*—In subject 3 the *sterno-thyroid* was double—the abnormal part arising from the first rib-cartilage and costo-clavicular ligament, and joining the normal origin halfway up the neck. This abnormal slip bore much resemblance at its origin to the *costo-fascialis* described by the author in his former papers, differing only in its upper termination in the fibres of the sterno-thyroid, instead of the cervical fascia. The origin of the sterno-hyoid was placed between the two heads. In No. 4 was a muscular slip arising tendinous with the *anterior scalenus*, and inserted fleshy with the *medius*. In the male (No. 7) and the female (No. 33) well-marked specimens of the "*musculus glandulæ thyroideæ*" of Sœmmerring were found. The microscope proved satisfactorily that the structure of the formation was really muscular, and not median processes of the thyroid gland, as sometimes is the case. The slips appeared to be offsets from the inner fibres of the *thyro-hyoid* muscle, which had become attached to the gland-capsule. In the female (No. 26) the *thyro-hyoid* was divided into two distinct muscles, the inner one being the smaller, showing a tendency to the formation of the levator thyroideæ. In No. 9 the right *sterno-thyroid* was joined by a clavicular slip, round which the tendon of the omo-hyoid, as before described, played as through a pulley; on the other side the same muscle received a slip from the *sterno-hyoid*. The two sterno-thyroids also decussated across the median line by a considerable portion of their inner fibres. This arrangement is found in the Squirrel and some other Rodents. In No. 8 was a *cephalo-pharyngeus* muscle having the somewhat unusual origin from the spine of the sphenoid and spheno-maxillary ligament. At its insertion it blended with the lower fibres of the middle constrictor.

In the male (No. 11) and the female (No. 19) were found examples of the *sternalis brutorum* muscle. In both it was present on the right side only, and well developed. In the male it arose tendinous from the tendon of the sterno-mastoid, from the manubrium sterni 1¼ inch below its upper border, and from the fascia covering the pectoralis major muscle 2 inches below the clavicle. Its whole length was 3½ inches, and it ended below in a fleshy radiating way upon the fascia covering the sheath of the rectus and external oblique. In the female it arose tendinous from the manu-

brium sterni opposite the second rib, and had no connexion with the sterno-mastoid. It formed a slender muscular belly, 4¼ inches long by ⅜ of an inch wide, and ended upon the sheath of the rectus opposite the sixth rib-cartilage.

This muscle has been found by the author in 7 out of 175 subjects, in which it has been carefully looked for. This is 4 per cent. Five of the instances were in males, and 2 in females. In the males it was found, in 1 on both sides, in 3 on the right side, and in 1 on the left side only. In the females it was found in both instances on the right side only.

Professor Turner fixes the frequency of its occurrence, from observations upon 650 subjects, at about 3 per cent. He found it 9 times on both sides, 5 times on the right, and twice on the left side only; while in 5 more it was single, and crossed the median line (Journ. of Anat. and Phys. No. 2. May 1867, pp. 247, 248). Professor W. Gruber found it in 5 out of 95 subjects, in 3 on both sides, once on the right, and once on the left side only (Mem. de l'Acad. Imp. de St. Pétersbourg, L. iii. 1860).

Thirty-two columns of the Table are occupied by the remaining muscles of the *Arm*. This is six more than those of last year; the additional *lines of variation* being made up by the sterno-scapular, anconeus epitrochlearis, extensor carpi intermedius, ext. pollicis et indicis, and ext. medii digiti (which have been found so frequently as to require separation from the muscles with which they are most closely connected), and the pronator radii teres.

9. *Pectoralis major.*—In the male (No. 2) and the female (No. 25) a detached outlying slip was found on both sides at the lower border of this muscle, arising from the *epigastric aponeurosis* covering the rectus muscle, and inserted, separately from the rest of the pectoralis muscular fibres, upon the deep surface of the upper fibres of the tendon of insertion. In the male the slip was small on one side, and arose opposite to the sixth rib-cartilage; but on the other side large, and reaching as low as the seventh rib. The author looks upon the above abnormality as the homologue of the "*portion ventrale*" of Cuvier and Laurillard's plates, constituting, in most Mammalia, a large and separate portion of the pectoral group of muscles. It is also homologous with the "*costo-humeral*" of Professor Huxley, and the *chondro-epitrochlear* of Duvernoy. Meckel describes it as remarkably distinct in the Bats, drawing the wings powerfully down and inwards (Anat. Comp. vol. vi. p. 206). Zenker describes it as the "*brachio-abdominal*" muscle in the Batrachian reptiles, in whom it is frequently continuous with the rectus abdominis (Batrachomyologia. Jenæ, 1826, p. 39).

In the male (No. 13) was developed, on the left side only, a very large and well-marked example of the muscle described by the author as the "*chondro-coracoid.*" It is placed in the Table among the sundries. It consisted of a separate muscle arising by two digitations, the upper from the sixth rib, and the lower from the epigastric aponeurosis covering the

rectus, and formed a muscle 2 inches wide, while passing upwards and outwards across the axillary space, below and quite distinct from the pectorals, to be inserted by a broad tendinous expansion into the *fascia* covering the origin of the *coraco-brachialis* muscle as high up as the coracoid process. The pectoralis minor muscle was also very large and divided, and altogether the arrangement of these muscles resembled much that found in the *Felidæ* and Rodents. The *chondro-coracoid* portion forms, in the Norway Rat and Rabbit, a separate element distinct from the other pectorals. It seems to be an upward transition of the insertion of the epigastric slip above described, with which it coincides closely at its origin. In animals it is considered by some writers a part of the *panniculus carnosus.*

In 4 male subjects the clavicular fibres of the *pectoralis major* were fused with those of the *deltoid,* leaving only a small opening below for the cephalic vein. This arrangement has been recorded by Otto as an absence of the clavicular fibres of the deltoid, and by Seiler as an origin of the deltoid from the whole of the clavicle (Observ. Anat. 1808, fasc. i.). It indicates that kind of blending of the clavicular elevators of the upper arm which reaches its highest form in the lower portion of the cephalo-humeral muscle of the Carnivora and Rodents.

In the female (No. 20) the clavicular fibres of the pectoralis were split into two distinct muscles, one occupying the sternal, and the other the middle third of the bone. They were united at their insertion only.

10. *Pectoralis minor.*—In no less than 8 subjects, 4 male and 4 female, in 7 on both sides, and in 1 (male) on the left side only, a portion of the tendon of this muscle, usually the upper part, was prolonged over a pulley-like groove upon the *coracoid* process, and pierced the coraco-acromial ligament to be connected with the tendon of the supraspinatus muscle, and implanted with it into the capsular ligament of the shoulder-joint. In 2 of the males (Nos. 10 & 12) the tendon was also connected with the upper fibres of the *glenoid* ligament at its point of union with the capsular. In No. 10 the pectoralis minor was arranged in the complex manner shown in fig. 4. The fibres of the upper digitation (*a*), arising from the second rib and intercostal fascia, were joined to a roundish

Fig. 4 (Subject No. 10).

tendon which passed over the coracoid groove, gave off a slip to the coraco-acromial ligament (*e*) as it perforated it, and was implanted partly into the capsular ligament (*f*), and partly perforated it to join the fibres of the glenoid (*g*). The middle fibres (*b*), arising from the third and fourth ribs and fascia, were attached by a shorter tendon to the inner margin of the coracoid, and connected by aponeurosis to the origin of the coracoid muscles. The lower fibres (*c* & *d*), arising from the fifth rib and the adjacent intercostal and epigastric aponeuroses below the border of the pectoralis major muscle (with which they were partly blended), divided soon into two parts, of which the upper (*c*) was inserted into the fascia of the coracoid muscles (cut off short in the woodcut); while the lower (*d*) were united in a tendon which passed over these muscles, pierced the capsular ligament above the subscapularis (divided in the cut), and finally joined with the uppermost tendon to be inserted into the upper part of the glenoid ligament. A bursal opening existed at this point between the joint and the subscapular bursa. In the adjoining figure the pectoralis major, part of the deltoid, the coracoid muscles, and the subscapularis are partly removed, and the shoulder-joint opened to show the glenoid ligament.

In this curious instance the lower part of the muscular arrangement is evidently a "*chondro-coracoid*" muscle joined up to the pectoralis minor at two separate points, viz. at its origin and insertion. At its origin it coincides with the usual origin of this muscle, reaching down nearly as far as the latissimus dorsi; while at its insertion it is fused with the glenoid tendon of the pectoralis minor. Its course over the origins of the coracoid muscles quite coincides with a frequent insertion of the "*chondro-coracoid.*" It resembles considerably the intermediate pectoral of the lower animals.

This remarkable insertion into the glenoid ligament goes far to corroborate the views expressed by Macalister (Journal of Anat. and Phys. No. 2. May 1867, p. 317) upon the homology of his *coraco-glenoid* ligament with the humeral tendon of the pectoralis minor.

In another male subject (No. 13) the upper fibres of the lesser pectoral passed over the coracoid process to be inserted into the *coraco-acromial ligament*. In a female (No. 32) the upper fibres of the muscle on the right side were inserted by a flat aponeurotic tendon, half an inch wide, into the lower border of the *clavicle,* forming almost a separate *sterno-clavicular* muscle.

11. *Sterno-scapular.*—In 5 males (on both sides) and in 5 females (of which 2 were on both sides, 2 on the right, and 1 on the left only) it was found that a portion of the lower fibres of the *subclavius* muscle were implanted upon the tubercle of the *coracoid* process, and were usually separated by a distinct interval from the rest of the muscle, constituting a decided formation in most of the specimens of a *sterno-scapular* muscle, first distinguished and figured by the author in his paper of 1865 (fig. 4), and compared with the muscle of that name in animals.

In the subject there figured a muscle was found, coexisting with the sterno-

scapular (marked *b* in the above-mentioned figure), which has again been met with in the male subject (No. 4) of this year's series. On the left shoulder was found a distinct band of muscular fibres, nearly an inch in width, arising just outside of, and in connexion with, the *omohyoid*, from the base of the coracoid process, and inserted into the clavicle with the outermost fibres of the *subclavius* muscle. It has been named by the author the *Scapulo-clavicular* muscle, as it seems clearly to be the homologue of the muscle described and figured by Cuvier and Laurillard, under the name of the "*scapulo-clavien,*" in the Rat-mole of the Cape, and the *Didelphys marsupialis* or Sarigue (plates 216 & 195). The author has found the muscle also well marked, separate, and distinct in the Norway Rat (*Mus decumanus*), as well as in the Guineapig, and in a less distinct form in the Rabbit. The muscle is noted in the Table, in column 40, among the single specimens.

12. *Latissimus dorsi.*—In the male (No. 9) was a detached slip from the ninth intercostal fascia of the right side, which joined this muscle high up near its insertion. In the female (No. 29) was a musculo-tendinous slip, passing from the left latissimus across the axillary vessels, and inserted into the fascia covering the *biceps* muscle. It seemed to be a formation between the ordinary "*Achselbogen*" and a *dorso-epitrochlear* muscle. A similar slip, reaching only to the fascia covering the coraco-brachialis, was present on both sides in No. 32.

Out of 102 subjects, viz. 68 *males* and 34 *females*, a *dorso-epitrochlear* slip of muscular fibres has been found by the author in 5; 3 of which were males, and 2 females. In 2 of the males the slip was lost on or joined the scapular head of the *triceps* muscle. In the third male it was lost on the fascia covering the *coraco-brachialis.* All these were found in both arms. Of the 2 females, one was found on both sides, and ended on the fascia covering the *coraco-brachialis*, and the other on the left side only, and ended in the fascia covering the short head of the *biceps.* In 6 of the 102 subjects the latissimus gave off a considerable slip to the insertion of the *pectoralis major* (*Achselbogen*); 3 were in males, 2 on both sides, and 1 on the left side only; and 3 in females, 2 on both sides, and 1 on the left only. In 3 subjects the upper costal fibres of the latissimus were connected with a broad muscular slip arising with them, which, after crossing the axilla upwards and outwards, were inserted with the *pectoralis minor* into the coracoid process, or into the fascia of the coracoid muscles just below it (*chondro-coracoid*). Two of these were in males, one on the right side only (a similar slip on the left side joined the pectoralis major), and the other on the left only. The third was found in a female subject on the left side only.

13. *Coraco-brachialis.*—In 2 males (Nos. 1 & 10) and 3 females (Nos. 21, 23, & 24) this muscle presented an entirely double formation. The upper slip was inserted into the usual place; the lower passed down further, to be connected with the internal brachial ligament or intermus-

cular septum as far down as the ridge of the internal condyle. The musculo-cutaneous nerve usually passed between the two. The lower slip has been described by the author as a separate element, the *coraco-brachialis longus* (Journal of Anat. and Phys. No. 1. p. 49). This form of coraco-brachialis is found in the Ornithorhynchus, Echidna, and some Rodents. In the female (No. 30) this long muscle existed in the form of a distinct bundle given off from the middle of the coraco-brachialis, and ending in a long tendon which was implanted fairly upon the inner condyle, and which seemed to originate in a differentiation of the internal brachial ligament. In the female (No. 23) was found a good specimen of the muscle named by the author in the same paper the "*coraco-brachialis brevis*," or "*rotator humeri.*" It arose separately from the under surface of the coracoid process near its base, and was inserted into the neck of the humerus, just below the lesser tuberosity and above the latissimus dorsi. It is found usually in the Quadrumana, coexistent with the longer form, and in the Rodents and Carnivora singly.

In the male (No. 9) a large coraco-brachialis gave off a considerable bundle of muscular fibres, which joined bodily those of the *brachialis anticus.* This has been before found by the author, and also by Macwhinnie (*op. cit.*) and Hyrtl (Lehrbuch, S. 863). It is significant as the homologue of the *semimembranosus* in the lower extremity, supplying the homologous origin from the coracoid (ischium) as the brachialis supplies the homologous insertion.

14. *Biceps brachii.*—In 2 males and 6 females, out of the 36 dissected this session and given in the Table, this muscle was provided with a *third* or *humeral* head. In the left arm of No. 14 a slip of muscular fibres was given off from the lower third of the *coraco-brachialis,* and joined the short head about its middle. This seems to be merely a doubled or divided short head, of which the lower portion separates from the coraco-brachialis, lower down than usual, as we frequently find in the Quadrumana. In both arms of No. 17 the muscular slip, half an inch wide, was given off from the humerus just at the insertion of the coraco-brachialis. In the 6 other instances the third head arose, in the most usual place, with the upper fibres of the *brachialis anticus,* and joined the tendon at the same point as the two normal heads. In 1 (No. 24) it was found on both sides, in 4 on the left side only, and in 1 on the right side only.

Out of 175 subjects examined by the author, a *third* or humeral head of the *biceps* was found in 14, viz. in 7 *males* and 7 *females.* In 4 others, viz. 3 males and 1 female, it was found coexistent with a *fourth* head, also arising from the humerus. In 3 of these the fourth head arose from one of the tuberosities or the borders of the bicipital groove; and in the fourth, from the outside of the humerus, between the insertion of the deltoid and the origin of the supinator longus. This gives a proportion of about 1 in 9 out of the 175 subjects, agreeing nearly with the computation of Theile, viz. 1 in 8 or 9, and not with that of Hallett, viz. 1 in 15.

In the right arm of the male (No. 10) (noted in column 15 in the Table) was found a *brachio-radialis* as a detached muscle (fig. 5, *a*) of considerable size, arising separately from the upper part of the outer condyloid ridge of the humerus and intermuscular septum, just below the deltoid (*d*) and above the supinator longus (*c*). Passing as a flat muscular band downwards, forwards, and inwards, outside the biceps (*b*), it was inserted by a flat aponeurotic tendon into the *oblique line* of the *radius*, close below the bicipital tuberosity, and between the insertions of the supinator brevis (*e*) and pronator radii teres (*f*). Although quite detached from other muscles, and inserted into the radius below the biceps, the homological relation of this muscle, in the situation of its origin from the humerus, to the fourth head of the biceps just alluded to (recorded in the author's paper of 1864), is clearly apparent; and it holds the same relation to that *external* humeral head of the biceps as the detached *brachio-fascialis*, described in the author's papers of 1864, 1865, and 1866, does to the *internal* humeral head of the same muscle. Meckel, quoting Pietsch (Journal de Roux, t. xxxi. p. 245), mentions an instance in which *three* humeral slips, one from the outside of the humerus (homologous with the foregoing abnormality), one from the inner side (homologous with the more common third head), and one from the short or coracoid head of the biceps, joined together to form one muscle, which was inserted into the radius by a separate tendon, behind the normal one, upon the bicipital tuberosity (Muskellehre, S. 504). He also quotes Rudolphi (in Blumenbach's Med. Bibl. Bd. i. S. 176) and Sels (Diss. Anat. Musc. Variet. sistens, 1815, p. 12) for an instance in which a muscular bundle of the size of a finger passed from the outer head of the biceps to be inserted separately into the radius. The more perfectly detached form of this abnormality is also alluded to by Theile (in Sœmmerring's Encyclop. Anatomique, Jourdan's Trans. 1843, p. 217), by R. Wagner (in Heusinger's Zeitschrift, Heft iii. Bd. iii. S. 345), and by Hyrtl (Lehrbuch). A case very similar to the present has been described by W. Gruber (in Müller's Archiv, 1848, S. 428) as a variety of the *brachialis anticus*, in the right arm of a very muscular male, in whose left arm was found the more common form of the third head of the biceps. It arose from the humerus, close to the outer condyloid ridge, as a thick muscle, and was inserted by a separate tendon into the radial tuberosity just below the biceps, giving off a slip of tendon to the aponeurosis of the forearm.

Fig. 5
(Subject No. 10).

15. *Brachialis anticus.*—In both arms of the male (No. 6) some of the fibres of this muscle were directly continuous with those of the *supinator longus*, an ape-like arrangement before noticed by the author in his paper of 1866. He has found it in 6 males on both sides out of 102 subjects. It is recorded also by Macalister (*op. cit.* p. 19). Slips from this muscle to the outer part of the fascia of the forearm have been mentioned by Sœmmerring, Theile, Macwhinnie, and Hyrtl. In the female (No. 22) the brachialis anticus was divided into two muscles, an outer and an inner, both inserted into the coronoid process. This has been observed by the older anatomists. It resembles the arrangement in the Rabbit and Agouti.

16. *Anconeus epitrochlearis.*—In his paper read before the Royal Society in June 1866, the author mentioned and tabulated a detached muscular slip in the right elbow of a male subject, arising separately from the back part of the inner condyle of the humerus, and passing across and superficial to the ulnar nerve, and distinct both from the triceps and flexor carpi ulnaris muscles, to be inserted into the inner side of the *olecranon* process of the *ulna*. This muscle he again described and figured in his paper of last year, comparing it with a like muscle he had found in the Rabbit. During the last session this muscle has been found in 4 male subjects out of the 36—in 3 in both arms, and in 1 in the left arm only. The author has also found the muscle in his dissections of the Orang, Bonnet-Monkey, Mole, Hedgehog common Weasel, Cat, Norway Rat, Squirrel, and Ornithorhynchus. In the Mole it is particularly large and well developed, as are all the muscles of the upper arm. It is also well marked in the Norway Rat and Rabbit. In the subjects of the Table of the present year it has been very carefully looked for, with a view to determine the frequency of its occurrence, and to compare the results with those stated by Professor Gruber of St. Petersburg, in a paper published in the 'Mémoires de l'Acad. des Sciences de St. Pétersbourg,' in June 1866. This eminent observer states that he has found this muscle (*epitrochleo-anconeus*) as frequently as in 34 per cent. of subjects—in 26 out of 79 males, and in 8 out of 21 females. In 14 it was on both sides, in 12 in the right, and in 3 in the left arm only. It seems, therefore, to be more commonly found in the Sclavonic than in the Anglo-Saxon races. Professor Gruber figures and minutely describes this muscle, both in the human subject and in many animals—in *Inuus nemestrinus*, *Cebus fatuellus*, *Galeopithecus*, *Myogale*, *Dasyurus viverrinus*, Arctic Bear, Lion, Cat, Hare, 3-banded Armadillo, Seal, and many others. It seems to correspond with the muscle described in the Hyrax by Mivart and Murie as a fourth head of the *triceps*, and in the Rabbit by Krause as the *anconeus quartus*. It is figured, but not distinguished from the other anconeal muscles, in Cuvier and Laurillard's plates of the Panther, Genet, Beaver, Marmot, Rat-mole, Great Anteater, Elephant, and many other animals of various genera.

17. *Pronator radii teres.*—In 2 males and 2 females this muscle was

doubled or split along nearly its whole length into a *condyloid* and *coronoid* muscle, the latter being inserted higher than, and external to, the former part. In No. 1 the coronoid origin was a rounded tendon. In the female (No. 35) the two parts were distinct throughout, and connected only by a small slip at their insertion. In No. 31 the condyloid origin of the pronator was the only one present. The occasional occurrence of a double pronator has been noticed by Albinus, Sœmmerring, Theile, and Meckel. Mr. Macalister has lately called attention to this formation as an evidence of a second or accessory embryonic germ, represented by the coronoid origin, and homologous with the tibial head of the soleus (Journal of Anat. and Phys. Nov. 1867). The coronoid origin is not found in the lower Mammalia, and is present only in the higher Quadrumana. It was found by the author large and well marked in the Orang, arising by a strong, broad tendon, common to it, the flexor carpi radialis, and the flexor sublimis digitorum, and giving off a slip to the separate flexor indicis, and with the median nerve passing between it and the condyloid origin. In the same animal there was no tibial origin whatever to the soleus. The coronoid head of the pronator was not found by the author in the Bonnet-Monkey. Dr. Humphry found it in the Chimpanzee disposed as in the human subject.

18. *Flexor sublimis digitorum.*—In the left arm of a male (No. 4) was an unusual fusiform muscular slip from the coronoid origin of this muscle, ending in a long tendon which, passing under the annular ligament to the palm, gave origin to the outer half of a bipenniform *first lumbricalis* muscle. The inner head of the lumbrical arose from its usual place. This abnormality was described in the arm of a negro in the author's paper of 1865, the long tendon arising in that instance from the deep fibres of the sublimis, along with a coronoid "*accessorius ad flex. long. pollicis,*" and was joined in the lower part of the forearm by a muscular slip from the radius. In No. 7 the tendon to the middle digit was double. In Nos. 11 and 17, males, and in 20, 30, 32, 34, and 36, females (7 in all), the origin of the flexor sublimis from the coronoid process was twofold, viz.:—one, fleshy, from the upper part of the inner border of the process, and continuous with the fibres of the condyloid and ligamentous origins; and a second, flat, tendinous, and riband-shaped, from the lower angle of the coronoid process—the latter joining the fibres of the radial origin before these united with the rest. In all but 2 this arrangement was on both sides.

In 2 females (Nos. 19 and 22) the sublimis tendon to the little finger was absent. In the latter the perforated tendon was supplied by the fourth *lumbricalis* muscle. This arrangement forms a contrasted instance to that just mentioned, in which the first lumbricalis took origin from a tendon supplied by the sublimis. In the left arm of the female (No. 20) a musculo-tendinous slip was given off from the *sublimis* to the palmar fascia, in aid of a very feeble *palmaris longus*. Such a slip has been found by Macalister in *Cebus capucinus*. Rosenmüller has also described it in the human sub-

ject (*op. cit.* S. 6). In the opposite arm both the palmaris longus and the substitutory slip from the sublimis were wanting. In the female (No. 23) a separate digastric muscular belly was provided for the perforatus tendon of the index, while the rest of the muscle was much divided. In No. 31 a like digastric muscle gave gave off the perforatus tendons to both the index and little fingers. This was exactly like the instance described in the author's last paper. It has been recorded also by Meckel, and lately by Macalister. It is occasionally found in the Quadrumana.

19. *Flexor profundus digitorum.*—In no less than 4 males and 6 females out of the 36 subjects of the present year, and in 19 subjects out of a total of 102, was found the rounded, tapering, muscular slip arising with the condylo-coronoid origin of the sublimis, and joining, either by fleshy fibres or by a long tendon, some part of the *flexor profundus* or its tendons. It was observed by Gantzer, and named by him the "*accessorius ad flexorem profundum digitorum.*" In two males (Nos. 2 and 9) it joined that part of the profundus which supplied the *index* finger. In No. 9 it came off from the coronoid in common with a like slip to the flexor longus pollicis. In seven subjects it joined the second tendon of the profundus, viz. that to the *middle* digit; and in one female (No. 31) it was large, and ended in a long and good-sized tendon, dividing at the wrist into separate tendinous slips to the *three* inner digits, presenting almost the appearance of an intermediate common flexor, homologous with that which constitutes the chief bulk of the combined flexor muscles in the Carnivora and other Mammalia. It was similar in many respects to the arrangement in the Negro before alluded to. The author has found the slip of connexion between the coronoid fibres of the *sublimis* and the *profundus* in the arm of the Orang-outang. It is slender, and joins that part of the profundus which becomes differentiated into a *flexor indicis* in this animal, as well as in the Gorilla and Chimpanzee. The author has also found the same slip in the *Macacus radiatus*, arising musculo-tendinous with the sublimis, and uniting just above the wrist with the combined tendons of the flexor profundus and longus pollicis, just before the tendinous slip to the thumb is given off. In *Nycticebus tardigradus*, or Slow Loris (in which animal the common flexors are still distinct as in Man and the higher Quadrumana), a slip of tendon from the sublimis unites with the profundus above the carpus, and joins also the flexor pollicis (Mivart and Murie, Proc. Zool. Soc. Feb. 1865, p. 24). Meckel also describes this in the Loris. It is also found in *Cheiromys*, according to Owen, and in *Tarsius*, as described by Burmeister, showing in these animals a more decided tendency to the more complete amalgamation and substitution found in animals lower in the scale. In the Hedgehog the author has noted its presence in a more decided form, and still more largely developed in the Guineapig, Surmulot, and Rabbit, where it assumes more of the size and importance which it possesses in the Carnivora, in whom it constitutes the chief bulk of the combined flexors.

In the left arm of a male (No. 5) a considerable slip, amounting almost to

an equal division of the tendon of the *flexor longus pollicis*, joined bodily with the *indicial* tendon of the *profundus*, just at the point of origin of the first *lumbricalis* muscle, which arose equally from both by a double penniform belly. In the right arm of the same subject, a single penniform muscle, arising from the middle third of the front surface of the radius below the flexor longus pollicis, gave off a tendon which passed under the annular ligament behind that of the flexor longus pollicis, and ended by giving origin to the outer half of a bipenniform *first lumbricalis* muscle; but it did not form any other kind of junction with the indicial tendon of the profundus. The last abnormality has been observed by Theile and Henle. It has an evident relation to that on the left arm of the same subject, although the slip does not quite reach the tendon of the profundus; and it has also a more remote one to the slip before described from the *sublimis* to the bipenniform first lumbricalis—the connexion between the two in the latter being kept up by the blending of a *coronoid accessory* muscle of the *profundus* or *flexor pollicis longus* with the slip to the *lumbricalis*. In one of last year's subjects the tendon to the bipenniform lumbricalis came directly from the belly of the flexor longus pollicis, in place of having a distinct radial muscular belly. In the male (No. 7) the profundus itself gave off a long musculo-tendinous slip to the *second* lumbricalis. In the female (No. 19) that portion of the fibres of the flexor profundus which arises from the interosseous ligament, and gives rise to the indicial tendon, formed a distinct *flexor indicis*, an areolar interval extending along its whole length and separating it from the rest of the fibres of the profundus.

20. *Flexor longus pollicis.*—In 27 out of the 36 subjects, viz. 13 males and 14 females, this muscle received a fleshy, fusiform coronoid origin, the "*musculus accessorius*" of Gantzer. Sometimes it was distinct from the coronoid fibres of the sublimis, but was more frequently blended with them. In both arms of the male (No. 5) the accessory muscle received an additional muscular slip from the *condyloid* fibres of the sublimis, the fibres of which were continuous with those of the tendon before described as passing to the first lumbricalis muscle. This coronoid or condyloid slip has been found by the author in 40 subjects out of 102, viz. in 22 out of 68 males, and in 18 out of 34 females. It would thus seem to be more common in the latter sex. In 13 of the males and 12 of the females it was present in both arms, in 4 males and 4 females in the right arm only, and in 5 males and 2 females in the left only. The condyloid origin occasionally found forms a still closer homology with the chief or condyloid origin of the combined flexors of the lower animals. In 3 subjects, viz. 1 male (No. 13) in both arms, and 2 females (in No. 28 in the right arm, and in No. 32 in the left only), the flexor longus pollicis tendon subdivided into two, the inner and smaller joining in two of them the indicial tendon of the perforans about the wrist—but in one (No. 28) having a distinct insertion into the outer part of the base of the ungual phalanx of the index finger, lying in

the sheath as a separate tendon. The former of these arrangements has been observed by the older anatomists, Fleischmann, Loschge, Gantzer, and Meckel. Both represent very closely the tendinous connexion usually existing between the long flexors of the toes as a slip from the flexor hallucis to the second, third, and sometimes the fourth tendons of the flexor communis. In his paper of 1866 (p. 235) the author described a variety in which the communicating slip passed in the opposite direction, viz. from the indicial tendon of the profundus to the flexor longus pollicis, an arrangement which is found in the Gorilla, according to Duvernoy, and in the Chimpanzee as described by Professor Wilder (Boston Journ. of Nat. Hist. vii. 364). This resembles an occasional variety in the communicating slip between the homologous tendons in the foot of the human subject.

21. *Lumbricales manûs.*—Besides the abnormal origin of the *first* lumbricalis before alluded to, there were 8 other irregularities affecting these muscles in the 36 subjects. In Nos. 5 and 10, male, and No. 36, female, the *second* lumbricalis was bipenniform in its origin from the contiguous sides of the first and second perforating tendons. In the left arm of Nos. 4 and 18, and in both arms of No. 8, all males, the *third* lumbricalis was bifurcated, with a double insertion into the extensor aponeurosis of the third and fourth digits. This was the case also in the left arm of No. 7, while in the right arm, both the *third* and *fourth* lumbricales were bifurcated and inserted into the contiguous sides of their opposing digits. In the left arm of No. 18 the *fourth* lumbricalis was absent. In the left arm of No. 17 a slip of communication was observed between the origin of the *first palmar interosseus* and the middle of the *first* lumbricalis, which thereby assumed the appearance of a bipenniform muscle, an abnormality which does not appear to have been before recorded. All these subjects but one were males; 4 were found on both sides, 3 on the left only, and 1 on the right only.

Out of 102 subjects, viz. 68 *males* and 34 *females*, examined by the author, some of the *lumbricales* of the hand have been found abnormal in 19. In 4 of these, abnormalities of two kinds occurred, making altogether 23 specimens; 15 were males and 4 only females, giving a proportion of twice as frequent in the male sex. 8 were found on both sides, 8 on the right side only, and 7 on the left only. The *second* was doubled in its insertion once only, in the left hand; and was bipenniform in its origin in two instances. The *third* was bifurcated and double at its insertion in 10 instances, in 6 on both sides, 2 in the right, and 2 in the left hand only. It was bipenniform in its origin in 2 subjects, in both hands. The *fourth* was doubled in 2 instances, 1 in the right and 1 in the left hand. In one female it supplied, in both hands, the perforatus tendon of the fifth digit. Once it was inserted into the ulnar side of the ring-digit instead of the little finger, as has been observed by Moser and Theile; and in three instances it was totally wanting—once on the right and twice on the left side. The last abnormality has been recorded by Sœmmerring and Meckel. The above figures are considerably smaller than those given by Froment (Recherch. sur plusieurs

points d'Anatomie, 1853), who found some of these muscles abnormal in 45 per cent. of subjects. The proportionate frequency of the several muscles affected, however, is much the same. He also found the *third* to be most frequently affected, as had been observed indeed by the older anatomists, Petsche, Walther, and Heister (in Haller's Disput. Anatom. Select.), as well as by Meckel and Theile.

22. *Flexor carpi radialis.*—In three males and two females, two of the males on the right side and the rest on the left only, the tendon of this muscle gave a slip of insertion into the trapezium before being implanted upon the base of the second metacarpal bone. This has been recorded by Albinus, Loschge, Fleischmann, Theile, and Hyrtl, and is mentioned by Henle (Muskellehre, S. 191).

Flexor carpi radialis brevis seu *profundus**.—In the left arm of a female (No. 28) was found a small specimen of the variety of abnormality described by the author under this name. It arose from the oblique line of the radius, under the origin of the flexor sublimis, by a falciform aponeurosis, with a fusiform belly $1\frac{1}{2}$ inch long, and was inserted by a round tendon into that deep process of the annular ligament which is implanted upon the ridge of the trapezium and trapezoid, enclosing the groove for the tendon of the flexor carpi radialis. In the left arm of No. 32 (also a female) a large specimen of the same muscle existed, arising aponeurotic from the oblique line and outer border of the radius, with a fusiform belly ending in a round tendon which crossed deeply and obliquely across that of the flexor longus pollicis, close upon the wrist-joint, to be inserted in a fan-shaped way into the head of the os magnum, almost but not quite reaching to the base of the middle metacarpal bone. This specimen supplies a connecting link between the fusiform muscle attached to the annular ligament of No. 28 (which it resembled in its shape and origin) and the complete flexor of the middle metacarpal bone described by the author in former papers, which it closely resembles in its insertion. In the same hand the *flexor carpi radialis* gave off a slip to the trapezium. In both arms of No. 28 the *palmaris longus* was wanting; but in No. 32 that muscle was present

* While this is going through the press, the author has been favoured by Professor W. Gruber, of St. Pétersburg, with the last and several back numbers of the 'Bulletin de l'Acad. Imp. des Sciences de St. Pétersbourg.' In the last he claims priority of discovery and publication in the matter of the above-named muscle, which he figured and described in three male subjects in 1859 (in tom. xvii. no. 28, of that periodical), and which he then named "*M. radio-carpeus*" and "*M. radio-carpometacarpeus.*" The author takes the earliest opportunity of acknowledging this priority as regards himself. He was aware of, and has frequently referred to evidences of Professor Gruber's industry in observation, but he did not recognize his exceeding merits as a discoverer till informed of them by the pamphlets above mentioned. In the last, Professor Gruber himself refers to the publication of an instance of the "*M. radio-carpeus*" by M. Fano in the 'Bull. de la Soc. Anat. de Paris,' in November 1851, with which he himself did not become acquainted before 1859!! The grounds upon which he claims priority in face of this are not convincing; but he objects to the name given by the author, and announces that the muscle shall from henceforward be called "*M. radialis internus brevis* (*s. minor*)!

and normal in the same arm, and fusiform in the opposite or right arm. In both the sex was female, and the muscle was found in the left arm only. In the 8 instances previously described by the author the sex in 7 was male; and in all, the muscle was found in the right arm only. The latter peculiarity was also present in 2 specimens, of which notes have been kindly forwarded by Mr. Macalister of Dublin, one of which was inserted into the third metacarpal and the other into the annular ligament. In a note sent to the author in March last, the same excellent observer favoured him with a description of a peculiar arrangement of the muscle which he had found in a female in the left arm only. The muscle arose by two heads, viz. the more usual one from the radius between the flexor sublimis and flexor longus pollicis, and the other, flat and aponeurotic, from the internal condyle of the humerus. These joined in a muscular belly 4 inches long, which lay deeper than that of the flexor carpi radialis, and outside of the flexor longus pollicis, and was inserted by three slips of tendon into the deep surface of the annular ligament. The *palmaris longus* was also present, and normal in this case.

23. *Palmaris longus.*—In three males (Nos. 1, 6, & 15) this muscle and its tendon were both double, in the first two in the left arm only. In the last it was found in the right arm only—the supernumerary muscle being almost median and fusiform in shape, and the tendon of insertion reaching only to the annular ligament. In both arms of a male (No. 17) its tendon was split up into several parts, all inserted into the annular ligament closer than usual to the scaphoid. In both arms of No. 16 its belly was fusiform and nearly median, and its tendon of insertion gave off a slip to the origin of the *abductor pollicis.* In the left arm of the females (Nos. 31 & 34) the muscular belly was also fusiform and median in its position. In the latter, the tendon of insertion was double. In four, all females, the palmaris was found entirely wanting—in No. 28 on both sides, in Nos. 20 & 31 in the right arm only, and in No. 36 in the left only. In the right hand of the last, the *palmaris brevis* was also absent. No slip of substitution was found in any of these instances. In the right arm of the male (No. 13), and in the left of the female (No. 29), the only representative of this muscle was a feeble rudimentary tendinous slip.

Out of 102 closely observed subjects, viz. 68 *males* and 34 *females,* 23 have presented abnormalities of this muscle. In 7 of these they have varied in the two arms, presenting altogether 31 instances; 6 were double muscles, and 3 double tendons; of these, in 7 males and 2 females, 1 was in both arms, 2 in the right, and no less than 6 in the left arm only. It has been altogether absent in 9 instances, and rudimentary in 2, viz. in 4 males and 7 females; of these, 4 were in both arms, 4 in the right, and 3 in the left arm only. The number of absent muscles was thus nearly double in half the total number of females, giving a proportion of 4 to 1 of frequency of absence in this sex. In 5, viz. 3 males and 2 females, the muscular belly was median or inverted, in 2 on both sides, 2 in the right arm,

and 1 in the left only. In 1 male and 1 female only was the *flexor carpi radialis brevis present* when this muscle was *absent* or rudimentary. In the left arm of one female a substitutory slip came from the *flexor sublimis digitorum*. In 3 males, in 2 on both sides, and 1 on the left only, such a slip was derived from the *flexor carpi radialis*.

24. *Extensor carpi radialis longior*.—In 2 males and 1 female, twice in the right, and once in the left arm, this muscle was entirely blended at its origin with its twin muscle the *brevior*. In the male (No. 11) the coalesced muscle gave off two tendons, which had the usual insertions of the longior and brevior. In No. 2 it gave off three tendons, the middle one being that of the muscle called by the author the *extensor intermedius*, which was inserted with the brevior. In the female (No. 32) there arose in the left arm, from the combined muscular belly, no less than *four* tendons, of which the inner, corresponding to that of the *brevior*, was the largest and subdivided into two (making *five* in all), the superficial one of which was inserted into the bases of the second and third metacarpals, and the deeper into that of the third only. The outermost tendon, representing the *longior*, was inserted into the base of the second metacarpal; while the two *intermediate* tendons united at the lower end of the radius, to be inserted together, on the inner side of the *longior*, into the base of the *index*-metacarpal. This *coalescence* of the fleshy bellies of the *longior* and *brevior* has been alluded to as an absence of the *extensor brevior*, with the *longior* supplying two tendons, by Meckel (Muskellehre, S. 509), quoting Albinus (Hist. Musc. p. 446), and Salzmann, and also by Macwhinnie and Henle. Theile mentions it as a union of the *longior* and *brevior* (*op. cit.* p. 226). Meckel remarks upon its resemblance to the arrangement in the lower animals, and after him Macwhinnie and Henle. The construction in the female (No. 32) somewhat resembles the formation in the Ruminants; that in the male (No. 2) finds its counterpart in the Hyæna and Brown Bear. In the right arm of No. 7, and the left of No. 12, the tendon of the *longior* was split into two, both having the normal insertion.

In the female (No. 21) the *longior* gave off a large muscular slip to join the *supinator longus* high up. This resembles the doubleheaded supinator longus found by Mivart in the *Iguana tuberculata*, the second head of which arose with the extensor carpi radialis (Proc. Zool. Soc. June 1867, p. 783).

Extensor carpi radialis accessorius.—The above-described doubling of the tendon of the *longior* forms, apparently, the first point of transition to the abnormality found in no less than seven subjects (viz. Nos. 4, 6, 10, 15, & 17, males, and Nos. 27 & 29, females). In these a slip from the outer side of the tendon of the longior had a detached insertion into the base of the *pollex-metacarpal* and into the *first dorsal interosseous* muscle. Such slips have this session been very closely looked for as intermediate transitional forms of the muscle which has been described and figured by the author in former papers as the *extensor accessorius*. In the male (No. 4)

the insertion of this outer tendinous slip from the longior was found in both arms, and was so significant that it has been chosen as the subject of fig. 6. Leaving the outer side of the *longior* tendon and but slightly inferior to it in size, just above the radial styloid process, it crossed the depression of the "*tabatière*" under the extensor tendons of the thumb, and reached the first interosseous space. There it subdivided into two slips, the outer one of which was inserted into the inner part of the base of the *pollex*-metacarpal (*a*); and the inner, spreading out into a sort of aponeurosis, was first attached to the base of the *index*-metacarpal (*b*), and then passed into the united origins of the *abductor indicis* (*interosseus prior indicis* of Albinus) (*c*), and of an *interosseus primus volaris* of Henle (*d*). The first dorsal interosseus was entirely divided into two muscles, of which the posterior (*e*) arose from the contiguous metacarpals quite distinctly from the deeper muscle (*c*), which also arose by a bifurcated origin, one from the index-metacarpal, and the other from the slip of the *accessorius* tendon under description, in common with the *interosseus volaris*. In the figure, the dorsal portion is cut off close to its two origins to show the deeper part. In the same arm was observed an extensor-*intermedius* tendon, also leaving that of the *longior* (*f*), rather higher than the *accessorius*, and joining that of the *brevior* (*g*) at its insertion into the second and third metacarpals. The thumb-extensors are in the figure cut off close to their origins and insertions.

Fig. 6
(Subject No. 4).

In this specimen we have clearly some light thrown upon the way of the formation of the anomalous *extensor accessorius*. The abnormal muscle is produced simply by lateral differentiation and displacement of the outer part of the muscle and tendon of the *longior*, a process which stops, in the specimen just described, at the tendon only.

In the left arm of the male (No. 6) a similar slip from the longior was inserted into the base of the *pollex-metacarpal*, and was continuous with the deep origin of the *flexor brevis pollicis*, there being no *interosseus volaris* present. In the right arm of No. 10 the same abnormal tendon was even larger than the normal one of the longior, and there were decided marks of a division of the muscular belly into a distinct muscle. The accessory tendon divided about an inch above its termination into an inner slip inserted into the outer tubercular projection of the base of the index-metacarpal, and an outer one which subdivided; one of the subdivisions, being inserted into the base of the pollex-metacarpal, gave part origin

to an *interosseus volaris*, the other becoming connected with the origins of the *abductor indicis* and deep head of the *flexor brevis pollicis*. On the opposite arm of the same subject, the *longior* had simply a double tendon, each part of which was inserted into the base of the index-metacarpal, one crossing under the other in a curious way. In the left arm of No. 15, and in both arms of No. 17 also, the tendon of the longior gave off a slip to the pollex-metacarpal. The female (No. 27) had a similar slip in the left arm, and No. 29 in the right arm. In the opposite arm of the former was a complicated arrangement of the *extensor intermedius*.

The foregoing *accessory* slips of the extensor carpi radialis longior would not be observed in a casual dissection of the part, the normal and abnormal parts of the tendon being closely applied to each other and divided by a mere chink. It is only by following closely the tendons to their ultimate insertion, and removing the dorsal interosseous fascia where it covers and conceals them, that the real insertion becomes apparent. Hence it appears that this abnormal slip, though now found to be not infrequently present, has never before been recorded. Macwhinnie mentions that the tendon of the longior is sometimes inserted partly into the dorsal fascia of the hand (*op. cit.* p. 191); and Heister (in Haller's Disp. Anat. Select. t. vi. p. 739) describes a *Musculus radiæus externus tricornis*, two tendons of which were inserted into the *first*, and the third into the *second* metacarpal bone. These may have been instances of the same formation.

The fully formed muscle and tendon of the *accessorius* was much more adapted to challenge attention; but after a careful and prolonged search among the works of the older anatomists (kindly placed within his reach by Professor Sharpey), the author has found that only one incomplete example has been recorded. The specimen referred to is described by G. Fleischmann (in Abhandl. der physikalisch-medicin. Societät zu Erlangen, 1810, Bd. i. S. 28, with a figure by Loschge, Tafel I. fig. 2). It was an example (found in both arms of a woman) of that variety of the muscle in which the tendon is not inserted at all into the pollex-metacarpal, but passes bodily into one of the muscular bellies of a double or divided *abductor pollicis brevis*. Such a specimen was figured by the author in his paper of 1854. The absence of any bony attachment to the pollex-metacarpal seems to have obscured the real nature of the muscle. It was called by Fleischmann "*der zweibäuchiger Abzieher des Daumens*," or "*abductor pollicis biceps*." It seems to have been the identical specimen obscurely alluded to by Meckel under that name (Muskellehre, S. 517), and mentioned by Cruveilhier under the head of "*abductor pollicis brevis*" as a double-headed abductor of the thumb. Henle also seems to have followed this indication of a digastric long abductor of the thumb (Muskellehre, S. 224).

In 175 subjects in which the author has had this muscle carefully

looked for, it has been found as a muscle and tendon, distinct from the extensor carpi radialis longior, in 6 subjects; viz. 5 *males*, in 3 in both arms, and in 2 in the right arm only, and in 1 *female*, where it was found in the left arm only. In Cuvier and Laurillard's plates of the dissection of the Common Seal (*Phoca vitulina*, pl. 19), is figured a slip of tendon from the single radial extensor of the carpus to the pollex-metacarpal, sending off a slip to join the insertion of the extensor pollicis. Humphry also describes the same slip as inserted simply into the pollex-metacarpal in that animal (Journ. of Anat. and Phys. May 1868, p. 306). In the great Anteater and Tamandua, Meckel describes a double or second supinator longus, of which the tendon of one is inserted into the ensiform bone and palmar fascia. This appears to be the homologue of the *accessorius*. The author has also found its homologue in the Ornithorhynchus and Echidna.

25. *Extensor carpi radialis intermedius.*—The muscle and tendon described by the author under this name, or its representative tendon passing between the *longior* and *brevior*, have been found this session in no less than 13 subjects. In the male (No. 11) and in the female (No. 22) it was represented by a muscular belly distinct from those of the longior and brevior, an arrangement which has been recorded by Albinus, Meckel, and Theile. In the left arm of the former subject, the muscular slip left the origin of the longior; and the tendon, after crossing between that of the longior and the radius, gave off a slip to the tendon of the brevior, and was finally inserted into the index metacarpal inside the longior. It was in the right arm of this subject that the radial extensors were blended, as before described. In the males (Nos. 2, 5, & 16) and in the females (Nos. 27 & 34), intermediate tendinous slips came off from both the longior and brevior (see fig. 7 *b*). A similar case is recorded by Bergman (Handschr. Notiz.) and quoted by Henle. In Nos. 2 & 5 these slips simply joined together to be inserted into the index metacarpal inside the longior. In the right arms of Nos. 16 & 27, the slips crossed each other without joining, to be inserted with the opposite longior and brevior tendons respectively. In the right arm of No. 34, two slips from the longior and one from the brevior united in a single tendon, which subdivided to be inserted with the longior and brevior respectively, as in the Ruminants. In the left arm of No. 27 the double slips united into one, which crossed under the tendon of the brevior to be inserted into the middle metacarpal bone on its *inner* or *ulnar* side. In four males (in Nos. 4 & 15 in both arms, and in 12 & 18 in the left arm only) and in two females (in the left arm of Nos. 30 & 34) the slip passed from the longior above to the insertion of the brevior below. This form of divergence has been recorded by Albinus and Sœmmerring. In another female (No. 23) the slip passed from the brevior above to the longior below.

Out of 102 subjects, viz. 68 *males* and 34 *females*, the *extensor carpi radialis intermedius* has been found complete or incomplete in 32, 19 males

and 13 females; *i. e.* a considerable majority proportionately in the latter sex. In 18 it was found in both arms; in 5 in the right; and in 9 in the left only.

26. *Extensor carpi radialis brevior.*—In 16 subjects out of the 36 of the present year, the tendon of insertion of this muscle was implanted upon the adjacent part of the base of the *second* metacarpal, as well as upon that of the *third,* its normal insertion. In two of these (Nos. 15 & 16) this insertion was made by a short but distinct slip. In all the others there was no division of the tendon at its insertion. In the right arm of the females (Nos. 29 & 30) the tendon gave off a slip from its inner side to the origin of the *third dorsal interosseous* muscle, showing a disposition to the formation of an insertion into the base of the fourth metacarpal, as recorded by Albinus (Hist. Musc. p. 446), and quoted by Macwhinnie. In the *Iguana tuberculata,* Mivart describes the single radial extensor as inserted by three tendons into the second, third, and fourth metacarpal bones. Thus, at intervals, there are found in the human arm, slips from the radial extensors to the four outer metacarpals, the extensor ulnaris providing for the fifth. In his paper in the first Number of the 'Journal of Anatomy and Physiology' (Nov. 1866), the author showed the occasional occurrence of a special *flexor* also for each of the metacarpals.

The above insertion of a slip of the brevior into the index-metacarpal is the first indication of that form of *intermedius* which passes *from* the brevior *to* the longior. The latter muscle and tendon, however, seem more prone to this fissuring or differentiation than the former.

27. *Extensor communis digitorum.*—In two subjects, abnormalities of this muscle resulted from a division of its muscular belly. In Nos. 5 & 13, males, it gave a separate belly to each tendon—as recorded by Albinus, Brugnone, and Meckel. In the female (No. 30) the indicial portion only was provided with a distinct belly,—as mentioned by Henle. In two males (Nos. 7 & 15) and three females (Nos. 24, 33, & 36) there was a multiplication of its tendons. In the female (No. 24) there were in both arms two tendons to the *middle* digit. In both arms of the male (No. 15) there were two to the *little* finger. In the left arm of the male (No. 7) there were two each to the *ring-* and *little* digits; while in the right arm were two each to the *index* and *little* fingers, and no less than *four* to the *ring*-digit. In the left arm of the female (No. 36) there were two each to the *index, middle,* and *ring*-fingers; while in No. 33 there were two each to the *middle* and *ring.* On the right hand of the last was a small fusiform muscle, ending in a tendon, which was implanted upon the fascia covering the first interosseous muscle. All these abnormalities find a parallel in those of the extensor longus digitorum pedis. The most noteworthy is the last, which resembles the slip given off from the tendon in the foot to one of the metatarsals, described in the author's last paper as resembling the formation in the Sloths and some Reptiles.

In the left arm of a muscular female (No. 27), a large slip of muscle

and tendon from the common extensor passed outwards, in the oblique groove of the radius, to be united with the tendon of the *extensor secundi internodii pollicis* at the base of the first phalanx (fig. 7 *a*). This very rare abnormality had its parallel also in the foot, in a slip from the extensor longus digitorum pedis to join the tendon of the extensor proprius hallucis, described in last year's paper. In both arms of a female (No. 25) a similar tendinous slip joined the tendon of the *indicator*. This also occasionally has its homologue in the foot in a double tendon to the *second toe*.

Fig. 7
(Subject No. 27.)

28. *Extensor minimi digiti.*—With only two exceptions out of the 36 subjects, this muscle presented a multiplication of its tendon; and in three instances a complete *double muscle* was present. In the latter instances, all of which were in males (Nos. 1, 14, & 17), and in both arms, the tendon of the abnormal muscle divided into two slips, one of which joined the extensor aponeurosis of the *ring-finger*. A double muscle has been found by the author in 4 males out of 68, and in 1 female out of 34, in all on both sides. In both arms of the male (No. 10) and the female (No. 34), and in the right arm of No. 32, the extensor of the little finger was provided with three tendons, one of which was furnished to the *ring-digit*. In the right arms of the male (No. 11) and the female (No. 20), and in the left arm of the male (No. 13), the muscle was provided with two tendons; one of which went also to the *ring-digit*. Thus, in 9 subjects, viz. 6 males and 3 females, out of the 18 of each sex, the *ring*-digit received a tendon from the extensor of the little finger, as well as one from the common extensor. In all the instances the latter was placed superficial to the former, constituting a close resemblance to the second or ulnar extensor muscle often met with in the lower animals.

Out of 68 males this insertion has been found in 9, and out of 34 females in 4; of these 9 (including 7 males) were found on both sides, 3 in the right, and 1 in the left arm only.

In 28 out of the 36 subjects in the Table, the tendon of the *extensor minimi* was simply doubled, *both* being inserted into the *fifth* digit. Eleven males and 8 females were so provided in both arms, 2 males and 1 female in the right arm only, and 6 females in the left arm only. This has been found to be the case altogether in 25 out of 68 males and in 18

out of 34 females in 102 subjects—a proportion of 43 per cent. In 32 it was in both arms, in 4 in the right, and in 7 in the left only. It seems to be the first point of transition of a slip to the ring-finger.

In the left arm of the male (No. 7), the extensor minimi digiti was entirely *wanting*. It was, however, amply substituted by three tendons from the common extensor.

29. *Extensor carpi ulnaris.*—In 5 males and 1 female this muscle sent forward a slip of its tendon to be attached to the *extensor aponeurosis* of the *little* finger. In 2 males and 1 female this occurred in both arms, and in three males in the right arm only. In another male (No. 6) and a female (No. 31), this slip was present, but reached only as far as the head of the fifth *metacarpal* bone, into the upper border of which it was implanted.

This curious homologue of the *peroneus quinti* of the leg has been found by the author in 12 per cent. of subjects,—viz. in 10 out of 68 males (7 of which were in both arms, and 3 in the right only), and in 2 only out of 34 females (in 1 in both arms, and in the other in the left only). Thus it is 2½ times as common in the male sex as in the female.

30. *Extensor ossis metacarpi pollicis.*—In all the 36 subjects of the Table, except 1 male and 2 females, this muscle was provided with two or more tendons. In the 72 subjects of the Tables of the present and last year it has been found to be the case in 49. In the male subject (No. 5) the division extended to the formation, in both arms, of two *distinct muscles,* the inner one of which was inserted by a single tendon into the base of the pollex metacarpal; and the outer was provided with not less than four tendons, three of which were also inserted into the same bone, while the other gave part origin to the *opponens pollicis.* In this subject the extensor primi internodii was present, and inserted, with the secundi, into the extreme phalanx. In both arms of two males, and in the right only of three more, there were *three* tendons to the muscle, of which, in two instances, two were inserted into the metacarpal, and the third into the trapezium, or gave part origin to the *opponens* or *abductor pollicis* muscles. In one subject, one of the tendons was inserted into the metacarpal bone, trapezium, and opponens respectively; and in another, into the metacarpal, opponens, and abductor respectively. In one, the tendon of the extensor primi internodii came also from this muscle. In both arms of two females and in the left arm of a third, two of the tendons went to the metacarpal bone, and the other to the trapezium and abductor, or to the latter only. In the opposite arms of those subjects in which *three* tendons were found on one side only, there were always *two* tendons—one inserted into the metacarpal, and the other into the opponens and abductor pollicis.

In 24 subjects, viz. 9 males and 10 females on both sides, 1 male and 1 female in the right arm only, and 2 females and 1 male in the left only, the tendons of the extensor ossis metacarpi pollicis were *two* in number.

In 7 of these, both the tendons were inserted only into the metacarpal. In 14, the supernumerary one was inserted into the trapezium also. In 8 of them, slips were given also to the origin of the opponens pollicis; in 3, to that of the abductor; and in 4, to both these muscles. In two instances only, the second tendon joined the opponens or abductor without being connected with the trapezium—much resembling the slip of the *extensor carpi radialis accessorius* before described, by producing a *digastric abductor* of the thumb. The latter has apparently been confounded by anatomists with the abnormality under description. In the Orang-outang, the author found the extensor ossis metacarpi pollicis provided with a double tendon, one implanted into the trapezium and giving origin to the opponens pollicis. In most of the lower species of Monkey and Mammalia its tendon is single; its differentiation is therefore an evidence of elevation of type.

31. *Extensor primi internodii pollicis.*—In six cases the belly of this muscle was blended indistinguishably with that of the extensor *ossis metacarpi pollicis.* From this sprang three or more tendons, one of which was inserted into the base of the first phalanx of the thumb. Three were in males, and three in females. Three were in both arms; two in the right and one in the left only. This arrangement has been found in 4 out of 40 males, and in 4 out of 30 females.

In 5 males, 1 in both arms, 2 in the right and 2 in the left only, the tendon of this muscle was inserted entirely into the *ungual phalanx,* either in conjunction with or by the side of that of the *extensor secundi.*

In 3 males and 2 females the tendon, though having chiefly its normal insertion, sent forwards a slip to the same destination. This last arrangement has been found in 12 subjects out of 70.

In the male (No. 7) there were two slips of tendons to this muscle in the right and three in the left arm; of which two on one side, and one on the other, passed forward to join that of the *extensor secundi.*

In three subjects both muscle and tendon were entirely wanting—in the female (No. 19) in both arms, in the female (No. 22) and in the male (No. 14) in the left arm only. Also in the left arm of the female (No. 30) the tendon only (apparently from abortive development) was represented by a slip reaching from the styloid process of the radius to the base of the first phalanx of the thumb, exactly as recorded in a subject in the author's last paper. On the right arm of the same subject the tendon was very feeble, and came entirely from among those of the extensor ossis metacarpi pollicis. The muscle and tendon have been found entirely, or almost wholly wanting in 2 out of 68 males, and in 3 out of 34 females, a proportion in the latter sex of three times as many as in the former.

32. *Extensor secundi internodii pollicis.*—In 4 females and 6 females the tendon of this muscle was double, both having the normal insertion. In 2 males and 2 females it was so found in both arms, in 1 male and 3

females in the right arm, and in 1 male and 1 female in the left only. In the left arm of No. 22, in which the extensor primi was altogether absent, the *secundi* sent off a substitutory slip to the *first* phalanx. In the left arm of No. 33, a slip from the *secundi* was given to the *first* phalanx, and one from the *primi* was sent forward to the *ungual* phalanx, forming a mutual compensation (see fig. 8, *c*, *d*).

33. *Extensor indicis.*—In the left arm of the female (No. 33), the subject of many abnormalities, this muscle was found to be quite doubled, a tendon from each going to the usual insertion. The tendon only was doubled in 3 males in both arms—and in 3 females, 1 in both arms, and 2 in the left only. In the left arm of a male (No. 15) the muscle arose from the radius, carpus, and interosseous ligament, and not at all from the ulna. In both arms of the female (No. 35) it arose in common with an extensor medii digiti, from which also the index received a third slip of tendon, in addition to those from the double indicator (see fig. 8 *a*). A double tendon or muscle to the indicator has been found in 11 out of 102 subjects, viz.:—in 6 males, on both sides; and in 5 females—2 in both, and 3 in the left arm only.

Fig. 8 (Subject No. 33).

34. *Extensor pollicis et indicis.*—In one male (No. 11), in the right arm, and in two females (No. 32 & 36), in both arms, were found the curious muscle first described by the author under this name. Arising between the extensor secundi and extensor indicis, from the hinder surface of the ulna and adjacent interosseous ligament and intermuscular septum, the muscle ends in a single tendon, which, passing in the common extensor sheath, divides on the carpus into two tendons. The inner is inserted either separately into the base of the *first phalanx*, or joins the *common extensor aponeurosis* of the index; while the other passes outwards to join either the *extensor primi* or *secundi internodii* of the *thumb*. In both the females its pollex-tendon joined that of the *secundi*, while its index-tendon joined wholly the *common extensor* in one, and sent part of its fibres to the base of the first phalanx in the other.

In the right arm of the male subject (No. 11), the origin of the muscle was peculiar and differed from all the other specimens. Instead of arising from the ulna with the other muscles, it arose more superficially

from the intermuscular septum, between the extensor communis and extensor ossis metacarpi pollicis, forming a layer intermediately placed, and appearing, at first sight, to be connected with the deep surface of the common extensor. Its pollex-insertion was with the tendon of the *secundi*; and at its indicial it joined the outermost of the tendons of a *double indicator* to be inserted into the base of the first phalanx. This origin of the muscle seems to form an intermediate link, and to connect it with the somewhat similar abnormality of the extensor communis digitorum in subject 27 (see fig. 7 *a*).

35. *Extensor medii digiti.*—In 2 males (1 in the right arm, and the other in the left), and in 4 females (2 in both arms, 1 in the right, and 1 in the left), this muscle was found arising in common with the *indicator*, the lower fibres of which gave off a tendon to be inserted into the base of the first phalanx of the middle digit. In 2 females, in both arms, it was found as a separate muscle arising, below the indicator, from the ulna and interosseous ligament. In the right arm of one of them (No. 28) the muscle had a peculiar origin from the intermuscular septum between the extensor communis digitorum and supinator brevis, higher and more superficial than the rest of the deep muscles. Forming a fusiform belly, it ended in a long tendon, which was inserted into the extreme or ungual phalanx of the middle finger, internal to the common extensor, crossing the fibres of insertion of the interosseus. This abnormality, again, has a resemblance to the variety of the extensor pollicis et indicis just described in the male (No. 11). In both arms of No. 33 (the subject of fig. 8) the muscle (*b*) was distinct and gave off two tendons, of which the inner was inserted into the base of the middle digit; and the outer subdivided into two slips, one to join the inner tendon of a *double indicator*, and the other to be inserted upon the *middle metacarpal* fascia. This hand showed a remarkable complexity and profusion of the special extensor muscles.

Out of 102 subjects this special *extensor* of the *middle finger* has been found 4 times in the 68 *males*, and 7 times in the 34 *females*, giving a proportionate frequency of nearly four in the latter sex to one in the former. It is a muscle constantly present in the Quadrumana.

36. *Extensor brevis digitorum manûs.*—In 3 male and 3 female subjects, slips of muscle were found on the dorsum of the hand, arising, distinct from the dorsal interossei muscles, from the os magnum or unciform bones, and inserted into the common extensor aponeurosis of one or more digits. In the right hand of No. 3, and the left of No. 7, males, and in both hands of No. 21, a female, there was but *one* slip, which was inserted with the tendon of the second dorsal interosseus into the *middle* digit. In both hands of No. 2, male, there were *three* slips, arising respectively from without inwards from the os magnum, unciform and cuneiform bones. The outer was inserted with the second dorsal interosseus into the *middle* digit—and the two inner on each side of the *fifth* digit, with the abductor and interosseus respectively. In the female (No. 23) there were *two* slips,

in both hands, one from the os magnum to the *middle* digit, and the other from the unciform to the *ring*-digit. In the female (No. 33), the subject of the last figure, the right hand, only, showed *three* slips, arising from the dorsal aspect of the bases of the second, third, and fourth *metacarpals* and their dorsal ligaments, and inserted into the *corresponding* digits.

In 68 males this muscle has been found by the author in 7, and in 34 females in 3—giving nearly an equality in the sexes. In looking over the works of the older anatomists, the author finds that short single dorsal slips to the index or middle finger had been observed by Albinus, and described by him as the "*Musculus extensor brevis digiti indicis vel medii*" (Acad. Annot. lib. iv. cap. vi. p. 28, and tab. v. fig. 3, 1734). A single slip from the carpus to the index is described by Gantzer as an *indicator biceps* (*op. cit.* p. 14), and similar ones by Otto as an "*indicator anomalus brevis*" (Seltene Beobacht. S. 91), arising from the radius in one instance, and from the third metacarpal in another. In two other male hands the last-named anatomist found a slip from the carpus to the middle finger, which he calls the "*extensor anomalus brevis des Mittelfingers.*" Sœmmerring, Petsche, and Sandifort have described slips which might be confounded with these, but which refer rather to the true *indicator* giving off an *extensor medii digiti.* In all of them, however, the slips were single, and did not form the broad flat muscle described by the author in his former papers. It is remarkable that they were all found in male subjects. The short common extensor of the digits is represented in the *Bradypus tridactylus*, in the two-toed Anteater, and in the Saurian, Chelonian, and Batrachian Reptiles, according to Meckel (Anat. Comp. vol. v. pp. 386, 388, & 391, and vol. vi. pp. 346 & 351, and Archiv, v. p. 47).

37. *Abductor pollicis.*—In 2 males and 2 females this muscle was divided into two portions, rather widely separated at their origins from the trapezium and annular ligaments respectively. In the 2 males and in 1 female this was the case in both hands, and in the other female in the left hand only. In the two males there was, in addition, a considerable muscular slip from the inner of the two to join the fibres of the *opponens pollicis* in their insertion.

38. *Abductor minimi digiti.*—In the right hand of the male (No. 2) a separate muscular head from the anterior annular ligament joined the tendon of this muscle at its insertion. In both hands of No. 3 the muscle was double, the *flexor brevis* being *absent.* In the left arm of No. 11 the muscle was arranged in two parts, viz. the normal origin, and a high origin 2½ inches above the wrist. The latter arose by two heads, one fleshy, from the fascia covering the *flexor carpi ulnaris*, and the other tendinous, from the tendon of the *palmaris longus.* These united above the wrist to form a fleshy muscle, larger than the normal origin, and placed external to it, which became united with it just before its insertion.

This abnormality has been found in 3 males only out of 102 subjects of both sexes examined by the author. It has been described by Sœmmerring

(*op. cit.* p. 272). It was also found by Günther and Milde (Die chirurgische Muskellehre, Taf. 30. fig. 5. 18). In the right arm of a muscular soldier Gantzer found a fleshy muscle connected with the insertion of the *abductor minimi digiti*, arising from the sheath of the *flexor-carpi-radialis* tendon, to which he gave the name of the "*accessorius ad flexorem carpi radialem*" (*op. cit.* p. 12), which was evidently a muscle of the same character as the foregoing. Macwhinnie mentions similar high origins of this muscle, arising from the tendon of the *palmaris longus*, as varieties of the last-named muscle (*op. cit.* p. 191). This abnormal upward extension of the origin of the abductor minimi digiti seems to correspond to the shortest of the three muscles representing the ulnar carpal extensor found in the Anteater (Meckel, Archiv, B. v. S. 45, *k*). In both hands of the female (No. 20) the muscle was provided with a double tendon of insertion; and in the right hand of No. 33 the whole muscle was divided into two parts.

39. *Interossei manûs.*—In both hands of 4 males and 3 females, and in the left only of one other female, the "*interosseus primus volaris*" of Henle was found. In No. 4 this muscle, as before described, was connected at its origin with an *accessory* slip of the *extensor carpi radialis longior* (see fig. 6 *d*). It has been found in 12 out of 102 subjects, usually on both sides. In the males (Nos. 3 & 4) the *first dorsal* was separated into two muscles, the *abductor* and *interosseus prior indicis* of Albinus.

40. *Sundries.*—Besides the *scapulo-clavicular* and *chondro-coracoid* muscles described in the former part of the paper, in subjects 4 and 13, this column marks in the male (No. 7) an abnormality of the *infra-scapular* muscle, which consisted in a separate origin and distinct superficial position of the fibres derived from the spine of the scapula. In the right arm of the female (No. 21) the *supinator longus* received a large muscular slip from the *extensor carpi radialis longior*, as described with the abnormality of the latter muscle. On the right side of No. 35 two large fleshy slips from the ninth and tenth ribs, and on the left side from the eighth also, quite separate and somewhat distant from the rest of the *serratus magnus*, were inserted into the lower angle of the scapula, with the lower fibres of that muscle, which reached no lower than the seventh rib. This differentiation of the lower fibres of the serratus resembles the *depressor scapulæ* muscle found in the Birds. The female (No. 36) was remarkable for the very rare absence, in the right hand, of the *palmaris brevis* muscle.

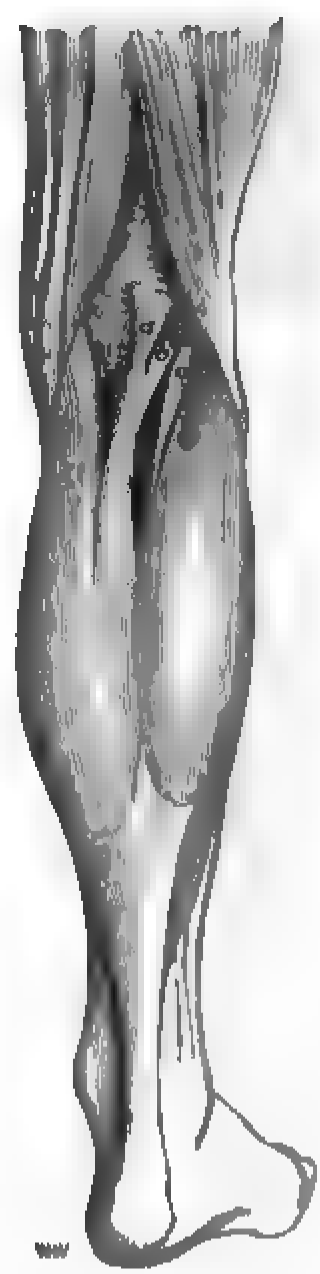
The remaining sixteen columns are occupied by the abnormal muscles of the *Leg*. The additional lines of variation are partly composed of muscles which have not been before especially regarded (such as the *pyriformis*, *gemelli*, and *opponens minimi digiti*) and partly of more numerous specimens of abnormalities presented by those which were before comprised in the column of sundries (as the *plantaris*, *peroneus brevis*, and *adductor hallucis*).

41 & 42. *Pyriformis* and *Gemelli.*—In 3 males and 1 female the tendon of the *pyriformis* was blended with that of the *obturator internus*

and inserted with it into the groove of the great trochanter. In one of these males (No. 4), and in the female (No. 19), the *superior gemellus*, which usually intervenes, was entirely absent on both sides, and in one other male (No. 17) on the right side only. In the other instances it was inserted into the common tendon. On the right side of the female (No. 19) the *inferior gemellus* was also absent. In two females the *pyriformis* muscle was divided into two parts, between which passed a portion of the great sciatic nerve. This is a frequent and striking abnormality usually noticed by anatomical writers upon the subject.

43. *Plantaris.*—In the right leg of the male (No. 3) a double muscular belly was found, both joining a single tendon rather larger than usual. In both legs of the female (No. 22) a muscular slip, nearly equal in size to the normal belly, passed from the inner side of its origin to be inserted upon the *posterior ligament* of *Winslow*, close to the insertion of the *semimembranosus*-tendon, and under the inner head of the gastrocnemius. This curious slip appeared to be an instance of a development of muscular fibres in the substance of a tendon, similar to that unusual one which is seen in the tendon of the *peroneus quinti* (fig. 10). In its origin and direction, however, it has some resemblance to the *third head* of the *gastrocnemius* muscle found in the left leg of the male (No. 7), and marked in the Table among the sundries (col. 56). It may, perhaps, be most convenient to describe it in this place. A fleshy tapering head of muscle of considerable size arose from the middle portion of the popliteal surface of the femur just above the condyles (fig. 9, *a*). Opposite to the knee-joint it was joined on the outer side by a broad tendinous slip (*b*), arising from the posterior ligament of Winslow close to the plantaris muscle (*c*). The two on joining, formed a considerable bundle of muscular fibres, which, increasing slightly as it descended, joined the *inner* head of the *gastrocnemius* just before its union with the outer. This abnormality resembled in some respects that described by R. Quain (plate 80. figs. 4 & 5), in which a third head, arising from the outer femoral condyle, crossed the space between the popliteal artery and vein, and finally joined the deep surface of the outer head. Henle also describes a third head, arising from the popliteal surface of the femur, and ending in a cylindrical tendon which spread out

Fig. 9 (Subject No. 7).



and united with the point of junction of the *soleus* and *gastrocnemius* (Muskellehre). Theile also met with such a supernumerary head (*op. cit.* p. 316).

44. *Flexor longus digitorum* and *lumbricales pedis.*—In 2 males (Nos. 4 & 12) and 4 females (Nos. 22, 27, 29, & 30) the tendon of the *flexor longus digitorum* gave a considerable slip in the sole to join that of the *flexor longus hallucis*, as well as received one from it. In 1 male it was found in both feet; and in the other, as well as in 3 out of the 4 females, in the right foot only; in the remaining female it was present in the left foot only.

This slip has been especially noted only in the present year, and returns an average of about 18 per cent. Schultze of Rostock found it in 29 subjects out of 100, and usually on both sides (Zeitschrift für wissenschaft. Zool. xvii. 1). Nearly the same proportion was found by Turner in 50 subjects (Edinb. Phil. Trans. xxiv.). The slip did not in our subjects seem to be in any way compensatory for any want of size in the tendon of the flexor hallucis; for in one female (No. 22) that tendon was unusually large even before the junction. In the female (No. 27) the flexor hallucis received also a slip of reinforcement from the *flexor accessorius.* In the foot of No. 29, female, the *perforating* tendon to the second toe was derived wholly from that of the *flexor hallucis*, the common flexor supplying only the three outer toes. In the right foot of the male (No. 11) the *perforating* common flexor of the third toe gave off a slip of reinforcement to the *perforated* opposite to the base of the metatarsus.

In the male (No. 1) the *fourth lumbricalis* was absent in both feet. In the left foot of No. 9 the same muscle was double and bifurcated, and inserted into the contiguous sides of the fourth and fifth toes. In No. 8 the *third lumbricalis* was double in both feet, and inserted into the contiguous sides of the middle and fourth toes.

According to Froment (*op. cit.*), varieties in the *pedal lumbricales* are very rare. Gantzer had previously stated the same thing (*op. cit.* p. 17). In the notes of Sœmmerring, quoted by Theile, Behrends is said to have observed the absence of the *two middle* ones. Rudolph, quoted by Gantzer (*op. cit.*), found the *third* absent in the left, and the *fourth* in the right foot of a muscular male.

Out of 102 subjects examined, the *second lumbricalis pedis* was found, in one, *absent* on both sides; in one, the *third* arose from the tendon of the perforatus instead of the perforans; in another, it was double on both sides; the *fourth* was *absent* in three subjects—in one on both sides, in one in the right, and in one in the left foot only; in one the same muscle, in both feet, was *doubled*, bifurcated, and inserted into contiguous sides of the corresponding toes. This gives a proportion of only 7 per cent. of abnormalities in these muscles, contrasting widely with the frequency of irregularity of the same muscles in the hand.

45. *Flexor longus hallucis.*—In both feet of a male (No. 17) the tendon of this muscle gave no slip to that of the *common flexor.* In both feet of

a female (No. 31) the slip was, on the contrary, unusually large. In the right foot of another (No. 27) it received a considerable slip from the *flexor accessorius*, as well as from the common flexor. In a third (No. 29) it supplied the whole of the *perforating* tendon to the *second* toe, after receiving only a very small slip of fibres from the common flexor.

46. *Extensor primi internodii hallucis longus.*—In all the 18 males except two, and in 13 out of the 18 females, the base of the *first* phalanx of the *great toe* was the point of insertion of a more or less considerable slip of tendon, described by the author in former papers under the above name. In all except three it existed in both feet, in two in the right foot, and in one in the left only, exhibiting an unusual uniformity in this respect.

In 3 males (Nos. 5, 7 & 11) the tendon was derived from that of the *tibialis anticus*, leaving it a little above the ankle.

This curious parallel to the *peroneus quinti* on the outside of the foot, had been previously found by the author in five other male subjects—in four in both feet, and in one in the right only. The frequency of its occurrence is in about 8 per cent. of subjects, although this variety seems to have escaped the observation of anatomists. In the male (No. 15) the tendon existed in the right foot only, as a well-defined and strong fibrous band connected with the anterior ligament of the ankle-joint, with a distinct insertion into the first phalanx of the great toe. It was totally unconnected with any other tendon. This instance presents a curious parallel to the like occasional abortive development of the homologous tendon in the hand, viz. the *extensor primi internodii pollicis*, which has been described in these pages as having been found, in two instances, connected solely with the styloid process of the radius, close to the posterior ligament of the carpus.

In all the rest of the subjects in which this tendon was found, it was an offset of that of the *extensor proprius hallucis*, and was inserted either singly into the middle of the dorsal border of the base of the first phalanx, or joined with the innermost tendon of the *extensor brevis digitorum* in the same insertion. Sometimes it was connected separately with the lower *muscular* fibres of the proprius, which showed, by a disposition to separate from the rest, a tendency to form a *separate* muscle, such as that described by the author in his former papers, and recorded by Meckel (Archiv, Bd. v. S. 117), and mentioned by Theile and Henle (Muskellehre, S. 277). The slip of tendon from the *extensor proprius* is recorded by Sœmmerring (*op. cit.* p. 323) and by Walther (in Haller's Disp. Anat. Select. vol. vi. p. 559).

47. *Extensor longus digitorum pedis.*—In No. 1, male, this muscle divided into four separate bellies, one to each tendon. The outermost was joined by two slips from that of the *peroneus tertius*, which was split into three parts. In the left leg of the male (No. 6) a tendon of reinforcement was given off in the opposite way, viz. from the outermost of the extensor longus to the middle of the upper border of the fifth metatarsal bone. In the right leg of the female (No. 35) the same abnormality occurred in the shape of two tendons to the fifth metatarsal.

Nearly the same arrangement was found in both legs of a male last year, making a proportion of 3 in 70 subjects. It was found, according to Meckel, in the Sloths and Reptiles.

In the female (No. 33) a slip was given from the second tendon of the extensor *longus* to that of the extensor *brevis*, as found in a male in last year's series. This was noticed by Macwhinnie (*op. cit.* p. 195) joining the great-toe tendon of the brevis.

48. *Peroneus tertius.*—In 15 subjects, viz. 8 males and 7 females, this muscle presented abnormalities of various kinds. The most conspicuous was its entire *absence* in the right leg of one male (No. 8) and two females (Nos. 21 & 23). In the left leg of the male its tendon was very small, and was lost upon the *fourth interosseous* fascia and muscle, not reaching the metatarsus.

Out of 68 *males* this muscle has been found to be absent in 5, and out of 34 *females* in 5 also, giving a proportionate *frequency of deficiency exactly double* in the *females*. In the 5 males it was wanting in 1 on both sides, in 3 in the right, and in 1 in the left leg only. In the 5 *females* it was wanting in 1 on both sides, in 3 in the right, and in 1 in the left leg only. It would thus seem to be absent most frequently in the *right* leg. In the only instance in which the left leg only is recorded, the representative in the right leg was a mere slip from the extensor-communis-digitorum tendon, which could scarcely be called a peroneus. This muscle is uniformly *absent* in the Quadrumana and all the lower animals, and appears to subserve especially the erect position peculiar, among mammals, to the human subject.

In both legs of the male (No. 1) and in the right leg of the female (No. 35) the tendon of the *peroneus tertius* gave off slips of reinforcement to the outermost tendon of the *extensor longus digitorum*, in front of the ankle. In both legs of No. 16 a similar slip joined that of the *extensor brevis*. In 5 males and 4 females it was provided with two tendons or split. In both legs of No. 4, and in the right leg of Nos. 5 & 9 (males), both the tendons were inserted separately into the fifth metatarsal bone, one close to its base, and the other about its middle. In the right leg of Nos. 7 & 9 (males), and No. 27 (female), in the left leg of No. 8 (male), and in both legs of No. 29 (female), one of the double tendons, or a slip from a single tendon, passed forward to join the *fourth* interosseous fascia or muscle. In both legs of No. 18 (male), and in the left of No. 19 (female), the tendon of the *peroneus tertius* was implanted wholly upon the *fourth* instead of the *fifth* metatarsal bone; and in both legs of No. 34 (female) it was *mainly* inserted into the *fourth*, giving off a slip only to the fifth.

Altogether in 102 subjects these varieties of *redundancy* were found to be *half as many again* in males as in females, contrasting widely with the greater frequency of *deficiency* in females before described.

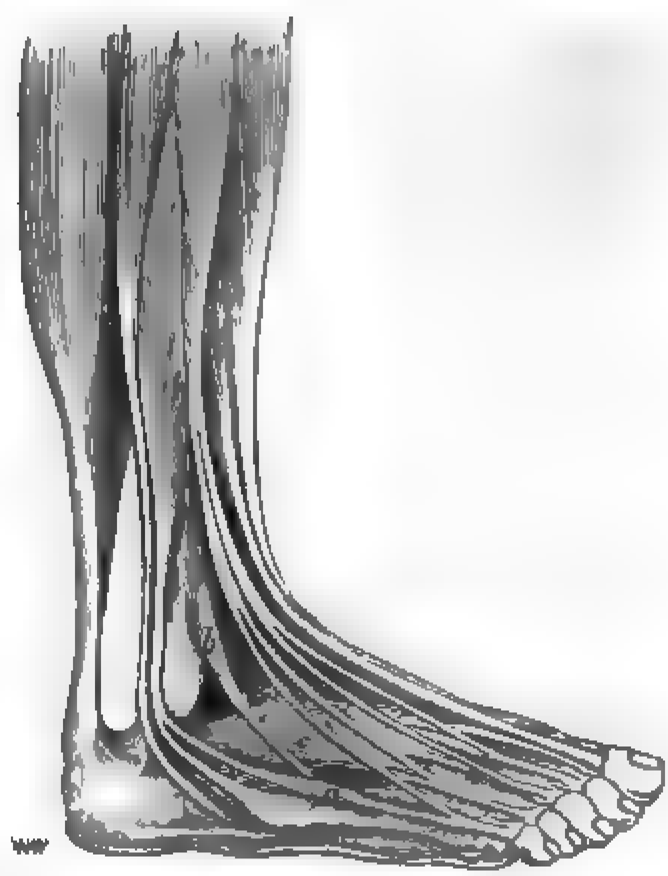
49. *Peroneus brevis.*—In the male (No. 3) the posterior fibres of this

muscle were connected with a distinct tendon, rather smaller than the normal one, which, passing in the groove and sheath of the *peroneus longus*, was implanted upon the outer margin of the *cuboid* bone behind its peroneal groove. A similar instance has been recorded by Macalister. It is of the same character as the *peroneus quartus* of Otto, which was inserted into the *calcaneum*.

The *peroneus quartus* has been found twice in 70 subjects by the author. Meckel describes it in the Kangaroo. In 1 male and 1 female the tendon of the *brevis* was found, in both legs, to give off a slip of reinforcement to that of the *tertius*.

50. *Peroneus quinti*.—This variety, constituted usually, in the human subject, by a slip of tendon from that of the *peroneus brevis*, given off below the outer ankle and joining the extensor aponeurosis of *the little toe*, was found in the male (No. 11) to have developed upon it a complete, distinct, and fusiform *muscular belly* (fig. 10 *a*), exactly like that described in the author's paper of 1864, and also by Hallett in his paper in the Edinburgh Medical and Surgical Journal of 1848. The peroneus tertius (*b*) was present and normal, and the tendons of the extensor brevis digitorum (*c*) complete.

Fig. 10 (Subject No. 11).



In 7 *males*—4 in both legs, 1 in the right, and 2 in the left only—the tendon of the *peroneus quinti* was complete, reaching to the extensor aponeurosis of the little toe. The same was the case in 4 *females*, 2 on both sides and 2 on the left only. In 2 *males* in both legs, and in 6 *females*, of which 4 were in both legs, 1 in the right, and 1 in the left only, the tendon, after parting from the peroneus brevis in the usual way, became spread out in front and lost upon the upper border of the *fifth metatarsal* bone about its middle—an incomplete arrangement, said by Mr. Davis to be found also in the Civet Cat (Journ. of Anat. and Physiol. May 1868, p. 217).

Out of 102 subjects, in the 68 *males* the complete muscle has been found once, the complete tendon in 18, and the incomplete tendon in 5, making 24 in the *males*. In the 34 *females* the complete tendon has been found in 5, and the incomplete form in 8, making 13 in the *females*. This gives about an equal average in the two sexes.

[redacted]

51. *Extensor brevis digitorum.*—In 4 *males* the tendon from this muscle to the *second* toe was found *doubled*; 1 was found on both sides, 2 in the right foot, and 1 in the left only. In 3 *females* the same arrangement occurred; in 1 in both feet, in 1 in the right, and in 1 in the left only. In 2 of the males (Nos. 4 & 8) the supernumerary tendon was inserted separately into the base of the first phalanx. In the rest it joined, like its fellow, the extensor aponeurosis. In the right foot of one male (No. 3) and one female (No. 22) the fibres of the muscle to the *second* toe gave a tendinous slip to the *first interosseous* muscle and fascia. This is doubtless an imperfect form of the same abnormality.

Out of 40 *males* the former arrangement has been found in 6, and the latter in 3; in 30 *females* the former was found in 3, and the latter in 1,—giving a total of 13 in 70 subjects.

52. *Abductor ossis metatarsi quinti.*—This muscle, first described and figured by the author in his paper of 1864, arising from the outer tubercle of the calcaneum, and inserted into the tubercle at the base of the fifth metatarsal bone, covered and concealed by the *abductor minimi digiti* and the insertion of the *peroneus brevis*, has been found this year in 19 subjects out of the 36, viz. in 11 out of the 18 *males* and in 8 out of the 18 *females*. Of the *males*, 5 were found in both feet, 3 in the right, and 3 in the left foot only. Of the *females*, 5 were found in both feet, 1 in the right, and 2 in the left foot only. It has been described by Professor Huxley and Mr. Flower in the foot of the Gorilla and Chimpanzee, and has been found by the author in that of the Orang-outang and Bonnet-Monkey; and also, lower in the scale, in the Cat, Hedgehog, and Squirrel he has found evidences, though less distinct, of its presence. It is figured by Strauss-Durckheim in the anatomy of the Cat. Mivart and Murie describe it in the Hyrax, and the former observer also in the *Iguana tuberculata*. In the Echidna also are fibres answering to this muscle. Meckel describes a similar muscle in the Makis, the Brown Bear, and the Coati.

Out of 63 *males* the author has found this muscle in 27, viz. in 19 in both feet, in 4 in the right, and in 4 in the left foot only; out of 34 *females* he has found it in 16, viz. in 10 in both feet, in 3 in the right, and in 3 in the left foot only,—giving a proportion of about 43 per cent. of all the subjects, and a frequency of 1 in the *male* to $1\frac{1}{4}$ in the *female* sex.

53. *Flexor brevis digitorum pedis.*—In 3 *males* and 3 *females* (in one of the former in both feet, and in 2 in the right foot only, and in 2 of the latter in both feet, and in the other in the right foot only) the outermost tendon of this muscle to the little toe was *absent*, as in the Quadrumana. In some, a substitutory slip of muscle, arising from the corresponding tendon of the *perforans* or long flexor, was discovered, but often no trace of a *perforatus* tendon could be found in the digital sheath. In the right foot of the male (No. 8) a slip of tendon from the *perforans* of the middle digit became blended with the *perforatus* at the metatarso-phalangeal joint. In the same foot a well developed fusiform muscle arising from the tendon

of the *perforans* supplied the perforated tendon of the *little toe.* In the right foot of No. 11 a long slip from the *flexor-longus-digitorum* tendon joined that of the *flexor brevis* going to the middle toe—as before described with the varieties of the former muscle. In the left foot of the same subject a similar tendinous slip to the *flexor brevis* tendon of the middle toe arose from the fibres of the *accessorius.*

Out of 68 *males* the outer tendon of the *flexor brevis digitorum* has been found *absent* in 10, viz. 6 in both feet, 3 in the right, and 1 in the left only. Out of 34 *females* it has been found *absent* in 5, viz. in both feet in 4, and in 1 in the right only. This gives a proportion of 15 per cent. and an exact *equality* in the two sexes. In 4 of these, viz. 3 males and 1 female, a substitutory slip of muscle and tendon arose from the corresponding tendon of the perforans, as in the Quadrumana. In one this was connected partly with the fibres of the accessorius, and partly had a separate origin from the tubercle of the calcaneum.

54. *Abductor hallucis.*—A considerable slip from this muscle to the *second toe,* arising with the front fibres, and inserted into the base of the first phalanx, as first found and described by the author in his paper of last year, was seen this year in three *males* and one *female.* In one of the males and in the female it was found in both feet, in one male in the right foot, and in the third in the left foot only. A similar slip is said by Meckel to be found in the foot of the White Bear.

Out of 40 *males* this slip has been found in 5, in 2 in both feet, in 1 in the right, and in 1 in the left foot only. Out of 30 *females* it has been found in 1 only, in both feet.

55. *Opponens minimi digiti.*—This muscle, first pointed out by Henle as frequently present in the human foot, has been noted this year in 6 subjects out of 36, in 1 male and 5 females. In 4 it was found in both feet, in 1 in the right and in 1 in the left foot only. In 1 female in last year's series it was found, remarkably large, in both feet. The author has found this muscle largely developed in the Orang. In the Bonnet-Monkey it was not present. It is figured in Cuvier and Laurillard's plates as very large in the foot of the Lion and Panther.

56. *Sundries.*—Besides the three-headed gastrocnemius already described, we have in this column, in the male (No. 4), an entirely detached portion of the *iliacus internus,* arising from the middle of the iliac crest by a thin tendinous aponeurosis, forming a flat muscular slip, which was inserted into the common tendon of the *psoas* and *iliacus* muscles. In both feet of the female (No. 20) the *abductor minimi digiti* was provided with two distinct tendons. In the left leg of the female (No. 22) a considerable fleshy slip from the *adductor brevis* joined the tendon of the *obturator externus.* In the right foot of the female (No. 27) the tendinous slip from the insertion of the *tibialis posticus* to the outer cuneiform bone, which usually gives part origin to the *flexor brevis hallucis,* passed wholly into that muscle without being connected with the cuneiform. In

the left leg of No. 29 a considerable muscular slip passed from the origin of the *pectineus* muscle across the deep femoral artery to join the fibres of insertion of the *adductor longus.* This slip was noted in two subjects in last year's series. It is also mentioned by Macwhinnie (*op. cit.* p. 194), and is found in some of the Quadrumana, Carnivora, and Rodents.

In the right leg of the female (No. 33) was found a complete specimen of the *flexor accessorius digitorum longus pedis,* arising by a single penniform belly from the lower third of the outer border of the *fibula* and the posttibial fascia, and ending in a tendon which joined that of the *flexor digitorum longus* at the point where the normal "*massa carnea Sylvii*" was connected with it.

This abnormal muscle has been found in 4 *male* subjects out of 68, and in 1 *female* out of 34. In all it arose from the lower third of the *fibula* and the *fascia* covering the *flexor longus hallucis.* In 3 of the males it was found in both legs. In the fourth male and in the female it was found in the right leg only.

On reviewing the foregoing 18 males and 18 females with reference to the Table, it will be seen, as might be expected, that the greatest absolute number of abnormalities in the vertical columns (reckoning both sides as one) is found in those muscles the variations of which have been hitherto best known to anatomists. These have been noted to ascertain simply their exact frequency.

Of supernumerary tendons of the *extensor minimi digiti* and *extensor ossis metacarpi pollicis* there are 33 instances—1 only of the males and 2 of the females, and these latter in the same individuals presenting the single insertion described in most anatomical manuals. The *coronoid* origin of the *flexor pollicis longus* is seen in 27 subjects, only 5 males and 4 females not possessing it. Slips of junction with the *flexor profundus digitorum* were found in 3 subjects. The *extensor primi internodii hallucis longus* is found in 29 subjects, only 2 males and 5 females not possessing it. The *peroneus quinti* tendon is found in 20 subjects, 10 in each sex. Next comes the *abductor ossis metatarsi quinti* in 19 subjects, 11 of which are males—a much larger proportion in that sex than was found in last year's series. The *extensor carpi radialis brevior* and *primi internodii pollicis* each give 18 abnormalities, 9 of which in the latter muscle were instances either of partial amalgamation or total absence, 4 in males and 5 in females. The *peroneus tertius* presents abnormalities in 15 subjects, 2 of which are instances of total deficiency, and both of them in *females.* This is curious when compared with the greater frequency of absence in the same sex of the *extensor primi internodii pollicis.* Both these muscles are eminently *human* muscles, and are never found even in the highest of the *Simiadæ.* The absence of the two muscles does not, however, seem to be correlated; it is not found in the same individual in any of the cases. The *cleido-occipital, palmaris longus,* and *flexor profundus digitorum* come next in frequency, each in 14 subjects, and nearly

equally in the two sexes. The *extensor carpi radialis longior* (with the *accessorius*) and the *extensor communis digitorum* are found abnormal in 12 subjects, and the *extensor secundi internodii* in 11. These numbers accord, relatively and proportionately, pretty closely with their parallels in last year's series.

The *greatest number* of *abnormalities in each subject* is found in the *males* :—No. 7 having no less than 25 (the greatest number, it is believed, ever found in one subject), of which 17 are in the arms, and 5 only in the legs; and No. 11 having 24, of which 14 are in the arms and 6 in the legs. No. 17 has 20, of which 13 are in the arms and 7 in the legs. In all these the greatest number is found in the *arms*. But, on the other hand, we find that No. 4, having also 20 abnormalities, has 9 in the arms and 10 in the legs.

Of the *females*, No. 33 has 20 abnormalities, of which 15 are in the arms and 5 in the legs; while No. 29 has also 20, of which 11 are in the arms and 8 in the legs. In some, the proportion of the number of abnormalities in the *arms* to those in the *legs* is even greater than the above—as, for example, in the males No. 10 (in which it is 11 to 1), No. 13 (12 to 2), Nos. 2 & 6 (10 to 2), and in the females No. 31 (12 to 2), and No. 23 (10 to 2). In the male No. 1 the abnormalities in the arms and legs are equal, 6 in each; and in the female No. 21 they are also nearly equal, 8 in the arms and 7 in the legs. In only one subject, the male No. 8, is the number of abnormalities in the legs (8) greater than that in the arms (4). The smallest number of abnormalities is in No. 14 (male), viz. 5 in the arms and 2 in the legs; and in No. 26 (female), 1 proper to the head, 2 connected with the arms, and 1 in the legs.

In estimating the total number of abnormalities, both sides of the body, when alike or nearly alike, and complications affecting mutually two or more neighbouring muscles, are, for the sake of convenience, reckoned as *one* instance. 296 are found in the 18 *males*, and 262 in the 18 *females*, making a grand total of 558. Of the 296 in males, 13 are found in the muscles proper to the *head and neck*, 24 in those connected both with the *head and neck* and *arm*, 182 belonging to the *arms* only, and 77 to the *legs* only. Of the 262 in *females*, 7 are found in those proper to the *head and neck*, 16 in those connected with the *head and neck* and *arms*, 168 belonging to the *arms* only, and 71 to the *legs* only.

The proportion of those in the *arms* to those in the *legs* in the two sexes is much more nearly equal in this year's than in last year's subjects (in which the latter predominated), and amounts to about 5 in the arms to 2 in the legs. All the *lines of variation* observed in former years, except the *occipito-scapular*, *supracostalis*, and a few other less important muscles, have been noted in this year's series.

Of the 296 abnormalities found in the 18 males, 173 have been found *on both sides*, 62 on the *right side only*, and 61 on the *left side only*, making 123 *single* or *one-sided* specimens. Of the 262 found in the 18

females, 138 have been found on *both sides*, 55 on the *right side only*, and 69 on the *left side only*, making 124 *single* or *one-sided* specimens, and giving rather *fewer* on the *right* and *more* on the *left* side than in the males. In the *females*, also, it will be observed that the proportion of the abnormalities found *on both sides* to those found on *one side only* in the same sex, is much *less* than in the *males*. This is found to depend upon the smaller number found in both *arms* of the female, viz. 96 in the *females* to 125 in the *males*, while the number found in the *left* arm *only* of the females is 51 compared with 38 in the *males*; and that found in the right arm only of the *females* is 29, compared with 36 in the *males*. On the other hand, the number found in the *left leg* only in the *female* is but 10, in comparison with 13 in that of the *male*, and with 20 in the *right leg* only of the former sex.

The disproportion in this particular comes out still more markedly when the whole number of 102 subjects comprised in the Tables of the three last years, viz. 68 *males*, and half that number, or 34 *females*, is taken into consideration.

The number of abnormalities in the 68 *males* is 414 on *both* sides, 108 on the *right*, and 101 on the *left* side *only*, making 209 *one-sided* specimens. The number of abnormalities in the 34 *females* is 209 on both sides, 68 on the *right*, and 81 on the *left* side *only*, making 149 *one-sided* specimens.

Thus in the *females* we find a proportionate preponderance to the amount of about 45 *one-sided* specimens; and these are mainly composed of those on the *left* side only.

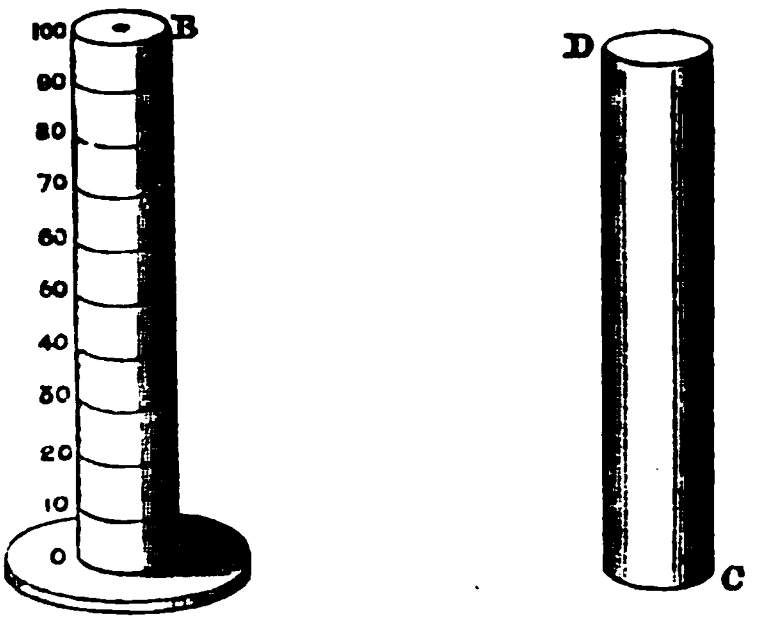
The total number of abnormalities in the 102 subjects is 981, of which 623 are in the 68 males, and 358 in the 34 females. The number found on *both* sides is 623, of which 414 are in the *males* and 209 in the *females*. The number found in the *right* side only is 176, of which 108 are in the *males* and 68 in the *females*. The number found on the *left* side only is 182, of which 101 are in the males and 81 in the females, making a total of 358 *one-sided* specimens.

XVIII. "On an Easy Method of measuring approximately the Intensity of Total Daylight." By Roger J. Wright, Esq. Communicated by Professor Stokes, Sec. R.S. Received May 21, 1868.

An easy method by which the amount of light may be at any time measured and registered appears to be still wanting. I would suggest the following plan, by which I believe the desired object may be attained.

A B is a rod of solid metal, terminated by a heavy base, which keeps the rod in a perpendicular position. C D is a hollow tube, blackened inside, of such a diameter as exactly to fit and slide over A B. The extremity, B, of the rod A B is painted of a snowy white, with a jet-black spot in the centre,

as shown in the figure. On A B is marked the scale, beginning with zero at A. The tube is pushed over the rod till the extremity C coincides with the zero line at A.



The method of using this instrument is as follows:—Draw the tube gently up the rod, at the same time looking steadily at the black spot before mentioned. It will be found, as the tube ascends, that the black spot will gradually disappear, and ultimately vanish in the gloom; it will also be found that on *different* days, and *different hours* of the *same day*, the *point at which the black spot vanishes* will *vary with the intensity* of the light. This point is read off on the graduated scale, and thus we are enabled to measure the intensity of the light at any required time. In taking an observation, it would be well to state whether that portion of sky round the zenith from which the cone of rays proceeds be clear or cloudy.

It will be seen that the result obtained by this method is not *scientifically* correct, as it will be affected by the eyesight of the person who makes the observation, but only in a slight degree. The method of measuring light, as just described, has been known to me for upwards of three years. The hope that I should some day be enabled to make the instrument *scientifically* correct has hitherto prevented me from making it public. As I understand that it is highly desirable to have some means of estimating the changes in the light which will occur during the total eclipse of the sun in August next, I no longer feel justified in keeping in the background an instrument which may *possibly* be of some slight assistance.

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OBITUARY NOTICES OF FELLOWS DECEASED

BETWEEN 30TH NOV. 1865 AND 30TH NOV. 1867.

Dr. BENJAMIN GUY BABINGTON was born in 1794. He was the son of Dr. William Babington, who, in his time, held a foremost place as a popular and successful London physician. Educated at the Charter House, he subsequently went through the usual course of study at Haileybury College then required of young men destined for the Indian Civil Service; he went out to the Madras Presidency as a member of that service in 1812. After remaining seven years in India, he was compelled by ill health to return home, and then determined to leave the Indian Service and adopt his father's profession. With this view he entered at Pembroke College, Cambridge, and took the degree of M.B. in 1825, and that of M.D. in 1830, In the meantime he commenced practice in London, and in 1831 was elected a Fellow of the College of Physicians. For the prosecution of his medical studies in London he had chosen Guy's Hospital, where his father was physician, and he was himself appointed assistant physician to that Institution in 1837, and promoted to be one of the physicians in 1840.

Dr. Babington was much esteemed as a clinical teacher, and was the author of papers on different professional subjects, published in the Guy's Hospital Reports, and elsewhere; but he also engaged in researches of more general scientific interest, and among them his observations on the blood, published in the 'Medico-Chirurgical Transactions' of 1830, deserve especial mention, inasmuch as he there showed that the liquid part of the circulating blood, or "liquor sanguinis" (a name proposed by him to distinguish it from the serum, and very generally adopted since), really contains or yields the coagulable matter, or fibrin, which solidifies in the process of coagulation. This, no doubt, was merely a confirmation by simple but well-devised experiments of the doctrine held by Hewson and his contemporaries, and accepted by most British physiologists; but the confirmation was needful and well timed on account of the erroneous views then prevailing on the continent on the authority of Prevost and Dumas. At a later time, namely in 1859, Dr. Babington communicated to the Royal Society a series of observations on the effect of various salts dissolved in water in retarding or otherwise altering the rate of spontaneous evaporation, and an abstract stating the nature and results of the experiments was published in the 'Proceedings' for 1859.

In the establishment of the Sydenham Society, since succeeded by the new Society of the same name, Dr. Babington took an active share. Up to the time of its dissolution he acted as Treasurer, and contributed to its publications an elegant translation of 'Hecker's Epidemics of the Middle Ages,' besides aiding in the revision of other works published by the Society. The Epidemiological Society, founded in 1850, owes its origin mainly to his exertions. He was its first President, and continued in that

office, taking part with unabated interest in its useful labours, till within a short time of his death.

It remains to be added that Dr. Babington was a man of varied accomplishments. He had a refined taste in art, and applied his hand in some of its departments with no mean success. He had great readiness and skill in devising and constructing apparatus and instruments of various kinds, and mechanical appliances for the ease and comfort of the sick. It is especially worthy of note that in 1829 he communicated to the Hunterian Society of London a method of inspecting the fauces and glottis in the living person by means of a small mirror passed back into the throat; so that, for aught that appears to the contrary, Dr. Babington is entitled to the credit of having made the first practical step in the art of laryngoscopy.

Dr. Babington was elected a Fellow of the Royal Society in 1828; in 1861–63 he served on the Council. His death took place on the 8th of April, 1866.

William Thomas Brande, D.C.L., F.R.S., was born on the 11th of January, 1788, in Arlington Street, St. James's, London, and died at Tunbridge Wells on the 11th of February, 1866, in the seventy-ninth year of his age.

When about six years of age he was placed in a private school at Kensington, where he remained four years. At this school he made tolerable progress in the rudiments of the Latin language, and learned to read Greek. On leaving it he was sent to Westminster, where, during a period of eight years, he made fair progress in classical and general knowledge. His father, who was an apothecary, suggested that he should select the Church for his future career; but he preferred the medical profession, and on the 2nd of February, 1802, according to the custom of those days, he was bound apprentice to his brother, who was a Member of the Society of Apothecaries. The family removed from Arlington Street to Chiswick, and here it was that Mr. Brande first became acquainted with Mr. Charles Hatchett, whose daughter he subsequently married. Mr. Hatchett at this time was much occupied in chemical investigations, and it appears to have been from his conversation and example that the subject of this memoir first acquired a strong attachment to Chemistry and Mineralogy. He often assisted Mr. Hatchett in his laboratory, and he received from him specimens of minerals which formed the foundation of a collection which he kept up through life. He was then simply a schoolboy fond of science, and this feeling was encouraged and developed by the kindness and attention of Mr. Hatchett. He had then no idea that chemistry would be his future profession.

In the year 1802, when in his fourteenth year, he paid a visit to his uncle at Hanover, and here he acquired a good knowledge of the French and German languages. In the spring of 1803 he visited Brunswick and Göttingen; but all his plans of study were interfered with by the breaking

out of the war and the approach of the French to Hanover. With some difficulty he escaped to Hamburg, and reached London after passing a month at sea in a Dutch merchant-vessel.

In 1804 he resumed his duties as apprentice to his brother, and in the autumn of that year, and in the seventeenth year of his age, he became a pupil at the Anatomical and Medical School in Windmill Street. He attended the lectures of Mr. Wilson and Mr. Thomas, and it was during this period of study that he made the acquaintance of Brodie (the late Sir Benjamin), Maynard, Ewbank, and other professional men of repute. He now studied chemistry under Dr. George Pearson, and became one of his pupils at St. George's Hospital. He here made the acquaintance of Dr. C. R. Pemberton, and by the aid of this gentleman he acquired a considerable knowledge of disease and a great liking for the practice of physic. About this time also he found a friend in Sir Everard Home, who was then in the zenith of his reputation.

A weekly meeting was held on Saturday evening at the Windmill-street School: this was founded on the ruins of the once celebrated Lyceum Medicum, and it may be regarded as the parent of the Westminster Medical Society. The meetings were attended by many medical men of repute; medical and scientific papers were read and discussed; and as chemistry was very often the subject of inquiry, Mr. Brande found that he was frequently referred to as an authority on matters connected with this science. Writing of this date, Mr. Brande states:—

"I was now full of ardour in its prosecution (*i. e.* of chemistry); and although my brother, with whom I still lived, whose apprentice I was, and in whose shop, notwithstanding all other associations, I still worked and passed a large part of my time, threw every obstacle in the way of my chemical progress that was decently in his power, I found time, however, to read, and often to experiment in my bedroom late in the evening. I thus collected a series of notes and observations which I fondly hoped might at some future period serve as the basis of a course of lectures, and this in time they actually did. It was at this period that, in imitation of Mr. Hatchett's researches, I made some experiments on Benzoin, the results of which were published in Nicholson's Journal for February 1805."

This, it may be remarked, was his first chemical contribution to scientific literature. He was then only about sixteen years of age. Mr. Brande also contributed to the same journal for June 1805 a paper on Respiration, which had been read to the Westminster Medical Society.

His first introduction to Sir Humphry Davy was about the year 1801, when he was a boy at Westminster School, then in his thirteenth year. He was introduced to Davy at the Royal Institution, which had been recently founded. After his return from Germany in 1804, he renewed his acquaintance with Davy, and, as a result, his zeal in the pursuit of chemistry was greatly augmented. Sometimes he stole away from home, or, instead of going to the Anatomical School in Windmill Street, contrived

to get admitted to one of Davy's lectures at the Royal Institution. It appears that at this time he formed a resolution of losing no opportunity that should present itself to enable him to take the office of a lecturer on chemistry.

In the autumn of 1805 he drew up an account of some experiments on Guaiacum resin. This formed the subject of a paper presented by Mr. Hatchett, and read before the Royal Society on the 19th of December. It was printed in the 'Philosophical Transactions' for 1806. He now became acquainted with Sir Joseph Banks. He was frequently consulted by Sir Everard Home on chemical and physiological questions; and by the advice of Sir Everard he was entrusted with the analysis of the calculi then in the collection of the College of Surgeons. The results were communicated in a letter to Sir Everard, which was read before the Royal Society on the 19th of May, 1808, and published in the Transactions with some observations by Sir Everard Home. Two other papers relating to the state and quantity of alcohol in fermented liquids were published in the Transactions for 1811 and 1813. For these he was presented with the Copley Medal in 1813.

The winter of 1808 was an important epoch in Mr. Brande's life. He being then in his 21st year, commenced lecturing, and gave two courses of lectures on pharmaceutical chemistry at Dr. Hooper's Medical Theatre in Cork Street, Burlington Gardens. These lectures were so well received that Mr. Brande was invited in the ensuing autumn to join the new Medico-Chemical School established by Mr. Wilson in Windmill Street. Dr. Cooke and Mr. Brande lectured there on alternate mornings on physic and chemistry; but the weight of the entire course fell chiefly on Mr. Brande. Dr. Cooke withdrew from the lectures, and in 1813 Mr. Brande joined Dr. George Pearson, who gave medical lectures at his house in George Street. Mr. Brande gave a course of lectures on Materia Medica at Dr. Pearson's, and at the same time continued the course of Chemistry at the small laboratory which he had fitted up in Windmill Street.

On the 13th of April, 1809, being then only 21 years of age, Mr. Brande was elected a Fellow of the Royal Society, and took his seat on the 20th of that month. This at once brought him in contact with all the leading scientific men of the day, including Cavendish, the elder Herschel, Maskelyne, and Wollaston. To all these he soon became intimately known.

On the 4th of November, 1812, Mr. Brande accepted the appointment of Professor of Chemistry and Superintending Chemical Operator to the Apothecaries' Company. This gave him the opportunity of remodelling many of their pharmaceutical processes, and of raising them to the level of the improved state of chemistry. There can be no doubt that Mr. Brande aided in an important degree in resuscitating the character of the Society and giving to it a scientific status. The late Mr. Hennell was his pupil. Mr. Brande was made Professor of Materia Medica, and he delivered annually a course of lectures on that subject.

In the spring of 1812, Mr. Davy (afterwards Sir Humphry Davy), with whom Mr. Brande was on cordial and intimate terms, delivered his last course of lectures in the Royal Institution. On the occasion of his marriage at this date he resigned the chair of Professor of Chemistry in the Royal Institution, and invited Mr. Brande to deliver for him a course of lectures which he had been in the habit of giving annually before the Board of Agriculture. These lectures were delivered gratuitously by Mr. Brande; but in the following year this Board was dissolved.

In the winter of 1831, Mr. Brande gave a probationary course of Chemistry at the Royal Institution, and in May of the same year he was unanimously elected Professor of Chemistry. In the autumn of this year he took possession of the apartments that had been occupied by Sir Humphry Davy, and was then completely installed as his successor. Sir Humphry went abroad and did not return for two years.

In October 1815, Mr. Brande transferred the pupils from his School of Chemistry in Windmill Street to the laboratory of the Royal Institution, and he commenced a regular and extended course of lectures on this science. In consequence of the death of Dr. Pearson, the chemical lectures were also transferred from St. George's Hospital to the Royal Institution. His lectures were well attended and contributed to raise the reputation of the Institution as a seat of practical instruction. He was subsequently assisted in these lectures by Mr. Faraday. Mr. Brande devoted himself at this time entirely to chemical pursuits and to lectures on the science.

In 1823 he was consulted by the Government and drew up a report on the manufacture of iron and steel. The manufacture of the metal for coinage formed a part of the inquiry; and the office being vacant in 1825, Mr. Brande was appointed Superintendent of the Die Department at the Royal Mint. In 1854, he resigned his professorship in the Royal Institution, and was made Superintendent of the Coining department at the Mint.

It was in the year 1825 that Mr. Faraday became associated with Mr. Brande in the lectures delivered at the Royal Institution. He also had the assistance of Mr. Faraday in the publication of the 'Quarterly Journal of Science and the Arts,' edited at the Royal Institution. This publication was commenced in 1816, and was carried on until 1836, when it fell into other hands and was soon discontinued. In 1836 Mr. Brande was named one of the original Fellows of the University of London, and a Member of the Senate of that Body; and in 1846 he became Examiner in Chemistry, an office which he retained until 1858. He received the honorary degree of Doctor of Civil Law in the University of Oxford. He was a Fellow of the Royal Societies of London and Edinburgh, and of many other British and Foreign Societies. From 1816 to 1826 Mr. Brande was one of the Secretaries of the Royal Society.

Mr. Brande was the author of a 'Manual of Chemistry,' which went through six editions, of a 'Dictionary of Pharmacy and Materia Medica,'

and of a smaller volume on Chemistry, especially designed for students, published in 1861. In 1842 he undertook the editorship of the 'Dictionary of Science and Art,' a most laborious undertaking; and at the time of his death he was occupied in revising a new edition of this work. His scientific contributions to periodical literature were also very numerous.

From 1808 to 1854, the long period of forty-six years, the subject of this memoir had been before the public as a lecturer on chemistry. He was indefatigable as a teacher of this science. In addition to the lectures regularly delivered at the Royal Institution and the Society of Apothecaries, Mr. Brande lectured for some time on chemistry to the classes at St. Bartholomew's Medical School. Many of the past and present generation, whether in or out of the medical profession, owe much of their knowledge of this science to his teaching. The substance of his lectures is incorporated in the great work by which he acquired a European reputation, namely his 'Manual of Chemistry.' This work was, in its day, one of the most popular in the English language, and there are few recent treatises on the subject which are not indebted to its pages for much valuable information. He found chemistry an ill-arranged collection of facts, and succeeded in reducing them into form and order, thus aiding greatly in the cultivation of the science and in placing it on an intelligible basis.

No scientific man who was brought in contact with Mr. Brande could fail to be struck with the accuracy and extent of his knowledge, the retentiveness of his memory, and the truthfulness and honesty of purpose by which he was always actuated. The friend of Gay-Lussac and Thénard and the associate of Davy and Faraday, he formed a connecting link between the chemists of the past and the present generation. He lived to see the most remarkable changes in the science which he had himself so successfully cultivated, but, like his great contemporaries, he preferred demonstration to speculation; and although ready to adopt what was established by experiment, however it might conflict with his previous views, he was strongly opposed to innovations based upon mere hypotheses. In private life Mr. Brande was well known as a man of genial character. His conversational powers were great, and no man could pass an hour in his society without retaining a pleasant reminiscence of him as a companion.

A. S. T.

William Brinton, M.D., was the second son of the late Mr. Henry Brinton, one of the principal carpet manufacturers of Kidderminster, where the subject of the present notice was born, on the 20th of November, 1823. After attaining remarkable proficiency at school, William Brinton was apprenticed to a surgeon in his native town, and in October 1843 entered the medical department of King's College, having in the preceding summer matriculated, with honours, at the University of London. He passed through his student career with great distinction, and was noted among his fellows not only for the general ability and indomitable perseverance with which he applied himself to intellectual work, but especially for his unde-

viating aim at precision and thoroughness of knowledge in all his studies. The character he bore as a student was honourably maintained at the University of London, where he took the degree of M.B. in 1847, and of M.D. in the following year. In the meantime he was appointed Assistant Demonstrator of Anatomy, and in 1850 Medical Tutor, at King's College, which appointment he held until 1853, when he was elected Lecturer on Forensic Medicine at St. Thomas's Hospital.

To St. Thomas's medical school Dr. Brinton became permanently attached by his being elected Physician to the Hospital in 1860, and appointed Lecturer on Physiology, having previously been associated with the late Mr. Grainger in that lectureship. He soon proved himself to be an able and accomplished teacher of physiology; and those who have heard him lecture speak with admiration of his appropriate language and agreeable delivery, and of the ready power and happy effect with which he used his pencil to illustrate his oral instructions.

From 1852 until his appointment to St. Thomas's in 1860, Dr. Brinton was Physician to the Royal Free Hospital, where he enjoyed large opportunities of medical observation; and during his tenure of that office he published in 'The Lancet' a series of "Clinical Remarks," which were much valued. He speedily rose in the estimation of his professional brethren and gained the confidence of the public, so that for some years before his death he enjoyed a much larger share of consulting practice than usually falls to the lot of young physicians of his standing.

When quite a young man Dr. Brinton contributed several articles to Dr. Todd's 'Cyclopædia of Anatomy and Physiology,' and translated Valentin's 'Text-book of Physiology' from the German. In 1857 he published a work on 'Ulcer of the Stomach,' and, two years later, one of more extended scope, on 'Diseases of the Stomach;' this was followed by a treatise on 'Food and Digestion.' These works were the result of a careful study of a subject to which he had given special attention. He entertained original views on the natural and perverted movements of the alimentary canal, and on the nature and cause of intestinal obstruction, and gave an exposition of his doctrine in the Croonian Lectures which he delivered before the College of Physicians.

Three papers by Dr. Brinton were read to the Royal Society, of which abstracts were published in the 'Proceedings;' viz. "Contributions to the Physiology of the Alimentary Canal" (1848), "On the Dentate Body of the Cerebellum" (1852), and "Experiments and Observations on the Structure and Function of the Stomach in the Vertebrate Class" (1861). He was elected a Fellow of the Society in June 1864.

In a memoir of Dr. Brinton which appeared in 'The Lancet' soon after his death, from which chiefly we have derived the substance of this brief notice, it is mentioned that among his numerous tastes and acquirements was a love of mountaineering, and that he was a member of the Alpine Club, and contributed two papers to the second series of "Peaks, Passes,

and Glaciers." Dr. Brinton died on the 17th of January, 1867, at the early age of 43. The cause of his death was renal disease of three years duration, against which he had all along borne up with marvellous courage.

Mr. Walter Crum, an eminent scientific chemist and manufacturer of Glasgow, was born there in 1796, and died at his residence, the Rouken, near that city on the 5th of May, 1867. He was the second son of Mr. Alexander Crum, of Thornliebank, senior partner in the firm of Alexander and James Crum, long established and much respected as merchant manufacturers in Glasgow. His mother, to whose family his personal characteristics appear to have had the strongest resemblance, was a daughter of the late Walter Ewing Maclae of Cathkin. Having received a liberal education at the Grammar-school and University of Glasgow, he entered upon the business of calico-printing, a department of the work of his father's establishment. With a view to the conduct and improvement of this branch of manufacture he early devoted himself to practical chemistry, and in 1818 and 1819 was an ardent student of that science in the laboratory of Dr. Thomas Thomson—at that time one of the few laboratories, if not the only one in this country, in which analytical research was systematically practised and taught. The intimate knowledge of chemistry which Mr. Crum thus acquired, combined with his general scientific attainments, enabled him to introduce many useful improvements into his own business, and thus to maintain and increase the excellence of its manufactures. As an important example of these may be mentioned the well-known process of "orange resist" on indigo, which was invented and practised for five years by Mr. Crum before it became known to other manufacturers.

Mr. Crum's ability as a chemical analyst, however, gave to his researches a scientific interest and a value beyond that which belonged to their mere practical application; and his first paper on the Analysis of Indigo, published in 1823 in the 'Annals of Philosophy,' established his reputation as a scientific chemist, and brought him into correspondence with, and procured for him the friendship of, many of the first chemists of his time, among whom may be mentioned, as the most intimate, Thomson, Faraday, Graham, and Liebig. He became a member of the Royal Society in February 1844. His other scientific papers followed in succession from 1830 to 1861; almost all of them were communicated to and printed in the Proceedings of the Philosophical Society of Glasgow, which he joined in 1834, and of which he became President on the death of Dr. Thomas Thomson in 1852. A list of these papers is subjoined; several of them were translated into German, and published in Liebig's 'Annalen der Chemie,' as in vol. lv. (1845), vol. lxii. (1847), and vol. lxxxix. (1854).

The most important of Mr. Crum's researches are those relating to indigo, gun-cotton, the acetates of alumina, and the dyeing of cotton-fibre. It was from Mr. Crum's analysis of sublimed indigo that he was led to

construct the formula for that substance, which is now universally adopted. This analysis was made at a time when accurate organic investigation involved much greater difficulty and the exercise of higher ingenuity than, with Liebig's method, is now required; and although Erdman and Dumas had from their analysis of a less pure substance deduced other formulæ, they afterwards confirmed that of Mr. Crum. The very ingenious method proposed by Mr. Crum for the analysis of nitrates was founded on the conversion of the nitric acid into nitric oxide by the action of sulphuric acid on mercury. He applied this method to determine the quantity of nitrogen in gun-cotton. Having determined the carbon by combustion with chromate of lead, he was led to the formula of $C_{12} H_7 O_7 + 3 NO_5$ for the most explosive gun-cotton—a result which has recently been confirmed, after the most rigorous investigations, by Mr. Abel and others, and it is now universally accepted. The action of sulphuric acid and mercury on gun-cotton led Mr. Crum to regard the latter as a nitrate, a view which, making gun-cotton analogous to nitric ether, was long opposed by nearly all chemists, but is now very generally adopted.

Mr. Crum's investigation of the acetates of alumina is very able and complete, showing the precipitating action of very dilute solutions of various salts on soluble alumina, in the same manner as he had shown such action at a much earlier date in the case of the sulpho-indigotates and sulpho-phœnicates. His papers on the peroxide of copper and on the action of nitric acid and peroxide of lead as a very delicate test for manganese are esteemed valuable contributions to analytical chemistry.

After the death of Dr. Thomson in 1852, Mr. Crum delivered to the Philosophical Society of Glasgow a discourse on the Life and Labours of his instructor, in which he dwelt with much feeling, ability, and discrimination on the varied acquirements and contributions to science of that eminent and laborious chemist.

Mr. Crum was remarkable for unbending rectitude of purpose and love of truth, for great acuteness of perception and decision of character; and his public spirit was as notable as his perseverance in conducting to a successful termination whatever he undertook. He occupied during many years a high place in public estimation, and took a leading part in every good work having for its object the educational, social, or political improvement of the people.

In his own extensive and interesting works at Thornliebank, he had an ample field for the energetic practical development of his sagacious and benevolent plans for the moral and intellectual improvement of the numerous workmen in his employment. For upwards of forty years Mr. Crum devoted much time and personal effort to furthering the interests of Anderson's University in Glasgow, of which he was a most liberal and effective patron and supporter. His election to the Presidency of that valuable institution gave him ampler opportunity for indulging his favourite desire of bringing scientific knowledge within reach of the artisan and

the general public. In many other ways he contributed to promote the cause of popular education.

Mr. Crum's character was marked by clear intelligence and strong practical sagacity, unbending love of truth and justice, frank sincerity, great kindness of heart, generosity, and active and judicious benevolence, but so unselfish and of such retiring modesty that he shrunk from the slightest display. Few men have lived more usefully, or have died more esteemed and regretted.

List of Papers by Mr. Crum.

1. "Experiments and Observations on Indigo, and on certain Substances which are produced from it by means of Sulphuric Acid," in Annals of Philosophy for 1823.

2. "On the Primitive Colours," 1830.

3. "On Chlorimetry, and a New Method of testing weak solutions of Bleaching-Powder," in Proceedings of the Philosophical Society of Glasgow for 1841, vol. i. p. 17.

4. "On the Manner in which Cotton unites with Colouring-Matter," 1843, *ibid.* p. 98.

5. "On the supposed influence of the Moon upon the Weather," 1844, *ibid.* p. 243.

6. "On the Action of Bleaching-Powder on the Salts of Copper and Lead," 1845, *ibid.* vol. ii. p. 68.

7. "On the Artificial Production of the Potato-disease," 1845, *ibid.* p. 90.

8. "On a Method for the Analysis of Bodies containing Nitric Acid and its application to Explosive Cotton," 1847, *ibid.* p. 163.

9. "On a peculiar fibre of Cotton which is incapable of being dyed," 1849, *ibid.* vol. iii. p. 61.

10. "Sketch of the Life and Labours of Dr. Thomas Thomson, F.R.S.," 1852, *ibid.* p. 250.

11. "On the Acetates and other Compounds of Alumina," *ibid.* p. 298.

12. "On the Stalactitic Sulphate of Barytes found in Derbyshire," 1861, *ibid.* vol. v. p. 39.

13. In 1862 Mr. Crum greatly extended his previous investigations, chemical and microscopical, on the manner in which cotton unites with colouring-matter, and on a peculiar fibre of cotton which is incapable of being dyed, "coton mort," both of which subjects he had studied very profoundly. The results of these important and difficult investigations, illustrated by numerous engravings, were published in the Journal of the Chemical Society for 1863.

Joseph Edye was born at Plymouth Dock (Devonport) in 1791. He entered the Navy in 1805, and served as secretary's clerk under Sir John Duckworth, on board the *Superb* in the action off St. Domingo (for which he in afterlife received a medal), and also on board the *Royal George*, 100

guns, at the passage of the Dardanelles. He attained the rank of Paymaster in 1811, and after service in the *Erne* was employed as Secretary in 1823 and 1824 to the Hon. Alexander Cochrane at Plymouth, from 1824 to 1827 to Lord de Saumarez at the same port, and from 1828 to 1834 to Sir Pulteney Malcolm in the Mediterranean during the pacification of Greece and the expulsion of the Egyptians from the Morea, where he acted as British Commissioner at the headquarters of Ibraham Pasha, in concert with the Commissioners of France and Russia, for the purpose of rescuing from slavery about 2000 Greek women and children found in the Egyptian camp, on which service he received an injury that subjected him to severe suffering for the rest of his life. He was also with the same gallant commander in the North Sea during the demonstration made on the Dutch coast in 1832 and 1833 at the siege of Antwerp. From 1836 to 1839 he was with Sir Philip Durham, as secretary at Portsmouth, and from 1839 to 1842 with Sir Graham Moore at Plymouth. He was placed on the half-pay of a secretary in 1843, and received a letter of thanks from the Admiralty for his important special services as Secretary to the Commission consisting of Sir P. Malcolm, Sir C. Adams, and Sir T. M. Hardy, which framed the laborious production, 'The Code of Naval Regulations;' and in 1865 he was appointed Paymaster-in-Chief, and received the Greenwich pension for his past service.

Mr. Edye was elected a Fellow of the Royal Society in January 1841. He died at his residence, Stoke, Devonport, November 12, 1866, leaving a widow and one son, a midshipman in the Navy.

Sir George Everest was born at Gwernvale (Brecon), the seat of his father, Mr. Tristram Everest, on the 4th of July 1790. In his early studies at Woolwich he made such progress that he was found qualified for a commission before the age prescribed by the regulations, and was noticed by the Professor of Mathematics, Dr. Hutton, as one who would distinguish himself. In 1806 the young cadet sailed for India, where he entered the Bengal Engineers, and after seven years' service, was sent to join a detachment in Java. There Lieut. Everest was selected by Sir Stamford Raffles, then Governor, to make a survey of the Island, in which laborious task he spent two years, and afterwards returned to Bengal. He was next employed in engineering works (improving the navigation of the outlets of the Ganges), and though appointed Chief Assistant on the Great Trigonometrical Survey of India in 1817, he remained for some months in Hindoostan to complete the establishment of a line of telegraphic posts from Calcutta to Benares.

In 1818 Captain Everest joined the party under Lieut.-Col. Lambton, Superintendent of the Survey at the headquarters, Hydrabad. Well qualified for this appointment by education and habit, he entered with great spirit on the duties by which his name has become noteworthy in the annals of Geodesy. Before that survey was undertaken the topography of

India was as incorrect as it was scanty; there was an error of forty miles in the breadth of the peninsula as laid down in the maps, and it was in consequence of a resolution of the East-India Company to amend this unsatisfactory state of things, by making their Atlas depend on trigonometrical operations, that the survey was commenced.

Captain Everest was carrying the work through an unhealthy part of the Nizam's territory when, in 1820, his health failed, and he was ordered to the Cape of Good Hope to recruit. Here he employed his leisure in an examination of the tract of country in which La Caille measured an arc of the meridian in 1752; and in a letter to Colonel Lambton he reviewed the circumstances under which the measurement had been made, and pointed out the discrepancies between the results and those obtained in similar operations in the northern hemisphere. This letter, printed in the first volume of 'Memoirs' published by the Astronomical Society, led eventually to the remeasurement and extension of La Caille's arc by Sir Thomas Maclear.

On the death of Colonel Lambton in 1823, Captain Everest was appointed Superintendent of the Survey, and devoted himself earnestly to the work. In the same year he took up the survey where his predecessor had left it in the valley of Berar, and extended it into the mountainous tract on the north. In November 1824 he measured a base-line in the Seronj valley, and in 1825 had carried the observations on to Bhaorasa, when his health again gave way, and he was compelled to seek the restorative climate of his native land.

Profiting by his sojourn in England, Captain Everest made himself acquainted with new scientific results bearing on his special pursuit. He was elected a Fellow of the Royal Society in 1827. Part of his time was spent in drawing up an account of the progress of the survey* subsequent to the operations recorded by Lieut.-Col. Lambton in the 'Asiatic Researches," which, with tables, maps, and plans, was published at the cost of the East-India Company, by whose authority it had been prepared. In this work, among the many scientific details, Captain Everest gives a few particulars of his personal adventures in the carrying on of the work—of the severe measures by which he disciplined his native followers and quelled a mutiny among them—of separation from his instruments and provisions by sudden floods—of the explorations through wild jungles in search of favourable observing stations—of journeyings through vast and magnificent forests where, more to be dreaded than tiger or hyæna, lurked the deadly typhus which prostrated him and his whole following. For months he was so weak that he had to be supported by two men while taking his observations at the great theodolite, and could not reach out his hand to the screw of the vertical circle without assistance; yet, though

* An account of the measurement of an arc of the meridian between the parallels of 18° 3′ and 24° 7′, being a continuation of the grand meridional arc of India, as detailed by the late Lieut.-Col. Lambton. 4to, London, 1830.

advised to resort to the coast, he persevered with his task, having a conviction that his absence would be fatal to its prosecution.

Colonel Everest returned to India in 1830, enabled to effect improvements in the survey, for he had made himself acquainted with the practice of the English Ordnance Survey, and with the best methods employed in Geodesy in other parts of Europe; besides which the Directors of the East-India Company had furnished him with the best instruments that could then be constructed. His labours and responsibilities were now largely increased; for in addition to his post as Chief of the Trigonometrical Survey, he had been appointed Surveyor-General of India. In 1832 he resumed operations on the great arc, from which date it was diligently carried on until its completion in December 1841 by the remeasurement of the Beder base-line by Captain Waugh*.

In this renewal of observations, one of Colonel Everest's principal difficulties was the training of assistants. In some other respects the fatigue and risk were mitigated; by the use of proper signal-lights, the surveying could be carried on at night and during the hazy period of the hot winds, whereby the parties were less called on for exposure during the rainy season. The chief himself was so indefatigable, that his contemporaries, playing on his name, were accustomed to speak of him as *Neverrest*. With his improved instruments, he was resolved to improve the Survey and tolerate no inferiority in the execution. On this point he writes, "If I have had any reason to suspect any defect in the instrument, or any instability in the platform, or any want of precision in the signal observed, or even if I have found in drawing up the angles that they presented any discrepancies for which I could not account, I have always felt not only that I was at liberty, but that it was incumbent on me to reject the whole set bodily, and replace it by an entirely new set of angles taken under circumstances free from objection."

With these concluding operations an arc of meridian more than twenty-one degrees in length had been measured by the two persevering chiefs of the Survey and their assistants, extending from Cape Comorin to the northern border of the British Possessions in India. Colonel Everest had speculated on carrying it further, beyond the Himalayas and across the wild regions to the north, until it struck the Russian triangulation within the dominions of the Czar. An arc stretching from the Indian Ocean to the Polar Sea would, indeed, as he himself describes it, have been "a vast project."

In 1843 he quitted India, and residing thenceforward in England, he brought out in 1847 his great book in two volumes quarto, 'An Account of the Measurement of two Sections of the Meridional Arc of India, bounded by the parallels of 18° 3′ 15″; 24° 7′ 11″; and 29° 30′ 48″.' In this work, published also at the cost of the East-India Company, such particulars are set forth as will enable a scientific observer to test the manner of working and the results obtained, and full explanations are given of

* Now Major-General Sir A. Scott Waugh, F.R.S.

the ingenious methods devised by the author for the elimination of error, together with tables, plans, and engravings of the instruments employed. For this work, and the long series of operations on which it was founded, the Royal Astronomical Society awarded their testimonial (equivalent to a medal) to Colonel Everest. "The Great Meridional Arc of India," said Sir John Herschel in presenting the testimonial, "is a trophy of which any nation, or any government of the world have reason to be proud, and will be one of the most enduring monuments of their power and enlightened regard for the progress of human knowledge."

The Asiatic Society of Bengal, on completion of the Survey, elected Lieut.-Col. Everest one of their Honorary Members, with an appreciative eulogium of his scientific services. He was a Fellow of the Astronomical and of the Royal Asiatic and the Geographical Societies. Of the latter he was a member of Council, and filled the office of Vice-President. He was knighted and made C.B. in 1861. His scientific writings are comprised in the two works above mentioned, in papers on subjects connected with surveying published in 'Asiatic Researches' and in the 'Memoirs' of the Astronomical Society, and in a letter on certain computation errors discovered in the logarithm tables of the Great Survey, printed in the 'Proceedings' of the Royal Society.

In 1863–65, Sir George Everest served on the Council of the Royal Society. He died in London, December 1, 1866. His name having been given to one of the highest summits of the Himalayan range, will long be remembered in India.

John Goodsir, Professor of Anatomy in the University of Edinburgh from 1846 to 1867, was born in the year 1814, at Anstruther in Fife, in which county his father and grandfather were well known and much respected medical practitioners.

When little more than a boy he was sent as a student of arts to the University of St. Andrews, where he passed through the curriculum, but, as was the custom at that time, without taking his degree. At this early period of his life he was fond of the study of metaphysics, and imbibed the doctrines of Coleridge, which indeed gave a colour to the whole of his subsequent thoughts and speculations.

Being destined to follow the medical profession, he was apprenticed to Mr. Robert Nasmyth, the eminent Dentist in Edinburgh, and during his apprenticeship pursued his medical studies in the University and Royal Infirmary in that city. His anatomical teacher was Dr. Knox, and in his practical rooms he made the acquaintance of Edward Forbes, which soon ripened into friendship. For Forbes and Goodsir had tastes in common; they both took an active interest in watching the habits and tracing out the structure of animals, and their conjoint researches added several new members to the British fauna.

When he had obtained the Licence of the Edinburgh College of Sur-

geons, Mr. Goodsir returned to Anstruther, where, whilst assisting his father in practice, he found time to pursue his anatomical and zoological studies, and in 1838 brought before the British Association his observations on the Development of the Teeth.

Shortly after this he returned to Edinburgh to fill the office of Conservator of the Museum of the College of Surgeons, for the duties of which he was well adapted, not only from his anatomical knowledge, but from his skill in preparing and displaying anatomical and pathological specimens. He now became a Member of the Wernerian, Botanical, Medico-Chirurgical, and Royal Societies of Edinburgh, and communicated to their Proceedings and Transactions numerous memoirs, which rapidly brought him into notice as an industrious and keen observer of animal form and structure in both healthy and morbid conditions. His well-known papers on *Sarcina*, on the anatomy of *Amphioxus lanceolatus*, on secreting structures, and on the structure of the placenta may be mentioned as examples of his work at this period.

In 1842–43 he delivered lectures at the College of Surgeons, in which he ennunciated his views on various important physiological and pathological processes, which were subsequently incorporated, along with some observations by his brother Harry, in an octavo volume published in 1845. In these lectures he contended that the nucleus of the cell was a persistent element of the textures, that it existed even within the bone-corpuscles, that it played a most important part in the nutrition of the textures, and that great multiplication of the nuclei occurred in disease of cartilage, bone, and other tissues.

In 1844 he was appointed Demonstrator of Anatomy in the University to Dr. Monro *tertius*, and on the resignation of that gentleman in 1846 he succeeded him in the Chair of Anatomy, an office which he continued to hold until his death. In the same year he communicated to the Philosophical Transactions a Memoir on the Development of the Suprarenal, Thymus, and Thyroid Glands, and was elected a Fellow of the Royal Society.

Mr. Goodsir enjoyed for many years remarkable success as a teacher. He gathered together a large nnmber of students, and for several years the attendance on his class numbered between 300 and 400. In his Lectures on Human and Comparative Anatomy he did not satisfy himself with giving a mere descriptive account of the various structures he was called on to expound, but he pointed out the relations of his science to physiology, pathology, histology, morphology, and development. It was to this mode of illustrating the dry details of anatomy, more perhaps than to any special faculty for exposition, that his success as a teacher was due. He worked most assiduously at Comparative Anatomy, and by his labours the collection in the Anatomical Museum of the University has been very largely increased, partly by the specimens added during his lifetime, and partly by the purchase of his private collection since his decease. In 1850 he projected and

edited the Annals of Anatomy and Physiology; but as his health shortly afterwards began to give way, the journal, after three numbers had been published, was discontinued. In 1853 he was obliged to withdraw for a year from active work; and though after his return from the continent he resumed the duties of his chair, yet he had to depute much of the work he had at one time performed to an assistant. He still continued, however, his original investigations, and in 1856 published a series of memoirs on the morphology of the skeleton. The mechanism of the joints also attracted a large share of his attention, and he has left behind him some manuscript essays on this subject which will shortly be published in the collected edition of his writings. The paralytic affection from which he suffered gradually impaired the vigour of his constitution. At the close of 1866 he could no longer attend to the duties of his class, and he died at Wardie, a suburb of Edinburgh, on the 6th of March, 1867. The retired life he had led for many years before his death gave him much time for private study, and his extensive knowledge of modern languages, as well as the excellent library he had collected, made him well acquainted with the progress of anatomy in all its departments. He worked at his science in a high-toned, philosophic, and most honourable spirit; and in his scientific and personal relations he strove to be candid and just to all men.

William Gravatt was born at Gravesend in 1806. His father, Colonel Gravatt, having been appointed Inspector of the Royal Military Academy, settled with his family at Woolwich; and here William Gravatt acquired his first practical knowledge in military and civil engineering. He was sent in due time to the establishment of Messrs. Donkin and Co. to be prepared for his future profession of civil engineer, and soon secured the good opinion of his masters as well as the regard of his fellow pupils. During his engagement with Messrs. Donkin he was employed for some time at the Thames Tunnel, and was twice instrumental in saving the lives of men who were working there. His first independent employment was in 1832, when he was appointed Engineer to the Calder and Hebble Navigation at Halifax. In 1833 he removed to London, and was elected a Fellow of the Royal Society. About this time he invented the level which bears his name, and introduced the system of reading the staff with the telescope, instead of trusting to the staff-bearer—a method of working which has long superseded all other modes of observation. Mr. Gravatt also contrived another instrument, which he called a Nadir, of great value in carrying out a system of levels in cases when there were obstacles to the employment of a regular staff of assistants. When M. Scheutz brought over his calculating machine to this country in November 1844, Mr. Gravatt announced its arrival to the Royal Society, and took a lively interest in it. He further undertook to explain the mechanism and operation of the machine to men of science and others who chose to inspect it, and afterwards proceeded with the machine to Paris for the same purpose. He

also superintended the publication of a volume of Specimen-Tables calculated and printed by its agency. In 1856 Mr. Gravatt read to the Royal Society a paper upon the theory of the Gyroscope, which was at that time exciting much interest in the scientific world. About the same time he published a short pamphlet upon the propulsion of ships by means of a jet of water, an application of power which seems now likely to be brought into practical use.

Mr. Gravatt's life was unfortunately terminated on the 30th of May, 1866, by the accidental administration of an excessive dose of morphia.

CHARLES JAMES HARGREAVE was born near Leeds in December 1820. After leaving his school (Bramham College) he distinguished himself at University College, London, and took the degree of LL.B. with honours in the University of London. From 1843 to 1849 he was Professor of Jurisprudence in University College. Having acquired a high reputation at the equity bar, he was appointed in 1849 a Commissioner of the Incumbered Estates Court in Ireland. Those who know the nature of the duties and the state of things for remedy of which the court was created, will be aware that a singular combination of legal knowledge, sound judgment, and imperturbable temper was required. All were found in Mr. Hargreave, as was soon seen and acknowledged. The condition of those who came under the jurisdiction of his Court was described, we believe by Mr. Hargreave himself, in a manner which, all jokes being logical fallacies, has no worse fault than the sophism of a part for the whole. He said that punch was no longer known in Galway, only toddy; for whiskey and sugar could be got on credit from Dublin, but lemons required ready money. Of himself, his colleague (Judge Longfield) wrote as follows:—"It may be said that his first experience in a court of justice [he had been a conveyancer and draughtsman] was to preside in it as judge. But no person could observe any deficiency. His patience, his learning, and his impartiality quickly received the respect and confidence of the practitioners in his court, and his unequalled sweetness of temper made him a general favourite. . . . . But he was most in his element when an unusual combination of circumstances and complicated deeds seemed to produce inextricable confusion. His habits of order and his fine mathematical mind at once arranged the rights of the parties with a certainty approaching mathematical demonstration. He never seemed happier than when he was engaged in a subtle mathematical analysis, or in determining the rights arising from a deed when every event occurred except those contemplated by the conveyancer who drew the instrument."

Judge Hargreave (so entitled from 1858, when the court was made permanent) died at Bray, County Wicklow, April 23, 1866. There is no doubt that his constitution was destroyed by his junction of two severe intellectual pursuits; and there is too much reason to fear that the excitement

of his last effort, presently noticed, was the immediate cause of his death. He became a Fellow of the Royal Society in 1844. In the 'Transactions' for 1848, 1850, 1858 appear his papers on Differential Equations, on Linear Equations of Differentials and of Differences, and on the Problem of Three Bodies, the first of which received a Royal Medal. His other communications are to the Philosophical Magazine, 1847–1864. One separate and posthumous work, 'An Essay on the Resolution of Algebraic Equations,' 1866, 8vo (printed for private circulation), demands notice. This speculation especially refers to equations of the fifth degree, or *quintics*. Mr. Hargreave believed himself to have arrived at a solution in the same sense and manner as the cubic has long been solved. The solution of the cubic is not quite pure. An expression having nine values, or three triplets, gives in each triplet the three roots of one of three cubics, which only differ in containing different cube roots of unity. Mr. Hargreave alleges that he produces five similarly associated quintics, of which the five quintuplets of roots can be given in an expression of the 25th degree. The complexity of the analysis, compared with that of a quadratic, is probably in even a higher ratio than the complexities of the *discriminants* (p. 9) of a quintic and a quadratic; and there will be very few readers. The result cannot yet be pronounced upon; but assuredly the thought and the skill employed will remain the subjects of lasting admiration.

His old teacher, Mr. De Morgan, informs us that the most remarkable point, though not the greatest, about Mr. Hargreave was the change in his handwriting. From sixteen to eighteen years of age he wrote in a manner which almost required a microscope to decipher; his examination-papers put the proof of the binomial theorem into the area of a visiting card. He emerged from his legal studies with a round Roman hand of more than average size, and much more than average legibility.

On the 22nd January, 1867, died at Plymouth Sir William Snow Harris, Kt., in his seventy-fifth year. He was the only son of Thomas Harris of Plymouth, Solicitor, whose family had settled in that town as early as the year 1600. He was educated first at the Grammar School at Plymouth, after which he entered the medical profession, and completed his studies in Edinburgh. Harris commenced the practice of his profession as a militia surgeon, and afterwards became a general practitioner in Plymouth; but his love of science, especially of electricity, interfered with his practice.

In 1820 he invented a system of lightning-conductors, by which he became more generally known than by his discoveries. This system had reference chiefly to the defence of the Royal Navy from the destructive effects of lightning, and its peculiarity consisted in permanently fixing sufficiently massive copper bands in the masts, and in leading these copper lines along the ship's timbers to the copper sheathing, so as to afford the

required security at all times, and under all the variable circumstances in which the ship might be placed.

In 1824 Harris married the eldest daughter of Richard Thorne, Esq., of Pilton, North Devon, and from this time he chiefly devoted himself to the cultivation of electrical science. The earlier results of his study at this period were for the most part laid before the Royal Society of Edinburgh in a series of papers, the first of which, entitled "Experimental Inquiries concerning the Laws of Magnetic Forces," appeared in the 'Transactions' in 1829; but the paper is dated Plymouth, July 1, 1827. It contains an account of the hydrostatic magnetometer. The second paper, "On a new Electrometer, and the heat excited in metallic bodies by Voltaic Electricity," is dated May 5th, 1831. The third paper, dated April 5th, 1833, is "On the Investigation of Magnetic Intensity by the Oscillations of the Horizontal Needle." By this time the author had been elected a Fellow of both the London and Edinburgh Royal Societies.

The bent of Harris's mind for improving and constructing electrical instruments is shown at this early period. Indeed his connexion with the Royal Society was in a great measure due to this cause. The President, Sir Humphry Davy, having been attracted by his electrical thermometer, invited him to give an account of it to the Society, which he did in 1826; and his first paper appeared in the Philosophical Transactions under the following title: "On the relative powers of various metallic substances as conductors of Electricity."

Harris's researches on some of the elementary laws of electricity appeared in the Philosophical Transactions in 1834, 1836, and 1839, and they display in a striking manner the author's ingenuity and delicate manipulative skill. He was not satisfied with the attainment of his end by *any* means, but the means themselves were the subject of long and patient thought and repeated trials, until the best means possible under the circumstances had been hit upon. This care in the selection and improvement of apparatus might seem to an ordinary observer to be often superfluous, but it led to success, and to the thorough understanding of the conditions of success; so that the ultimate failure of an experiment in Harris's hands became next to impossible. But with all this love of apparatus, and of its minute details, Harris had none of the spirit of a mere mechanical artist; he knew that the best instrument does the best work only under the guidance of the best mind. But Harris's ingenuity was by no means confined to his apparatus. There was not a room in his house, from the attic to the kitchen, that did not bear marks of an original mind. He converted the ceiling of his children's nursery into a planetarium, and the floor into a compass card. He did not disdain to invent a child's toy, or to rectify a defect in his ingenious kitchen range.

In 1835 the Copley Medal, the "olive crown" of the Royal Society, as Davy loved to call it, was bestowed on Harris in recognition of the value of his papers on the laws of electricity of high tension. In 1839 his

"Inquiries concerning the Elementary Laws of Electricity" formed the subject of the Bakerian Lecture.

In the midst of these researches concerning the general doctrines of electricity, Harris had never ceased to labour at the practical question of the protection of ships from lightning. Erroneous views on the subject were then common; highly educated men and naval officers were found to affirm that lightning-conductors did more harm than good; that they attracted lighting to the structure they were intended to protect, and more to the like effect. A mixed commission of naval and scientific men was at length appointed by Government to take the whole subject under review. The committee met several times in the rooms of the Royal Society at Somerset House, and Harris was called upon to give his evidence. Dr. Wollaston took great interest in the inquiry, and was present during Harris's experiments, which were so satisfactory that the committee earnestly recommended his system for general adoption in the Royal Navy. Still, however, he had much opposition to contend with. Trial had been made of his conductors in the case of ten ships sent to various parts of the world, and experience had fully proved their value, yet an order was given (or threatened) for the removal of the conductors from each ship as soon as it came into dock. But in the mean time the protective effects of his system were so strikingly exhibited on shore, that the order above referred to was never carried out. Some granite chimneys in the victualling-yard at Devonport were in the course of being fitted with the conductors. In the case of one chimney the fittings were completed, in the other the work was delayed in consequence of some adverse order. A storm passed over Plymouth, the protected chimney was unhurt, the unprotected one was struck and rent asunder.

Harris's conductors now began to find favour at the Admiralty, and the scientific discoveries of their inventor were at length recognized in the same quarter, so that he was recommended to the Government as worthy of an annuity of £300 "in consideration of services in the cultivation of science." Prejudices against his system, however, lingered in the minds of naval men and others, and in order to remove them Harris published in 1843 his well-known work on 'Thunder Storms.' He endeavoured also, in papers in the Nautical Magazine and in separate pamphlets, to spread information concerning damage by lightning. He was always on the watch for an illustrative example, and once he got the trace, never gave it up until he had tracked it to the ship's log deposited in Somerset House, or obtained an account from the captain or one of the officers of the ship that had been struck. Accounts of such casees, in the form of letters or pamphlets, he caused to be circulated among persons in authority, including the various foreign ambassadors; and it may be mentioned that Harris's system was adopted in the Russian Navy before it was fully admitted into our own. In 1845 the Emperor presented Harris with a valuable ring and a superb vase, in acknowledgment of the merits of his system.

At length all difficulties in the way of his long-cherished object were overcome. The various objections to his conductors had been met, the merits of his system were clearly understood, and its adoption in the Royal Navy was secured. It was then also felt that some further public recognition was due of the benefit thus conferred on the Naval force and maritime industry of this country and the world. Accordingly, in 1847 the honour of knighthood was conferred upon him, and soon afterwards a grant of £5000 was made to him by Government, in consideration of his public services. In 1850 he was elected an Honorary Member of the Naval Club at Plymouth, and in 1854 of the Royal Yacht Club at Cowes, as an especial acknowledgment of his services to the Royal Navy. Nothing could be more congenial to his tastes, for he was always more of a sailor than a landsman. He had a yacht of his own, and was never tired of exercising it; but he loved to be on the sea in whatever craft.

In the midst of his multiplied engagements Sir William did not forget the claims of elementary instruction in science. The Manuals of Electricity, Magnetism, and Galvanism published in Weale's "Rudimentary Series," and which had a large sale, testify to the activity of his mind in this direction.

Harris's sympathies were with the Bennetts, the Cavendishes, the Singers, the Voltas of a past age: Frictional electricity was his *forte*, and the source of his triumphs. He was bewildered and dazzled by the electrical development of the present day, and almost shut his eyes to it. He was attached too closely and exclusively to the old school of science to recognize the broad and sweeping advance of the new. He was not conscious even of being behind his age when he presented to the Royal Society in 1861 an elaborate paper on an improved form of Bennett's discharger, and still less in 1864, when he discussed the laws of electrical distribution, and still relied upon the Leyden jar and the unit-jar.

Although Sir W. Harris's powers as a scientific inquirer cannot be reckoned as on a par with those of some of his great contemporaries, he was highly ingenious and inventive, a clear thinker, and a suggestive writer. He did his work well, and left his mark on the science of his day; and while some of his labours will be forgotten and others be absorbed and blended with the branch of physical science that he cultivated, still there are many points in Harris's character as a man, and in his habits as a philosopher, which will be dwelt on with pleasure and profit.

In August 1861, on returning from an excursion in his yacht, Sir William was seized with a painful disease of the eyes (*iritis*), which did not yield to medical treatment during some months. He was confined to the house until the following May. In the autumn of 1862, in consequence of a return of the malady, he underwent two painful operations; but the result eventually was, that he lost the sight of one eye, and found the vision of the other much impaired, and his general health weakened.

Recovering, however, in some measure, from this trying illness, he be-

came most anxious to complete a work the materials for which had been accumulating during a series of years. This was a complete Treatise on Frictional Electricity, to which he intended to add biographical notices of the leading electricians of the past. He had also prepared a minute account of the history of the Leyden jar. The first portion of this work was completed, under the supervision of the author, at the close of 1866, when Sir William was seized with his last illness, which ended fatally on the evening of the 22nd January 1867. He bore his sad calamity during five years and a half with the greatest patience, calmness, and fortitude, and was never heard to murmur.

Dr. William Henry Harvey was born near Limerick, on February 5, 1814. Educated at Ballitore School, Kildare, his youth was spent in his father's office at Limerick; but a love of natural history very early developed itself, and even while engaged in business he found time to pursue botanical studies with ardour and success, contributing in 1832 and 1834 articles on "Algæ" to Sir William Hooker's 'British Flora,' to the 'Botany of Beechey's Voyage,' and to Mackay's 'Flora Hibernica.'

His scientific zeal led him in 1835 to accompany his brother to the Cape of Good Hope, to which colony the latter had been appointed Treasurer and Registrar-General. The results of his botanical studies during his stay there he embodied in numerous contributions to periodical literature, and in his 'Genera of South African Plants,' a work the second edition of which was nearly ready at the time of his death. He came home from the Cape in 1839, but returned again in the following year to fill the place of his brother, who had died on the voyage home.

In 1844 he returned to Ireland upon being appointed Keeper of the Herbarium to the University of Dublin. He also received the honorary degree of M.D. of that University, and was soon after elected Professor of Botany to the Royal Dublin Society. His botanical zeal and energy manifested itself by the commencement, in 1845, of the publication of his 'Phycologia Britannica,' a magnificent work, the numerous plates of which were drawn, lithographed, and coloured by himself.

Upon the occasion of being invited to deliver a course of lectures on Algæ before the Lowell Institute, Boston, U.S., he travelled in North America during 1849-50, carefully exploring the coast from Halifax to the Keys of Florida, and collecting material for a large work, the 'Nereis Boreali-Americana,' which was published by the Smithsonian Institute. During 1853-56 he made a long tour in the Southern Hemisphere, the University of Dublin continuing to grant him his full salary, and Professor Allman delivering his lectures to the Royal Dublin Society. Visiting Ceylon, Australia, Tasmania, New Zealand, the Friendly and the Fiji Islands, he investigated and collected the algæ of these countries, and on his return home published a part of his results in his 'Phycologia Australis.'

In 1859 he commenced, in conjunction with Dr. Sonder and with the assistance of the Cape Government, a complete description of the flowering plants and ferns of South Africa, in a work entitled "Flora Capensis," the chief share in the labour of which fell to Dr. Harvey. As a companion to this he also commenced a series of illustrations under the title "Thesaurus Capensis." Neither of these works did he live to see completed.

In 1856, the Chair of Botany in the University of Dublin becoming vacant through the removal of Professor Allman to Edinburgh, Harvey was chosen to fill it, and about the same time he was appointed lecturer at the Irish Museum of Industry.

Unhappily his unceasing labours began to tell upon his health; and though he recovered from a severe illness which prostrated him in 1861, his condition in 1864 became so alarming that he was obliged to pass the following winter and spring in the south of France. The improvement he experienced there was, however, but temporary; and after another winter spent at Dublin in painful attempts to finish the works he had begun, he removed to Torquay, where he died, May 15, 1866.

He was thus cut off before he could receive the full acknowledgment of his many services to science. He had, however, achieved a great reputation as a botanist, and on his own especial subject of Algæ he was admitted to be the first authority. He was an accurate and careful observer; the numerous illustrations, all drawn and lithographed, and many coloured, by his own hand, are evidence that he spared no pains to make his works trustworthy and sure, while the personal expense and risk to which he exposed himself both in his exploring expeditions and in his literary undertakings, testify to his devotion to science. He was as modest as he was meritorious, and his personal character endeared him greatly to his friends. His election to the Royal Society is dated June 3, 1864.

Percival Norton Johnson died on the 1st of June, 1866, aged 73. He was the only son of John Johnson, at one time the only commercial assayer in London; and after working with his father for some years, he established himself in Hatton Garden half a century ago.

He rapidly rose to the highest eminence as an assayer and metallurgist; and his opinion was so much sought after that he could hardly get through the work which crowded upon him.

It is not a little remarkable that the extreme accuracy of his assays was made a ground of objection to them. He for the first time reported the exact amount of gold and silver in the specimens submitted, whereas, before, the quantities had only been stated approximately; and this was not relished by the buyers of bullion, inasmuch as contingent advantages in buying upon exactly known value were not so great. Upon this being represented to him by the merchants, he at once stated that he was willing, if required, to purchase all bars upon his own assays; and this was the reason of his taking

up the refining business, in which he so largely and successfully engaged. His ability in this (as in all other branches that he entered upon) was soon recognized publicly; and when the gold bars from the Brazilian "Gongo Soco" mines, which came over in very large quantities, were refused at the Mint on account of brittleness, he was consulted on the matter, and undertook to refine and toughen them, in which he perfectly succeeded.

It was in this gold that he discovered the existence of palladium; and having succeeded in its separation, he introduced it commercially, at once determining and making known the best uses to which it could be applied.

After he had been in business some years he visited Germany, and was much interested in mining operations there, to which he gave special attention. It was at this time that he met with the compound alloy called "German silver," then in a very crude state of manufacture. He brought over with him some of the metal, analyzed it, and upon the basis of his analysis he commenced and carried on its manufacture, and introduced it to general use, laying the foundation of the enormous business which has since arisen in this branch of metallurgy.

About this time he was much engaged in mining pursuits, and was consulted upon, and visited professionally, nearly all the mines in England, Wales, Scotland, and Ireland, and many important ones abroad. He was the first to introduce into Cornwall the German shaking-, jigging-, and washing-table, with important improvements of his own. He will always be remembered throughout the mining districts for his great kindness and consideration toward the miners, whose social condition it was his constant aim to improve. At great expense to himself, he erected schools in the neighbourhood of the mines, and took an active part in their supervision. He also used his utmost endeavours to alleviate the toil of the workmen in ascending and descending mines, and with this view he, at the Tamar mines, made the experiment of a sloping gallery, which ran for a considerable distance under the river, by which means the miners could walk up and down without the use of a ladder.

Amongst his many inventions of less note may be mentioned several pottery colours, amongst them the "rose-pink," at a time when that colour was much wanted in the potteries.

His greatest success, however, and that which has proved the most valuable to the progress of chemistry and manufacture generally, was the platinum business. To him undoubtedly belongs the credit of having been the first who successfully refined and manufactured platinum upon a commercial scale, and introduced it for the important purposes to which it is specially adapted. The first large and perfect sheet of pure platinum ever produced was made by Mr. Johnson at 79 Hatton Garden; and, seeing the immense importance of the metal, he ever since made it his speciality.

His eminence as an analyst should also be noticed; so great was it, that

the only other commercial assayers in London, though his rivals, used to send him all compounds or minerals of a difficult and complicated nature to report upon for them.

Accomplished as he was in his department, and singularly successful in perfecting whatever he undertook, his opinion was always sought for with earnestness and received with confidence. Few men have worked more perseveringly and effectively for the improvement of their profession.

Mr. Johnson was elected a Fellow of the Royal Society on April 30, 1846.

The life of Sir WILLIAM LAWRENCE, Bart., closed on the 5th of last July. He had nearly completed his 84th year. Not more than two years before his death he resigned his active duties at St. Bartholomew's Hospital, but even then he did not cease from work, and was attacked with apoplexy just previous to an examination at the College of Surgeons.

Lawrence, the son of a surgeon, was born on the 16th of July, 1783, at Cirencester, in Gloucestershire. After receiving a preliminary education at a private school, in his seventeenth year he came to London, and was apprenticed to Abernethy, in whose house he resided for five years. But at the expiration of three years, being then only twenty, he was appointed Demonstrator of Anatomy at St. Bartholomew's Hospital. He discharged the duties of this office with remarkable ability for twelve years. When twenty-two, he received the diploma of the Royal College of Surgeons. Eight years afterwards he was appointed Assistant-surgeon to St. Bartholomew's Hospital, and eleven years later still he became one of the principal surgeons. He held this office for more than forty years, and when he retired in 1865, he was unanimously elected by the Governors to the complimentary office of Consulting Surgeon. Thus from first to last he was connected with the hospital for sixty-eight years.

It is well known that at the early period of his life he worked very hard without interruption, as indeed he did almost to the last. Besides discharging his public duties with rare efficiency, he read very much and wrote too. At eighteen years of age he published an anonymous translation of a Latin work—Description of the arteries of the human body, by Dr. Ad. Murray, Professor at the University of Upsal. In his twenty-sixth year he obtained the Jacksonian Prize for an essay on Hernia. This was the foundation of his important book on the subject, which went through five editions. Lawrence on Ruptures has been for many years past, and will be for many years to come, a standard work on the subject of which it treats. It is remarkable, not only for the thoroughly good English in which it is written, and for clearness and justness of thought, but for the mastery of the subject which it exhibits, and for widely extended and accurate research.

In the year he obtained the Jacksonian prize, he published a translation of Blumenbach's Comparative Anatomy. About the same time appeared

his paper on Fungus of the Testis. The novel plan of treatment there proposed was generally accepted and adopted for many years.

In 1814 he was appointed Surgeon to the London Infirmary for Diseases of the Eye, now called the Royal London Ophthalmic Hospital, and in the following year surgeon to the Royal Hospitals of Bethlehem and Bridewell. These offices he held for many years. He devoted a very large share of his attention to the study of diseases of the eye, and in 1833, after many years of patient observation and reflection, he produced his celebrated treatise on the subject, having previously (in 1830) published a smaller volume on syphilitic diseases of the eye. A comprehensive work, written with all the ability and skill which characterized his work on Hernia, it may be said to have marked an epoch in ophthalmic surgery. It was translated into many languages, a portion of it even into Arabic, and went through many editions in English. The last was published in Philadelphia in 1854.

In 1813 he was elected a Fellow of the Royal Society, and subsequently nominated a Vice-President. But it does not appear that he ever contributed any paper.

In 1815 Lawrence was appointed Professor of Anatomy and Surgery to the College of Surgeons. There, during the years 1816, 1817, and 1818, he delivered those famous lectures on Comparative Anatomy, Physiology, Zoology, and the Natural History of Man, which astonished so many, and aroused such animosity, that had their author been a man of less capability, he would, beyond doubt, have been ruined for life.

The question at issue between the author and his assailants is not one which can be discussed within the limits of this notice. But it may be safely said, that were such a work to appear amongst us now, it would evoke no censure beyond that which lies within the bounds of fair scientific or literary criticism. The doctrine (if it can be so called) which then prevailed concerning the nature of Life, and the weakness of which Lawrence exposed with an unsparing hand, has long since become a dogma of the past; and in the discussion of this great question it is only fair to state that he was far in advance of his time. And, again, his view of the other chief subject, the relation of mind to brain, which was then denounced so fiercely, is (whether right or wrong) identical with that of many of the most enlightened physiologists of the present day. In scientific argument, Lawrence was more than a match for his opponents, but he was assailed by weapons which have happily been since discarded by the champions of knowledge. It may, however, be urged in excuse for the grievous misrepresentation to which he was subjected, that he was not always sufficiently careful to guard against being misunderstood, and proclaimed what he then, and to the last, believed to be the truth, with disregard of, or indifference to, the convictions of those who were then looked up to as the leaders of thought.

In 1816 Lawrence published "An Introduction to Comparative Ana-

tomy and Physiology, being the two Introductory lectures delivered at the College of Surgeons on the 21st and 25th of March, 1816;" and in 1819 he published "Lectures on Physiology, Zoology, and the Natural History of Man." This last was the celebrated volume Lawrence was subsequently induced to suppress; but in 1823, Carlile, without the sanction or consent of the author, indeed, in spite of anything he could do to restrain him, printed and published a volume, entitled "Lectures on Comparative Anatomy, Physiology, Zoology, and the Natural History of Man," which included and was simply a copy of the two volumes mentioned above. Many other editions, variously modified, afterwards appeared. The last, called the 9th, was published by Bohn in 1848.

Lawrence taught at the Aldersgate School of Medicine in 1826–27, but he retired from this in 1829, when he succeeded Abernethy as Lecturer on Surgery at St. Bartholomew's Hospital. He occupied this chair for thirty-three years.

In 1828 he was elected on the Council of the College of Surgeons, and in 1840 one of the Examiners, and subsequently he was twice President of the College. Moreover, he became a foreign Associate of the Institute of France, and a member of a host of other societies. Then he was appointed, at first, Surgeon Extraordinary, and afterwards Sergeant-surgeon to the Queen. Finally, he received a title.

Besides the works already mentioned, Lawrence wrote much. He contributed no less than eighteen papers to the Transactions of the Medical and Chirurgical Society, of which he was for many years a member, and, at one time, President. He wrote many of the articles on Natural Science, some of them at a short notice, in "Rees's Cyclopædia." He contributed also to a work of Watt, entitled "Anatomico-chirurgical Views of the Nose, Mouth, Larynx, and Fauces."

In 1863 (he was then in his eightieth year) was published his last work, "Lectures on Surgery." This, in one volume, by no means embraces the whole course, but only that part of it which was devoted to what may be called the more general subjects, such as the nature of disease, inflammation and its consequences, fever, wounds, and specific diseases. This charming book appeared perhaps somewhat after its time; at all events, it would have produced more effect had its able and accomplished author consented to numerous pressing solicitations to undertake the task—the materials being ready to his hand—many years before. But as it is, better late than never. It is enough to say of it that therein are embodied the most matured experience of the author, and the conclusions at which he had arrived or the opinions he had formed on some of the largest questions which arise in surgery; and moreover it is written with such soundness of judgment and felicity of expression, that it is not only a highly instructive, but a very attractive volume. It is the work not only of a surgeon, but of a scholar.

On two instances Lawrence delivered the Hunterian oration at the Col-

lege of Surgeons. On the last, when the great majority of those whom he addressed were hostile to the views he set forth, his power of public speaking was subjected to a test to which perhaps few men would have proved equal. For the second time in his life did he on a great occasion arouse the anger of his auditory, and excite against himself bitter feeling of resentment. The opinions he expressed on certain questions which were then agitating the minds of many in the profession, and the contempt with which he spoke of certain existing institutions, called forth from all parts of the theatre loud expressions of dissent and denunciation. Probably most of those present thought that the orator would shrink from so unequal an encounter, and bow to the verdict which was so unequivocally pronounced. But Lawrence never for an instant wavered from his purpose. He, standing alone, calm and unshaken by the storm which raged around him, proceeded in a strain of unfaltering eloquence to the end, and concluded with a peroration of such power and beauty that those who had, during the hour, been loud in condemnation, could not resist joining heartily in the burst of applause which greeted the close. Undoubtedly he then exhibited some of the greatest qualities of an orator.

Lawrence was indeed richly endowed by nature, and he spared no pains to turn his great advantages to good account. Throughout his long life he enjoyed almost uninterrupted health; for within not many months of his death, he declared that he had never been kept from his duties by illness for a week together. His excellent health, notwithstanding hard work, continued through so many years with scarcely any intermission, although no doubt originally the result of an unblemished constitution, was yet, from first to last, carefully preserved by habits of singular regularity and uniform temperance in all things. No man could be less self-indulgent than he, no one was ever more orderly in his work or punctual to his engagements. In person too he was much admired. Above the ordinary height, elegant in form, strikingly handsome, and of noble presence, few who met him could have failed to be impressed. But Lawrence was still more remarkable for his powers of mind. His intellect, naturally of extraordinary strength and amplitude, had been very long and most industriously cultivated. Throughout his life, from the time when he first entered the hospital to its close, he was a diligent student, not of surgery only, in its highest and widest sense, and of those natural sciences upon which it is more immediately founded, but even of subjects less akin than those are to that which formed the business of his life.

Apart from all professional knowledge and skill, Lawrence was undoubtedly an accomplished man. He was a good classical scholar, and spoke fluently French, German, and Italian. His knowledge of history, both ancient and modern, was extensive, and in some parts perhaps profound. To the last he was true to the habit of his whole life. He not only read very much, and remembered to an extraordinary degree what he read, but thoroughly digested it, separating with remarkable skill the wheat from

the chaff. But beyond the more immediate occupation of his life—surgery, his chief affection was centred in those sciences which are included in Biology. He was not content to cultivate these sciences only, so far as they are supposed to be directly applicable to practice, but, from first to last, he pursued them for the charms they possessed. The zeal with which he studied, especially in early life, anatomy and physiology, human and comparative, is well known. Throughout his long career his interest in these subjects never flagged.

Again, it is acknowledged by those most competent to judge, that he wrote well. His style was excellent—correct, perspicuous, and graceful. He spoke, too, admirably. Whether in private or in public, he never appeared at a loss either for thought or expression, but was always lucid and to the point. From egotism and all affectation he was supremely free.

Although he was very fond in his later years of discussing with his friends any of the great questions of physiology, it does not appear that he ever committed his thoughts to writing. But those who listened with delight to his conversation were often led to regret that he was never induced to set forth his views in a more permanent form. There was something very attractive in the power of mind and range of knowledge which he, on almost all occasions, seemed to hold in reserve. But two facts especially impressed themselves on those who talked with him. He was always thoroughly well read in the subject, and his views, unfettered by prejudice, were ample and enlightened.

As a surgeon, Lawrence was distinguished rather by his clear and vigorous conception of principles than by any special or peculiar skill in the details of practice. His was no low or narrow view of the requirements of surgery; its practice in his hands was no mere empirical or mechanical art, but he thought it worthy of the highest powers of the intellect. He was no mere skilled artisan, but a thorough philosopher. His great ability was never more conspicuous than in prognosis. At times, with marvellous dexterity, he would predict the issue of a case which baffled or misled every one around him. Thus his most daring achievements as an operator were often crowned with success, which to others appeared in the distance as the remotest chance.

As an operator, he was decidedly skilful; but his skill was shown not so much in grace of action or in apparent manual dexterity, as in the ability with which he accomplished the end in view, and the manner in which he brought the operation to a close. Near-sighted in early life, his vision in his declining years, except towards the end, not only did not fail, but in some respects improved. To the last he operated without glasses, and those which for some years he occasionally used at other times, still later he almost entirely laid aside.

All his colleagues and pupils, and many others, were familiar with his amazing coolness and perfect self-possession under the greatest difficulties. He seemed never to be disturbed, even by the most perplexing and unto-

ward occurrence. Beyond the faintest blush which, perhaps, now and then for a moment passed over his countenance, no indication of any kind appeared that he was in the least degree affected. This wonderful equanimity made him the same at all times and under every circumstance. Whatever he may have occasionally felt was known only to himself. So far as observation went, the worst difficulties of an operation, or the storm of an indignant assembly, alike failed to evoke any evidence of emotion.

In the memoir of Sir Benjamin Brodie, which appeared in the Proceedings of the Society, it was truly said, "There is no profession where a man may in his lifetime be so distinguished, and leave behind so slight a record of his life, as the profession of Medicine or of Surgery. With the death of the man, there perishes in such case a vast amount of personal skill and observation, which, being unwritten, and, indeed, not capable of being written, can be amassed again only by the combination of similar talent, opportunity, and industry in another individual." Lawrence was an eminent illustration of this. Great and various as the merits are of the chief of his published works, they afford no adequate conception of their author. Lawrence, too, was greater in speech than in writing: as a lecturer, in his best days, he was probably without a rival; and those who knew him well will endorse the opinion of Sir Benjamin Brodie, that he was even greater in ordinary conversation than in public speaking. But, in truth, just as now his fame will not rest upon any single brilliant discovery, but upon the record of his whole work, so during life he was not remarkable for one great faculty in particular, but rather for that harmonious combination of various powers which made his character so complete. As a surgeon his name must ever be amongst the foremost of those which adorn the annals of his profession. W. S. S.

John Lee, eldest son of John Fiott (a descendant of an old Burgundian family), was born April 28th, 1783. He entered St. John's College, Cambridge, and in 1806 was fifth Wrangler, the senior for that year being Sir Frederick Pollock. In 1816 he took his degree of LL.D., and was afterwards elected a Fellow of the College, when, as Travelling Bachelor, he travelled widely on the Continent and in the East, during which time he gathered materials for the interesting collection of antiquities, which were subsequently arranged at Hartwell. In 1815 he changed his name by royal licence from Fiott to Lee, in compliance with his uncle's will, his mother having been daughter of William Lee, Esq., of Totteridge Park, and granddaughter of Sir William Lee, Lord Chief Justice in 1754.

In 1827 Dr. Lee succeeded to the whole of the family property, and became Lord of the manors of Hartwell, Stone, and Bishopstone, and patron of the livings of the two places first mentioned. The presentation to these livings he afterwards conferred on the Royal Astronomical Society, of which he was Treasurer from 1831 to 1840, and President in 1861–62.

A few papers on archæological subjects constitute nearly the whole of Dr. Lee's contributions to literature; but as a patron he excelled, and well deserved the designation of "our science-loving friend," bestowed on him by the late Admiral Smyth. One of his favourite occupations was the increase of the collections he had brought home from his travels; and visitors to the stately mansion of Hartwell will remember the pride with which the venerable owner conducted them from room to room, and fondly described his various treasures. His appreciation of antiquities led to his being chosen as Chairman of the Congress of the Archæological Association and two local societies, which met on Bosworth Field in 1862.

Dr. Lee's chief claim to scientific consideration was the building of an observatory in the grounds at Hartwell, where for many years observations were carried on, which are of recognized value in astronomical and meteorological science. It was there that Admiral Smyth continued his 'Cycle,' which was afterwards published, in the handsome quarto known as 'Speculum Hartwellianum.' The cost of this and three other works, by the same hand, 'Descriptive Catalogue of a Cabinet of Roman Imperial large Brass Medals,' 'Ædes Hartwellianæ,' containing an account of the scientific observations there made by different observers, and of the mansion and its valuable contents, and 'Addenda' to the same, was defrayed by Dr. Lee.

Dr. Lee was twice married, but died without issue. He was a Member of Doctors' Commons, in which he served the offices of Treasurer and Librarian, though he never entered actively into the practice of his profession. His appointment as Queen's Counsel by Lord Chancellor Westbury in 1864, afforded him high gratification. At the time of his decease he was the oldest magistrate in Buckinghamshire, having been named on the Commission of the Peace in 1819.

Though somewhat eccentric in manner, Dr. Lee was considerate and bountiful to those around him. As a master and landlord he was much respected. He gave a thousand guineas towards the establishment of the Bucks County Infirmary. He founded the Lee Fund of the Royal Astronomical Society for the relief of widows and children of deceased Fellows, and presented to the Society the 'Lee Circle,' a valuable astronomical instrument. He was elected a Fellow of the Royal Society in 1831, and at the time of his decease, which took place February 25, 1866, was a Fellow of the Society of Antiquaries, of the Linnean, Geological, the Syro-Egyptian, and other learned bodies.

Samuel Roffey Maitland died January 19, 1866, aged 74. His father was a merchant, of Scotch extraction, and in England a Nonconformist. The son was brought up in his father's persuasion, and accordingly, though he was for some time at Cambridge, he could not proceed to a degree. He was of St. John's College from October 1808 to February 1810, when he migrated to Trinity College, where, however, he never went into residence. He was called to the bar, practised for a year with good success, and then

abandoned the profession. He soon married, and settled at Taunton, where his father also settled. He then began the course of reading and collection of books, which formed the basis of his future career. In 1821 he took orders in the Establishment, and became the incumbent of a new church at Gloucester. This post he resigned in 1830, having in the mean time found that his vocation lay towards theological writing; he had then published various controversal tracts on the prophecies. In 1838 he was appointed librarian to the Archbishop of Canterbury (Howley), from whom he received the degree of D.D.; at the death of that prelate, in 1848, he returned to Gloucester, where he passed the rest of his life. From 1838 to 1865 is the period in which the works were produced which made him conspicuous among writers on mediæval history and theology.

We need not enumerate Dr. Maitland's numerous and multifarious writings; perhaps the most read is the 'Dark Ages, a Series of Essays,' in indication of the period so called from the common charge of neglect of literature and of the text of the Sacred Scriptures. He was not a popular writer; his subjects are too recondite, and his learning too profound.

But he is one of a class of whose writings it must be said, that wherever they take they bite. They are imbued, but not in excess, with a kind of humour which seems almost their own; some would describe it as quaint, but this word alone only distinguishes its class from others; a journalist describes it as "sly, dry, and shy, but never high." It has more likeness in it to the peculiar humour of Pascal than is seen in any other writer of our day. The character of Dr. Maitland's learning is that of the man who reads books which he has always by him, as distinguished from that of the man who knows how to go to the library and find by references. He had nothing to do with libraries except his own, and that of which he was for ten years *in loco possidentis*. Of this library he published a list of the English works previous to 1600 which are found in it, with valuable biographical references. As well as a man of letters, he was a book-fancier, and in early life a little of a bibliomaniac. His taste for these articles led him, when he first began to collect, to learn to bind them; and the writer of this notice remembers endeavouring, when a boy, to extract a book lettered 'Maitland's Works,' and finding that he was trying his strength upon one of the uprights of the bookcase, all of which were backed and lettered by the owner.

Dr. Maitland became a Fellow of the Society in 1839. He was for some years Editor of the 'British Magazine.' Cautious in the highest degree about literary investigation, he was by temperament a bold schemer. Long before Sir Rowland Hill appeared in the field he proposed to the Minister of the day that the Government should carry letters *for nothing*; he was satisfied that the deficit would be much more than made up by the impulse given to trade; and there are presumptions in favour of his view in the extraordinary tendency upwards of the revenue since the great change in the Post Office. In literature he was decidedly of opinion that it would be

amply worth its cost for the Government to pay for the construction of an index which should give reference to every human name mentioned in every book, from the invention of printing down to a recent period.

George Rennie, C.E., was born in the parish of Christchurch, Surrey, on the 3rd of December, 1791, and died on the 30th of March, 1866, at his house in Wilton Crescent, London.

He was the eldest son of the great engineer John Rennie, and from his early years was destined for his father's profession. His school education was commenced under Dr. Greenlaw, of Isleworth, and continued at St. Paul's School under the mastership of Dr. Roberts. He was then sent to the University of Edinburgh to pursue his academical studies, and during two years of his stay enjoyed the great advantage of living in the house of Mr. Playfair, the accomplished Professor of Natural Philosophy, and author of the celebrated 'Illustrations of the Huttonian Theory of the Earth.' On his return to London in 1811 he entered on the practical study of engineering, and was soon able to assist his father in all the departments of his profession.

In 1818, on the recommendation of James Watt and Sir Joseph Banks, he was appointed to the office, then become vacant, of Inspector of Machinery and Clerk of the Irons (*i. e.* dies) at the Royal Mint; and during the eight years that he held the appointment he made himself intimately acquainted with the construction and operation of the machinery employed in coining. The knowledge thus acquired stood him in good stead when he was called on, in conjunction with Messrs. Bolton and Watt, to furnish machinery for the Mints of Calcutta and Bombay, and, at a later time, for those of Mexico, Peru, and Lisbon, and when, in the reign of Louis Philippe, he designed similar but more extensive machinery for the Paris Mint.

On the death of his father he entered into partnership with his younger brother, now Sir John Rennie, and went through a long and active professional career, during which the brothers were engaged in designing and executing many engineering works of great magnitude and importance, at home and abroad. Among these may be mentioned various national harbours and docks and their subsidiary machinery; extensive drainage works; railway surveys and constructions, and as specially worthy of note, the first surveys of the present line of the Liverpool and Manchester railway, boldly and successfully carried by the Messrs. Rennie's advice over the Chat Moss, and the Namur and Liege, and Mons and Ménage lines, planned and executed by them in Belgium; also bridges in various parts. London Bridge was erected after a design made by George Rennie, which had been approved by his father; but in consequence of his then holding a government appointment his brother was appointed engineer to carry the work into execution. The Bridge over the Dee at Chester, with an arch of 200 feet span, is a monument of Mr. George Rennie's skill in this species of construction. The original design of this great stone arch was by Mr. Harrison, a well-known

architect; but as, through age and infirmity, he was unable to proceed with it, the whole plan was remodelled and rendered practicable, and the arch equilibrated with great scientific precision by Mr. Rennie, who further showed his constructive power by devising a very ingenious centre for supporting the arch during the process of building.

Mr. Rennie took much interest in the question of propelling vessels by the screw. The screw-propeller had been already tried at various periods and in various forms. Mr. Rennie's attention was specially drawn to the subject in 1836, by certain successful trials then made by Mr. Pettit Smith with a screw-propeller fitted to a small open boat. After due consideration of the results of this experiment, Mr. Rennie was satisfied that the method would be practicable and advantageous when applied on a larger scale, insomuch that he and his brother, at no small pecuniary risk, combined with some other enterprising persons to carry it into effect, and a vessel called the 'Archimedes' was built for the purpose, and fitted with a screw driven by engines of 80 horse-power. Notwithstanding adverse predictions, the 'Archimedes' succeeded perfectly; and the Messrs. Rennie subsequently, in 1840, constructed for the Admiralty an iron vessel of 210 tons and fitted it with a screw-propeller, by which a speed was obtained of twelve and a quarter miles per hour, which was four miles above the rate of the Admiralty paddle-steamers of that period. This vessel, the 'Mermaid,' was the first screw-propelled ship in the British Navy, and her introduction is a memorable event in the history of steam navigation.

Throughout his busy practical life Mr. Rennie bestowed much time and thought on the scientific side of his profession, and undertook various experimental inquiries on physical questions connected with it. Three papers containing the results of some of his investigations are published in the Philosophical Transactions, viz. "On the Strength of Materials," in 1818; "On the Friction and Abrasion of the Surfaces of Solids," in 1829; and "On the Friction of Fluids," in 1831. To the British Association for the Advancement of Science he made two communications on the Quantity of Heat developed by Water when rapidly agitated, and presented to the same body an elaborate Report on the progress of Hydraulics as a branch of Engineering, published in the volumes for 1833 and 1834; also Reports on Railway Constants (1838), and on the Changes in the Channels of the Mersey (1855 and 1856). He is the author of various contributions on bridges, water-wheels, and other subjects of practical engineering, in the 'Transactions of the Institution of Civil Engineers,' in 'Weale's Papers,' and in Woolhouse's edition of 'Tredgold on the Steam-engine.' He also brought out a new edition of 'Buchanan on Machinery,' and added much new matter.

Mr. Rennie was elected a Fellow of the Royal Society in 1822; in 1845 he was nominated a Vice-President, and appointed Treasurer in succession to the late Sir J. W. Lubbock, which office he held till 1850. He was also a member of several foreign academies.

Mr. Rennie married, in 1828, Margaret Anne, daughter of the late Sir John Jackson, Bart., M.P., by whom he has left issue two sons and a daughter. Some time before his death he met with a severe accident, from the effects of which he never recovered.

Mr. Rennie was a man of gentle nature and quiet demeanour, in private as well as in public life greatly esteemed and respected.

Henry Darwin Rogers was born in Philadelphia in 1809. At the age of twenty-one he was appointed Professor of Chemistry in Dickinson College, Carlisle, Pennsylvania, and afterwards to the Chair of Geology in the University of Pennsylvania. Then followed his superintendence of the Geological Survey of the State of New Jersey, residence at Boston, voyages to Europe, and (in 1857) acceptance of the Professorship of Natural History and Geology in the University of Glasgow, in which honourable post he died in May 1866.

Prof. Rogers published his Report on the Geology of New Jersey in 1835: a year later he was intrusted with the important task of investigating and rectifying the geology of the great State of Pennsylvania, in which he spent several years of earnest labour. His brother, Prof. W. B. Rogers, was at the same time employed in the preliminary survey of Virginia, whereby some of the most important problems in American geology—the structure of the Appalachian mountain-chain, and, indeed, of half the continent of North America—were simultaneously worked out by two of the ablest observers of the day. The results of this survey, discussed in a joint Report, were communicated to the Meeting of the American Association of Geologists and Naturalists held at Boston in the summer of 1842, "with an eloquence and fascination of style never surpassed." It placed the two brothers in an eminent position, recognized by the geologists of the world.

The anticipations formed on this occasion were fully confirmed by the final Report on the Geology of Pennsylvania, which was published at Edinburgh, with maps and sections, in 1858. To ensure the bringing out of this valuable work in a style commensurate with its importance, Prof. Rogers came to England, and, while residing in Edinburgh, made intimate acquaintance with many men of mark in the literature and science of Scotland. In society, and as a lecturer, his great and varied knowledge gave him an advantage which he exercised with graceful facility, and on favourite topics he would at times surprise and charm his hearers by bursts of eloquence.

Besides the works above mentioned, Prof. Rogers wrote papers and reports, which were published in the Reports of the American and of the British Association for the Advancement of Science, the Transactions of the American Philosophical Society, the Journal of the Boston Society of Natural History, in Silliman's Journal, the Edinburgh Philosophical Journal, and the Proceedings of the Geological Society. The long list of

subjects therein contained may be regarded as a memorial honourable alike to the author and to American science.

For a few years prior to his decease, Prof. Rogers was one of the editors of the Edinburgh Journal above referred to. His health being delicate, he spent his last winter in Boston, in company with his brother, and returned to Scotland but a short time before his death. He was elected a Fellow of the Royal Society in 1858.

William Parsons, third Earl of Rosse, was born at York, June 17, 1800, of a family which had been settled in Ireland from the time of Elizabeth, many of whom were distinguished in arms, at the bar, and in the Irish House of Commons. His father was one of the most eloquent persons of his day, and was the author of some works of value. Lord Rosse was educated at home by a private tutor, and when 18 years old entered Trinity College, Dublin. His career there was very successful both in science and classics, for he obtained all honours that were possible. He did not graduate there, though he answered for the degree; but in compliance with his father's wish went to Oxford, where he entered Magdalen College. At that time Oxford was far behind Cambridge in the cultivation of mathematical and physical science (to which he had now devoted himself), and he seems to have regretted this, though, no doubt, he did avail himself well of the instruction which he found. On leaving Oxford, he was returned for the King's County, which he represented in Parliament for eight years. He was not an active debater, though, whenever he spoke, he commanded attention; but in matters of business, and especially on committees, he had few equals. He then retired for a few years from political life to follow his favourite pursuit with more leisure, and discharge more completely the duties of a landed proprietor, in which he was exemplary. He was not exclusively devoted to astronomy or mechanics. In fact few minds of our day have grasped so wide a range of knowledge. He was a master of political economy, a greater one than many of its more renowned champions; for he brought to bear on it the sound common sense which was one of his chief attributes. He also devoted much attention to the great question of national education, and the loss of his authority and influence on that subject is deeply felt in Ireland at the present moment. He was a good chemist, would have attained a high position as a civil engineer, and possessed a large amount of military and nautical knowledge. As evidence of this it may be mentioned that he had formed the conception of armoured ships some years before the Crimean War, and had thoroughly investigated the problem. His calculations showed that vessels of no great tonnage could carry a sheathing of 4-inch plates which would be proof against the 32-pounder, the normal gun of the time. His elaborate memoir was sent into the authorities, acknowledged, and probably forgotten.

In 1836 he married Miss Field, the daughter of a Yorkshire gentleman,

in whom he found one worthy of himself. She sympathized in all his pursuits, mastered enough of astronomy to help him in his calculations, and entered into all his plans for the welfare of his tenantry and the good of her adopted country. And this last required no common strength of mind, for there was what might well startle a young Englishwoman. Kind and beneficent as her husband was, he was not less resolute in supporting the authority of law and putting down the murderous societies which were the terror and curse of that part of Ireland. This, of course, made him a mark for the assassin; he knew his danger; but the knowledge neither made him shrink from his duty, nor embittered his feelings against the misguided people who were conspiring against him. He held on his steady way, sustained by his calm determined courage, and perhaps by the fear inspired by his great physical power and consummate skill in the use of arms. For several years this danger existed, so that in his own park and at his telescope it was felt that all who could use weapons had better carry them. This continued till the terrible famine which ensued, crushed out under the weight of real misery the imaginary grievances of the agitators and showed men as they were. And none bore the test better than Lord Rosse, who applied to relieve the distress which surrounded him, not merely the power which belongs to habits of business and sound judgment, but something still more appreciable to common eyes; for during some years he devoted nearly all the income of his Irish property to give the unhappy sufferers the means of existence. This told on their hearts; and now also they began to be proud of his fame and to regard him as an honour to their nation, so that for the latter part of his life he ceased to be an object of hostility.

On the death of his father in 1841, he was elected an Irish Representative Peer; in 1831 he had been appointed Lord Lieutenant of his County; from 1848 to 1854 he was President of the Royal Society; and in 1862 he was elected Chancellor of the University of Dublin.

His appearance promised a long life, but it was cut short by an accident so trifling that it was neglected till too late. A slight sprain of the knee produced, after some months, a tumour which was ultimately removed by a severe operation. The wound was slowly healing, but his strength sunk in the process; and on October 31 he died as he had lived, patient and uncomplaining under his long and acute sufferings, gentle and considerate to all around him, and strong in Christian hope.

We think these personal details will interest the Society on their own account; but they also may serve to illustrate the habits of thought and action which guided Lord Rosse in those researches which culminated in the magnificent instrument with which his name will ever be connected.

So early as 1826 his attention was directed to the improvement of the reflecting telescope, and it is instructive to trace the steps of his progress as recorded in his papers in Brewster's Edinburgh Journal, and those which appear in our Transactions for 1840, 1850, and 1861. They are

entirely original; for there was no available information except the memoirs of Mudge and Edwards, which were only applicable to specula of four or five inches diameter. Lord Rosse's starting-point was the necessity of using speculum metal of the highest standard. For this he combined four equivalents of copper and one of tin: this compound is very brilliant and resists tarnish far better than if there be a slight excess of either ingredient. A striking example of this is a compound speculum polished in 1830, which lay neglected in his laboratory, yet a few years ago was still quite bright. Arsenic and other metals which have been recommended in small quantity, he tried, but preferred the simple alloy. This, however, is nearly the most intractable of all materials—harder than steel, more brittle than glass, friable, crystalline, and, worst of all, flying in pieces with any sudden change of temperature. In consequence, the common process of the founder does not avail here, except on a small scale; and all large specula which had been previously made contained a larger proportion of copper than the above, that they might have the necessary toughness. Yet more, he shared the general belief that the polishing could only be done by hand, that it was essential to feel the nature of the contact, that therefore only small surfaces could be accurately figured. For both reasons he was led to build up a speculum of small pieces. His first attempt was very ingenious. He combined a central disk with annular zones two or three inches broad, and ground and polished them spherical. In such a surface, each zone is of shorter focus than those within it, and the resulting image would be indistinct. But by a fit adjustment, each, beginning at the centre, was drawn back till their foci coincided; and the action of the compound was good even with an aperture of eighteen inches. Yet the complexity of the arrangement and its liability to change with temperature were weighty objections, and he was led by a new fact to devise another plan. In polishing these rings, he found the outer ones too large for hand-work, and made in 1828 a machine which gave a rectilinear motion to the polisher, while the speculum revolved slowly. This was only expected to produce a spherical figure, but it caused rings and other irregularities, which he saw would be corrected by adding a second excentric, which, by giving a lateral motion to the polisher, changed its course into a curve which might be varied from a right line to an ellipse almost circular. This had the desired effect; but on watching its action he saw that it would also give what was the great desideratum, a change of curvature from the centre to the circumference which could be varied at pleasure, and therefore could be made to give a true aplanatic figure. The machine so altered was employed through his whole life with only one important change, which was a means of setting the six-feet vertical while on the polishing machine, and testing it by a terrestrial mark. This was added in 1862, and very much facilitated the obtaining a perfect figure. He met the difficulty of obtaining a large speculum by making a strong frame of a peculiar brass which has the same expansion as fine speculum-metal, and soldering on its

front thin plates of the latter, cast on as large a scale as possible, and closely fitting them at the edges. From what has been said of the properties of this alloy, it may be judged how much mechanical skill the process required; but he succeeded in making a three-feet speculum, described in the Transactions for 1840. It was very light and strong, and defined perfectly, except that with large stars the diffraction at the joints produced four minute rays. For those below the fifth magnitude, and of course for fainter objects, this defect is insensible; and Lord Rosse believed that in this way only would it be possible to attain the extreme limit of telescopic vision. The construction is stronger than the solid speculum with a third of its weight; and the cellular distribution of the greater part of the mass enables it to assume the temperature of the atmosphere far more rapidly. During these experiments he was led to an important improvement in the casting of speculum-metal. When the melted alloy is poured into a mould the surfaces of it in contact with the mould harden, while the interior of the mass is still fluid; this cools in its turn, and in contracting exerts a powerful drag on the outer crust which gives way, and the whole is shivered. Besides, if the solidification is gradual the mass assumes a crystalline character, which gives it, when polished, a mottled surface. Lord Rosse met these difficulties by forming the bottom of his mould of iron, the sides of sand, and by leaving the top open: the metal in contact with the iron congealed almost in an instant, the sides of the speculum more slowly, as the sand is a worse conductor than the iron, the upper part remaining fluid longest; so that the contraction occurred chiefly at the back of the speculum, where it did no harm, while the front presented a layer of uniform and comparatively tough material. He found, however, that this layer is a little more liable to tarnish than that which is cooled slowly. The process is quite different from the chilling of cast iron, with which it has sometimes been confounded. All this was well; but now another difficulty was found. Copper in fusion absorbs a large quantity of oxygen, much of which it gives out on becoming solid, and speculum-metal appears to possess the same property. Owing to this it is always full of microscopic pores, and the escape of the gas when the alloy is cooled by contact with the iron, as it cannot rise through the viscid film which is formed, probably causes the bubbles and cavities which are found even when no air is entangled in the pouring. To give this gas a free escape was the obvious remedy, and that was effected by making the bottom of the mould of hoop iron, placed on edge and packed so closely that it retained the metal, but was pervious to gases. The plan was so successful that in 1840 he had finished a solid three-feet speculum, and had satisfied himself that even a six-feet was quite practicable.

He soon made the attempt, and succeeded in April 1842, in obtaining a perfect cast which, after being partly ground, was broken by the carelessness of a workman: two other failures are mentioned in his third paper; one of which, however, actually gave a disk more than seven feet diameter,

though of course too thin for use. This he regarded as a valuable result, for it showed the possibility of going far beyond the six feet, should such optical power ever be required. In these disasters his unfailing good temper and patience were not less admirable than his exhaustless mechanical talent. The whole mighty instrument was so far complete by February 1845, that on the 13th of that month he, his friend the late Sir James South, and another, saw in a lucid interval of clouds a few double stars and clusters.

Lord Rosse has given in the paper just referred to (1861) ample details of the telescope, both in respect of casting and figuring the specula and of the mechanism by which it is pointed to the heavens, and the observers enabled to use it with convenience and safety. In these we need not follow him beyond noticing the modest and undemonstrative character of the description, which gives but a very faint idea of the magnificence of what he had achieved. To those of us who have seen the telescope, and still more to the few who were present at the casting and polishing of the specula, it is but a lifeless shadow. They can never forget the machinery and furnaces that were installed in the towers and courts of the old castle whose walls still show traces of the sieges sustained in times of yore, the intelligence and discipline of the workmen whom he had formed from ignorant Irish labourers, and above all the matchless self-possession and unfailing resources of the master mind that guided all. In even beholding the instrument it is not easy to realize its colossal vastness; the machinery which moves it disappoints by its excessive simplicity, and it is not till one stands in the highest observing gallery and looks into the profound below, or over the surrounding country, that he obtains a true measure of its magnitude, and feels that it is sublime. It deserves notice that all this massive work was executed in Lord Rosse's laboratory, and that the whole had been so thoroughly considered that in no instance was it found necessary to deviate from the drawings which had been prepared.

With respect to its optical power, little need be added to what is said in the memoir of 1861. A more unfavourable location for it could hardly be found than the vicinity of the Bog of Allen; and its performance gives little earnest of what it could do in a finer climate and a few thousand feet above the sea. Hitherto it has also suffered from a disturbing cause peculiar to reflectors, and increasing very rapidly with their bulk. Besides the undulations of the air through which they look (which affect all telescopes), they have air-currents in their tubes caused by the great speculum being warmer than the atmosphere. The air in contact with the metal is heated, and rises along the upper side of the tube, while a stream of cold air replaces it, descending on the lower; and the eddies of the two cause strange wings and twirlings in the image. This goes on increasing as the night becomes colder, till sometimes all definition is lost. An example of this was given on March 5, 1845. The speculum was uncovered while cooler than the air, and was, of course, dewed. This did not dry off till

Regulus was passing; that star was shown as a round point of intolerable light; but as the night wore, the definition declined till ϵ Bootes came, which it only showed as two coloured flares, and Lord Rosse gave up further observing as useless. On October 20, 1848, under similar cicumstances of uncovering the speculum, the blue star of γ Andromedæ was seen with 1500 divided a full diameter of the larger component, the colours blue and purple. The previous evening, after a longer exposure, the star looked elongated, and the division appeared only by glimpses. For this there seem but two possible remedies, to keep the speculum as near the temperature of the air as possible, and to make the tube of open work, so that the warmed air may escape as soon as possible. The first of these was not easily practicable with so huge a block of metal, though he thought it could be effected; but the other, which was many years ago suggested by Sir John Herschel, he tried on the three-feet, and with such success that he resolved to apply a similar tube to the six-feet when it should require renewal. This will undoubtedly extend its good definition through a greater portion of the night, and make it more potent for the resolution of nebulæ. But, even under average atmospheric conditions, its power is astonishing. No achromatic in Britain gives any adequate notion of the way in which it shows the moon, close contrasted double stars, or clusters like 13 Messier. Its chief employment, however, has been on nebulæ, and the results which they have afforded are very remarkable. That it resolved a multitude of them which resisted all other telescopes was a matter of course; but it also revealed in many of these strange bodies the existence of forms and forces completely at variance with our previous conceptions of celestial mechanics. Many of the observations are given in our 'Transactions' for 1850 and 1861; and it may suffice to indicate a few of their results. The most remarkable is the spiral arrangement, which seems to prevail very commonly in nebulæ, and occasionally in clusters. Sometimes the spirals are as regular as the fire-curves of a pyrotechnic wheel; sometimes, when seen obliquely, like the section of a snail-shell; sometimes almost foreshortened into a ring; occasionally there is a double system; and in one or two instances the two sets turn in different directions. The dynamical condition which is implied by these appearances cannot be explained by any cosmical forces with which we are acquainted, and it is an object of the highest interest to science that they should be carefully watched for any signs of change.

It has also been ascertained that a remarkable class of nebulæ, called Planetary from appearing like uniform circular or elliptic disks, have no existence; they are shown to be systems of rings, sometimes very complicated, so that annular nebulæ, of which only two were previously known in our hemisphere, are by no means uncommon. And thirdly, some information has been gained as to nebulous stars. These rare objects differ from ordinary stars in having a round atmosphere of some extent, and faintly luminous. This is not continuous, as had been supposed; some-

times consisting of rings, sometimes separated by a dark interval from the central star. Such facts do not well accord with the old nebular hypothesis, but rather indicate a permanent state like that which last year prevailed for a few days in the noted star of the Northern Crown, in which a solid luminous nucleus is surrounded by an atmosphere heated to incandescence. It is far from improbable that such is also the case with many of those minute stars which this telescope reveals in thousands in bright nebulæ, which it cannot entirely resolve. The stars and the nebula are evidently connected, yet as evidently of different nature. As an example may be taken the great nebula of Orion, in which, when the speculum is in fine order, it shows these lucid points as thick as dust, while yet there is manifestly a different source of light. In this case the nebula seems brighter than the stars; and so Mr. Huggins has found that the nebular spectrum alone is visible. On this, and many similar questions, each new observation opens an ever-widening field of inquiry; and it is permitted to us to hope that the unrivalled power of this mighty instrument will still be exerted in that field, and that the son and successor of him whom we so deeply regret will follow his example in this, as in all else for which he was honoured and loved.—T. R. R.

James Smith, of Jordan Hill, near Glasgow, was born in Glasgow on the 15th of August, 1782. He was the eldest son of Archibald Smith, Esq., an eminent West India merchant in that city, and of Isabella Ewing, who died a few years ago in her 101st year.

Mr. Smith was educated at the Grammar School and University of Glasgow. He was for many years a sleeping partner in the West India House of Leitch and Smith in that city, but never took any active part in business. His tastes directed him to literary and scientific pursuits and the fine arts. He was in early life an enthusiastic book collector, particularly in the department of early voyages and travels, of which he has left a large and valuable collection. His love of yachting was one of the most prominent features in his life and it was lifelong. His first cruise in a yacht of his own was in the year 1806; his last in the year 1866. He was one of the earliest members of the Royal Yacht Club, now the Royal Yacht Squadron; and was one of the earliest and latest Commodores of the Royal Northern Yacht Club.

Most of Mr. Smith's scientific and literary researches were connected with his love of yachting. His earliest paper in any scientific publication was a notice in the Transactions of the Royal Society of Edinburgh of an undescribed vitrified fort, in the Burnt Isles in the Kyles of Bute, discovered by him in accidentally landing from his yacht. In geology, the science to which he was peculiarly attached, his attention was early directed [...]tion as to the relative position of the land and sea which had [...] remote geological epochs; and the west of Scotland, the district [...] resided, offered admirable opportunities for his inquiries. In

the sandstone cliffs between Gourock and Largs, in the trap-dykes of Cumbrae, and, indeed, nearly all round the coasts of the Firth of Clyde, there was unmistakeable evidence of the slow and long-continued action of the sea; and the beds of fossil shells found at various elevations, and remote from the present coast, obviously demonstrated that a great part of Scotland had, at a comparatively late geological period, been covered by the sea. In pursuing his researches, Mr. Smith soon perceived the importance of carefully comparing the shells of the living species of mollusca in European and more northern seas with those found in the superficial deposits of Scotland now known as post-pliocene or glacial drift, and with the forms occurring in raised beaches and other later formations. These researches he conducted by dredging explorations from his yacht. In this investigation he discovered that a large proportion of the mollusks whose shells are found in these deposits, but do not now exist in the Clyde, are still to be found living in the arctic seas. This led him to the conclusion announced to the Geological Society in 1836, of the existence, before the present state of things, of a colder climate than the present—a conclusion which, though opposed to what geologists had previously believed, is now universally accepted. In continuing his investigations, Mr. Smith found reason to distinguish the deposits in question into two, in the older of which a considerable proportion of shells and other remains had belonged to animals now extinct in our latitudes, but in the later of which, belonging to a more recent period, every one of the shells has been found as species now existing, though some of them only in the arctic seas. This is the period of long repose of the land, at a depression of about 40 feet below the sea, which now has left the modern shores of the west of Scotland fringed, at an elevation of 40 feet, with that old sea-cliff which is so marked a feature in its scenery.

From 1839 to 1846 the health of members of his family caused Mr. Smith to reside successively at Madeira, Gibraltar, Lisbon, and Malta, and valuable geological papers on each of those localities attest the zeal with which he pursued his favourite science.

His residence at Malta was the occasion of the remarkable series of investigations by which he is best known in literature and theology. These were first published in 1848 in 'The Voyage and Shipwreck of St. Paul, with Dissertations on the Life and Writings of St. Luke, and the Ships and Navigation of the Ancients.'

The part of the volume which relates to the voyage and shipwreck of St. Paul has been accepted by all critics and theologians who have since written as conclusively settling all doubtful and contested questions as to the narrative. This work is a remarkable instance of originality, ingenuity, and sagacity, and of the application of practical knowledge of seamanship and geology to the elucidation of a point of literary and theological interest.

The minute study of the writings of St. Luke, to which Mr. Smith was

thus led, conducted him to a view on the much-contested question of the connexion of the first three Gospels, which was first published in the 'Dissertation on the Life and Writings of St. Luke,' to which we have referred, and which was afterwards worked out in greater detail in a separate 'Dissertation on the Origin and Connexion of the Gospels,' published in 1853. On this subject Mr. Smith's view, which differs from any previously taken, though not accepted to the same extent as his conclusions respecting the voyage of St. Paul, has had many followers. He was engaged in the collection of materials for a more extended dissertation on the same subject when interrupted by his last illness.

Mr. Smith was a member of many scientific societies—of the Royal Society, the Geological Society, and the Royal Geographical Society of London, and the Royal Society of Edinburgh. He was president of the Geological Society of Glasgow and of the Archæological Society, and was for many years president of the Andersonian University in that city, and was unwearied in his exertions for its benefit, and for the improvement of its valuable museum. The date of his election into the Royal Society is Dec. 23, 1830.

Mr. Smith enjoyed vigorous health till the spring of last year, when a slight stroke of paralysis enfeebled his body, without affecting his mind. A further attack towards the close of the year terminated in his death at Jordan Hill on the 17th of January, 1867.

Mr. Smith was married in 1809 to Mary Wilson, granddaughter of Dr. Alexander Wilson, Professor of Astronomy in the University of Glasgow. By her, who died in 1847, he had nine children, of whom three survive,—Archibald Smith, Esq., F.R.S., late Fellow of Trinity College, Cambridge, a member of the Chancery Bar, and two daughters.

Sir James South, born in October 1785, was the eldest son of an eminent Pharmaceutical Chemist resident in Southwark. Educated at a private school, he acquired a fair knowledge of Greek and Latin, and such elementary instruction in mathematics as was current in those days. On leaving school he commenced the study of surgery, which he chose as a profession; combining with it that of chemistry, in which he was no ordinary proficient. In due time he became a member of the College of Surgeons, and rose rapidly into extensive practice. Sir A. Cooper, whose dresser he had been for a time, believed that if he had persevered he would have been one of the most eminent surgeons of his time. But a different fate was before him. While yet a boy his curiosity had been excited by a singular erection on the roof of a house near his father's, and he contrived to get acquainted with its owner. This was Huddart, an engineer of distinguished talent; known, among other inventions, by machinery for making ropes in which every fibre bears an equal strain. The structure was a dome for containing the equatoreal which afterwards became famous by his and Sir John Herschel's observations of double stars. The ideas

which he got from his visits to Huddart were not lost; and as soon as he was able, he procured a 6-inch Gregorian with which he observed eclipses, occultations, and looked at double stars. Observing soon became a passion; and when, in 1816, his marriage with a lady who brought him a considerable fortune made him no longer dependent on his professional work, he resolved to devote himself to astronomy. He began by constructing at his house in Blackman Street a magnificent observatory. After the death of Huddart he purchased the equatoreal which had been his first inspirer; a fine transit instrument, which he described in the Philosophical Transactions for 1826, was soon added; and a circle was in progress. His zeal for astronomy and his personal character won the regard of such men as Davy, Wollaston, Kater, Babbage, Baily, the two Herschels, and, at a later period, Faraday and Lord Rosse; and his house was a centre of scientific reunion. He took an active part in the formation of the Astronomical Society; and, in conjunction with Baily, exposed the defects of the 'Nautical Almanac' so effectually, that after some years of discussion it was brought to its present excellence in accordance with the recommendations of a committee, of which he was the chairman. From 1821 (when he became a Fellow of the Royal Society) till 1824 he worked with Sir John Herschel in forming the Catalogue of 380 double stars which (with a description of the equatoreal) appears in the Transactions for 1825. For this they obtained conjointly the medal of the Institute, and till the publication of Struve's Dorpat Catalogue it was quite unrivalled. During its progress it became too evident that the smoky atmosphere of the Borough was ill suited for delicate observations; and he transported another equatoreal to Passy, near Paris, where he expected to find a purer sky. He resided there more than a year, enjoying the society of those "men of renown," of whom the chiefs were Arago, Humboldt, and, above all, Laplace, whose scepticism about the orbits of double stars he had the pleasure of removing by *showing* him the angular motion of 70 Ophiuchi, the components of which were then near their least distance. The fruits of this sojourn appear in the Transactions for 1826 as an additional catalogue of 458 double stars, for which, and his paper "On the Sun's Right Ascension," he was awarded the Copley medal of that year and the gold medal of the Astronomical Society. On his return to England he purchased a property at Kensington, where he was free from smoke, not foreseeing (though many people who ought to have known better supposed him to be an Astrologer) that he should live to see it in the heart of a dense population. There, of course, he built a magnificent observatory, the finest private one probably that ever existed. It was now increased by the purchase of Groombridge's meridian-circle, to which he had eight additional microscopes applied by its maker, and by a valuable clock, the gift of the King of Denmark. By this time the labours of Guinand on optical glass had made it possible to construct object-glasses of a size till then unattainable; and he determined to procure one of the highest power, and pursue with his whole energy the career on which he

had so successfully entered. For this work he was highly gifted; he had a keen eye, a steady hand, great power of bearing fatigue and want of sleep, prompt decision in catching a bisection, and boundless enthusiasm. Fraunhofer, to whom he applied in the first instance, declined giving him an object-glass without also supplying its equatoreal. It would have been a happy thing for him had he accepted this proposal; but he had no faith in any instrument maker but Troughton, whom he worshipped with all the intense devotion of his impulsive character. He looked elsewhere: Cauchoix had completed a 12-inch object-glass, about which, in 1829, he was chaffering with the French Government; South heard of it, went over to Paris, tried it, paid the optician the price which he demanded, and started with it next morning on his way to England, to the great disgust of Arago that it was lost to France. But it was a fatal acquisition. Troughton was of course charged with the construction of the equatoreal; and a dome for containing it was also commenced. Unhappily, both of them were planned without much respect for the elementary principles of engineering, the first especially; and though Mr. Babbage and others pointed out to him its utter weakness, he merely answered, "It is designed by Troughton." He had no knowledge of mechanical science, and had unlimited faith in Troughton's infallibility. As was foretold, the instrument was a failure; and though many attempts were made to correct the inherent vices of its framing, they were only partially successful. The result was a deadly quarrel between the two friends (which perhaps might have been healed but for the intervention of other personal animosities), and a litigation, which after a run of four years was decided in a way unsatisfactory to both parties, most so to South. The dome was also a failure; in general it required four or five men to move it; sometimes it stuck fast! The loss of full £8000 thus spent without any useful result was itself a misfortune; but it was as nothing in comparison of the evil influence which these transactions exerted on his character. The feeling that his confidence had been misplaced, that the friendship which had been his ruling passion, and which he still cherished, was changed into bitter hate, made him suspicious and irritable; and he lost that reliance on the truthfulness and honesty of others, without which life becomes a desert. By degrees this left him in an isolation from many old and true friends; and also, as a necessary consequence, deadened his love of science, which, like all other good things, is strengthened by sympathy. And he had no longer a definite pursuit; he could not resolve to adopt any practicable form of equatoreal, though Lord Rosse offered to design and even to make one for him; and during the years that were wasted, the harvest of double stars which he had hoped to reap was gathered by Sir J. Herschel, the two Struves, Bond, and others. This object-glass, while in his possession, made only one discovery, the sixth star in the trapezium of Orion; and remained useless till he gave it to the Dublin Observatory in 1862, when Lord Rosse was elected Chancellor of that University. In the hands of Dr. Brünnow

it will doubtless be well employed. South, however, still worked with his meridian instruments; among other things he observed a considerable number of stars with the Groombridge circle, and the results are probably valuable, as each observation was direct and reflected on the same night with twelve microscopes. He for some years carried on an elaborate series of experiments on clocks to ascertain their performance *in vacuo*, the air's resistance to pendulums, the influence of various modes of suspending them, and the effect of screens in their vicinity. These are, it is believed, all preserved, and may perhaps be published. Another object to which he devoted much research, was the disturbance which might occur to an observatory from the vicinity of a railroad; and an account of his observations at Watford respecting it, which appears in our Proceedings for 1863, was his last contribution to science. For some years before his death his hearing and sight were almost entirely lost, and this helpless condition was made more afflicting by much bodily suffering, from which he was released on October 19, 1867. He was knighted in 1831 by William IV., with whom he was a favorite. He was one of the original Visitors of Greenwich Observatory, and a Member of the Astronomical Society, the Linnean, the Royal Society of Edinburgh, Royal Irish Academy, and of several others.—T. R. R.

JOSEPH TOYNBEE, well known as an eminent Aural Surgeon, was distinguished for the remarkable industry with which he strove to give a scientific character to the branch of medical practice to which he had devoted himself. He was born at Heckington, in Lincolnshire, and died at his professional residence, in London, on the 7th of July, 1866, in the fifty-first year of his age.

After completing his education at Lynn, Mr. Toynbee was articled to Mr. Wm. Wade, of the Westminster General Dispensary, and afterwards became a pupil at St. George's Hospital. Having early exhibited a taste for anatomical pursuits, he was, whilst still young, appointed Assistant Curator in the Museum of the Royal College of Surgeons. Soon after this, his love of anatomical inquiry displayed itself in a microscopic investigation, the results of which were embodied in a paper read before the Royal Society, and published in the Philosophical Transactions for 1841, entitled "Researches tending to prove the Non-vascularity of certain Animal Tissues, and to demonstrate the peculiarly uniform mode of their Organization and Nutrition." This memoir, though, when viewed by the light of recent histology, of but limited scope, was a real contribution to science at the period of its publication.

In his subsequent labours, Mr. Toynbee turned his attention exclusively to the Anatomy, Physiology and Pathology of the Organs of Hearing. Thus he contributed to the Meetings of the Royal Society four papers, bearing the following titles:—1. "On the Structure of the Membrana Tympani of the Human Ear" (1850). 2. "On the Function of the Membrana Tympani, the Ossicles and Muscles of the Tympanum, and of the

Eustachian Tube in the Human Ear, with an Account of the Muscles of the Eustachian Tube, and their Action in different Classes of Animals" (1852). 3. "On the Muscles which open the Eustachian Tube" (1853). 4. "On the Mode in which Sonorous Vibrations are conducted from the Membrana Tympani to the Labyrinth in the Human Ear" (1859). The first of the above-named memoirs was published in the Philosophical Transactions for 1851; whilst, of the other three, records will be found in the Society's 'Proceedings.'

In investigating the pathology of the ear, Mr. Toynbee proceeded by collecting a very large number of specimens of the human temporal bones and their contained parts, both healthy and diseased. The morbid specimens, said to number nearly 1700, were described by him in numerous contributions to the Royal Medical and Chirurgical Society, to the Pathological Society, and to various journals. Besides this, he published an interesting 'Descriptive Catalogue of Preparations illustrative of Diseases of the Ear in his Museum,' and also a systematic work on 'The Diseases of the Ear; their Nature, Diagnosis and Treatment,' besides occasional "Lectures" on the same subject.

He was for some time Surgeon to the St. George's and St. James's Dispensary, and was engaged in the general practice of Surgery; but, whilst retaining the title of Consulting Surgeon to the above-named Institution, he soon devoted his time and energy to Aural Surgery alone, in which department of his profession he gradually acquired a wide-spread reputation, his practice as an Aurist ultimately becoming very large. He was Consulting Aural Surgeon to St. Mary's Hospital, and to the Asylum for the Deaf and Dumb.

Mr. Toynbee's premature and sudden death was caused by the inhalation of chloroform, on the possibility of introducing which powerful agent into the tympanic cavities through the Eustachian tubes he had long been experimenting. He was found dead on his sofa, in his consulting-room in Savile Row, with cotton-wool over his face, and a chloroform bottle, his open watch, and various memoranda of experiments lying near him. It is but just, and it may be useful, to remark, that, while he doubtless fell a sacrifice to his experimental zeal, the deplorable event is in all probability to be ascribed to the extreme imprudence of inhaling chloroform when alone and unattended.

The subject of this brief notice was a man of enlightened intellect, quick temperament, and energetic habits, of enlarged views, cultivated taste, and benevolent disposition. In connexion with the St. George's and St. James's Dispensary, he founded a Samaritan Fund to provide the sick poor with bread, soup, wine, flannels, and coals, and also the means of ventilating their apartments. He was not only an active Treasurer to the Medical Benevolent Fund, but was an anonymous donor to it of the munificent gift of £500. He advocated the establishment of local museums, libraries, and institutes, the better education of

the working classes, and the improvement of the dwellings of the poor; and, at one time, spent much time and money in furtherance of such objects and purposes. He was vigorous and lively in both his professional lectures and his more popular "lecturettes," as he himself preferred to call them. His neighbours and friends at Wimbledon, where he latterly resided, fully appreciated his excellent public and social qualities. The date of his election into the Royal Society is March 10, 1842.

Robert Warington was born on the 7th of September, 1807, at Sheerness, where his father, Thomas Warington, who was a victualler of ships, then resided. He was educated at Merchant Taylors' School, and, being intended for a land-surveyor, he was, on leaving school, set to learn that business, but, after a few months, abandoned it in favour of chemistry, which he studied as the house pupil, and subsequently, in 1822, the articled apprentice of Mr. J. T. Cooper, then a well-known lecturer and manufacturing chemist.

On the opening of the London University (now University College) in 1828, Mr. Warington, having served his time with Mr. Cooper, was chosen by the Professor of Chemistry, Dr. Edward Turner, to be his assistant. In that capacity he continued at the College for three years, during which period he communicated to the Philosophical Magazine his first published research, entitled "Examination of a Native Sulphuret of Bismuth."

In 1831, Messrs. Truman, Hanbury and Buxton, desiring the services of an able young chemist in their great brewing establishment, engaged Mr. Warington, on the recommendation of Dr. Turner, and with them he remained till 1839. His connexion with the brewery did not prevent his independent pursuit of chemistry, and during this period he contributed papers to the Philosophical Magazine, "On the Establishment of a System of Chemical Symbols" (Sept. 1832), and "On the Action of Chromic Acid upon Silver" (Dec. 1837).

On the death of Mr. Hennell in 1842, Mr. Warington was appointed Chemical Operator to the Society of Apothecaries, a position he continued to hold till about a year before his death. His professional engagements now became numerous, and he was much employed as a scientific witness or adviser in important cases coming before the Courts of Law.

Mr. Warington's scientific activity manifested itself in various ways. In 1841 he took an important part in the establishment of the Chemical Society, and became one of the original Secretaries, which post he held for ten years. He was one of the promoters of the Royal College of Chemistry; and he took part in the formation of the Cavendish Society, and held the office of Secretary for three years. He was Chemical Referee of four of the principal gas companies of the metropolis. He served as Juror in the Chemical Section of the International Exhibition of 1862, and was

appointed to a similar office in the Paris Exhibition of 1867, but was unable to discharge the duty.

Mr. Warington's scientific acquaintance with Pharmacy, and the large experience he had acquired in the practice of the art, led to his being employed in revising the translation of the London Pharmacopœia, left unfinished by Mr. Phillips, and in aiding in the construction of the Pharmacopœia of 1851. For a like reason he was consulted by the Committee appointed to prepare the British Pharmacopœia of 1864, and undertook a still more important share, along with Mr. Redwood, in the preparation of the British Pharmacopœia of 1867, although his failing health allowed him but partially to perform his task.

Amid these varied labours of his active and useful life, Mr. Warington continued to furnish numerous contributions on chemical and pharmaceutical subjects, to the Memoirs and Quarterly Journal of the Chemical Society, the Philosophical Magazine, and other periodical works. To the Transactions of the Microscopical Society, of which he was an efficient member, he contributed several papers, and he was the inventor of a portable microscope, which has been favourably spoken of.

A subject of more general interest, which furnished an agreeable and instructive study to Mr. Warington for many years, was the mode of life of aquatic animals and plants preserved in the aquarium; and especially the maintenance in a limited quantity of unrenewed water of the chemical conditions necessary to their existence, through the mutually compensating operations of animal and vegetable organisms upon the medium they inhabit. The results of his observations were published, from time to time, for the most part in the 'Annals of Natural History,' and also furnished the subject of a Lecture delivered by Mr. Warington at one of the Friday Evening Meetings of the Royal Institution. The latest yield of these long-continued researches which he lived to make known, forms the subject of a valuable and interesting paper "On some Alterations in the Composition of Carbonate-of-Lime Waters, depending on the Influence of Vegetation, Animal Life, and Season," communicated to the Royal Society within a month of his death, and published in the 'Proceedings' of December 1867.

In 1835 Mr. Warington married Miss Elizabeth Jackson, by whom he has left a family. He was elected a Fellow of the Royal Society in June 1864; he died at Budleigh Salterton, in the county of Devon, on the 12th of November 1867.

Mr. Warington was remarkable for his varied taste and constant activity as an observer; he may be said, indeed, to have passed from one subject to another with too great a facility, and consequently his completed investigations bear but a very small proportion to the number of subjects he had continually under examination. He was of an exceedingly cheerful and genial disposition, and a man of simple unaffected piety.

The Reverend William Whewell, D.D., late Master of Trinity College, Cambridge, and a Fellow of this Society, was born on the 24th of May, 1794. His career affords one more, and a very striking illustration in addition to those which the biographical annals of our country so abundantly present, of what may almost be regarded as the normal progress from an origin altogether devoid of external advantages, and in the humbler walks of life, to eminence and distinction as well as social position, wrought out by innate talent rendered effective by energy and persevering application, and sustained by high moral qualities. Beyond his immediate parents, little is known of his family. His father, a man of probity and intelligence, pursued the calling of a joiner or house-carpenter in Lancaster. His mother appears to have been a person not only of excellent principle and good sense, but of some considerable mental culture. Their family consisted, besides himself, of a brother who died at an early age, and three sisters. His own health in early youth was feeble, and afforded no prognostic of the robust frame and stalwart vigour which so strikingly characterized his manhood. On the other hand, from his earliest years he manifested a remarkable fondness for reading, and exhibited such general promise of future ability, as induced his parents to remove him from the Grammar School in Lancaster, where he received the first rudiments of instruction, to that of Heversham, where he might obtain the advantage of an Exhibition for admission to Trinity College, the Vicarage of Heversham being in the gift of that body. This he secured, and was in consequence admitted at that College as a sub-sizar in the October term of 1812, and was subsequently elected as a full, or foundation sizar, and obtained a scholarship. In 1816 he graduated as second Wrangler and Smith's Prizeman, the first honours being carried off by a competitor (Mr. Jacob of Caius College) by whom to have been surpassed could no way be considered as a defeat. His undergraduateship, meanwhile, had been distinguished by obtaining the Chancellor's prize in 1813 for the best English Poem on the subject of Boadicea—a spirited production, which may be read with pleasure as something beyond a college exercise, and evidencing that strong vein of poetical talent which showed itself on many subsequent occasions.

In the year following his graduation as B.A., he was elected a Fellow of his College with whose interests and glory he ever afterwards considered his own as identified, and was very soon engaged in lecturing in Mathematics as assistant tutor, and subsequently in 1823 as full tutor of one of the "sides" of that numerous establishment, the tutor's chair on the other "side" being filled by Dr. Peacock, afterwards Dean of Ely. This important office he filled during the ensuing sixteen years, being joined in the performance of its duties during the last six with the Rev. Charles Perry, afterwards Bishop of Melbourne. Soon after taking his Master of Arts' degree, he entered into Holy orders, and in

due course graduated as Doctor of Divinity. He accepted no College living, however, or special cure of souls—not from any want of appretiation of the importance of the ministerial office, or doubt of his own aptitude for its exercise, but from a conscientious persuasion that his true sphere of utility would be found in the entire devotion of his powers to the furtherance of the objects of the University as a place of education, and to the improvement of its system of instruction in those great branches of mental culture in which it was beginning to be felt at that epoch that such improvement was not only possible, but largely needed. Such prospects we have seen amply realized; but it should not be forgotten for the credit of that illustrious establishment, that the movement in advance then making originated within itself, and was in no way forced upon it by any pressure from without. Thenceforward, then, his career may be considered as identified with the great cause of University improvement, and in a larger and more expansive point of view with that of philosophical, moral, and religious *culture* in the widest and best acceptation of the words.

During the period when he was pursuing his studies at Cambridge, the mathematical department of the University *curriculum* was in what might be called a transitional state. A perception had begun to be entertained of the absolute necessity of including within its range a knowledge of those powerful methods of investigation so familiar to the Continental mathematicians, but which could hardly be said to be known in England, and which at Cambridge had by some even been regarded with dislike, as innovational. In this latter feeling, in common with most of its younger members, he was far from participating, but on the contrary was only desirous to forward the movement which he saw commencing.

About the period when he entered on his tutorial duties, a very general sense had come to be entertained of this necessity; but a great obstacle to the introduction of an improved course of mathematical reading existed in the absence of elementary works in our own language adapted for the purpose of university teaching, in which the principles of the analytical methods as applied to physical subjects were exhibited, and a yet greater in the utterly unphilosophical and inadequate mode of treatment in what were termed "the branches" current in the University. The primary difficulty had been removed by the translation by Peacock and his coadjutors of the treatise of Lacroix on the differential and integral calculus published in 1816, and followed by a copious collection of examples illustrative of its application to problems of pure mathematics and the theory of curves in 1820. But the want of *readable* elementary works in all the branches, and especially in that of dynamics, such as might, as it were, break the abruptness of the transition, and bridge over the interval between the modes of treatment of that subject in the 'Principia' of Newton and

in the modern analytical processes, was severely felt. Accordingly we find him in the period of his tutorship, from its very commencement, engaged in the production of a number of elementary treatises devoted to this object, and to conveying the primary principles of mechanical philosophy in a sound and logical form, as well as to affording an insight into the modern ways of handling them, such as his 'Elementary Treatise on Mechanics' (1819); his 'Treatise on Dynamics' (1823); his 'Introduction to Dynamics,' 'First Principles of Mechanics,' and 'Treatise on the free motion of a Point and on Universal Gravitation" (1832); his 'Elementary Treatise on Mechanics,' and 'Analytical Statics' (1833), and his 'Mechanical Euclid' (1837). Of these works, the first mentioned has been considered by one excellently qualified to judge of its merits as "a work of great value, and very far in advance of any then existing text-book, for the clearness and correctness of its treatment of bodies in contact, and in the precision with which the assumptions involved in the laws of motion and the composition of forces are stated, and illustrated." At the end of the last named (the Mechanical Euclid) is attached a section "on the Logic of Induction," in which the leading idea which forms the foundation of his great work on the Philosophy of the Inductive Sciences, published three years later, viz. that Induction consists in *superinducing* upon an assemblage of observed phenomena, a conception—the creation of the mind, which is not *in* the phenomena, but which serves to bind them under a common aspect, and so give them an ideal unity—is anticipatorily introduced.

By these works, and by the influence which, as moderator in the years 1820, 1828, and 1829, he was enabled to exercise on the course of the examinations for degrees, he contributed materially to that improvement he so much desired to see established in the mathematics of the University.

It would give but a very inadequate idea, however, of the extent of his reading and of his extraordinary intellectual progress at this period of his life, to measure them by these productions. A more wonderful variety and amount of knowledge in almost every department of human inquiry was perhaps never in the same interval of time accumulated by any man; embracing not only Mathematical and Physical Science in all its forms, but extending over Classical and Continental literature, Metaphysics and History, Ethics, Social and Political Economy, together with Botany, Architecture, Engineering, and a host of other subjects—and that not by merely a general and superficial acquaintance, but one which an exact and conscientious application such as most men devote to some favourite branch of study, alone can give. Nothing short of such a store of precise and varied information could have qualified him for the production of those great works on the History and on the Philosophy of the Inductive Sciences which have placed his name among the brightest in the annals of our Philosophy, and the former of which appeared in

the year 1837, during the continuance of his tutorship, and the latter about three years later. These works were preceded (in 1833) by his Bridgewater Treatise 'On Astronomy and general physics considered in reference to Natural Theology.' But that a great change in his views as to the origin of our fundamental axioms must have taken place between the production of these and the last-mentioned work, may be inferred from a remarkable passage in that Treatise*, in which he distinctly refers the origin of even the axioms of Mathematics to experience, *i. e.* to a slow process of inductive observation, growing with our growth, and not to any innate *à priori* intuition.

Dr. Whewell was one of the founders of the Cambridge University Philosophical Society, whose initiation dates from November 15, 1819, its first Meeting for the election of Officers being held on December 13 of that year, when Professor Farish was chosen President, and Dr. Whewell was placed on the list of its Council. In 1820 he contributed his first paper to its Transactions, "On the position of the Apsides in orbits of great excentricity." To the Transactions of this body he continued to contribute, up to within a short period of his death, papers on a great variety of subjects,—on Dynamics, on Mineralogy and Crystallography, on Logic and Philosophy (more especially on the Platonic philosophy of ideas), and on the mathematical exposition of the doctrines of Political Economy, in which the formulæ embodying the results of those doctrines, as applied to questions of supply and demand, price, currency, &c., are derived from what he cautiously terms "The Equilibrium Theory" in analogy to the "Equilibrium Theory" of the Tides, distinguishing very justly between this and their true or dynamical theory, which takes account of momentary changes in the amount and incidence of the acting causes, and allows for the time requisite to enable them to work out their effects—a distinction of the most important kind, and one which goes to exhibit all the *quantitative* conclusions deduced in this science on the other hypothesis as tentative and provisional.

During the summers of 1826 and 1828 he took part with Mr. Airy in a series of experiments for ascertaining the mean density of the earth, by comparing the rates of the same pendulum in deep mines and at the surface. These experiments, made in the Dolcoath Mine, near Cambourne, in Cornwall, were excellently planned, and, so far as they went, admirably conducted; but by a singular fatality were in both instances cut short in their progress, and frustrated of their result, by accidents which could not have been foreseen and provided against—in the one case by the combustion in mid-air (from some cause never explained) of the basket containing the pendulums and other apparatus, in the act of raising them from the bottom of the mine to the surface—in the other, by the mine itself becoming deluged with water, owing to the

* Bridgewater Treatise, p. 336, ch. ix. *et seq.*

sudden subsidence of a mass of rock "many times as large as Westminster Abbey." These failures, however, are the less to be regretted, as, had they not occurred, the third and successful attempt made by the Astronomer Royal in 1854, in the Harton Coal-pit, near South Shields, at a greater depth, and with the immense advantage of electric communication between the clock above and the pendulum below, might never have been undertaken.

In 1828 Dr. Whewell was chosen Professor of Mineralogy, as the successor of Professor Clarke, a position for which he had prepared himself by a residence in Germany, under the instructions of Professor Mohs. The subject, especially its crystallographical department, had previously, however, attracted much of his attention, as is evinced by his elaborate memoir "On a general method of calculating the Angles made by any planes of Crystals," communicated in 1824 to the Royal Society (of which so early as 1821 he had become a Fellow). Several papers on the same department of Mineralogical Science were also communicated by him to the Cambridge Society in 1822, 1827, and 1828. This appointment, however, he held only for four years, and resigned it in 1832, when he was succeeded by Professor William Hallows Miller.

In 1827 he became a Fellow of the Geological Society, of which, such was the general sense of his proficiency in that science, in 1838, he was elected to the office of President. To this Society he communicated (in 1847) a paper on the distribution of the Scandinavian boulders. In the Meetings of the British Association, too, he took a lively interest, and was President of that body in 1841. He was the originator, or one of the originators of that system of Reports on the present state and progress of the several branches of science, which have from time to time been so usefully and instructively published in their annual Proceedings. In 1831 we find him writing to a friend on this subject. "The advice I gave them (the managers of the York Meeting) was to this effect: that the meeting should select eminent persons in each department of science, and beg them to make, by the next annual meeting, reports as to the present condition of their respective provinces, and the points where research will apparently be most useful; that the purport of these reports and the degree of interest which they may excite should be the guide and basis of future operations of the Association, if it continue; and that, at any rate, such collection of reports, if it can be procured, be printed—by which means their Wittenagemote will not have met in vain." This idea being acted on, he himself contributed from time to time, Reports on the Tides and on the Mathematical theories of Heat, Magnetism, and Electricity.

The subject of the Tides engaged a large portion of his attention, and gave occasion for a series of researches on the progress of the Tide-wave in different regions of the ocean, communicated to the

Royal Society, and printed in their Transactions from 1833 to 1850. His labours on this branch of physical inquiry were distinguished in 1837 by the award of one of the Royal Medals. In point of general result, these investigations may be considered as having afforded a clear and satisfactory view of the Atlantic Tides, while those of the Pacific (to which only a single memoir—the 13th in order—is devoted) remain still, in many of their features, enigmatical, and perplexed with difficulties which can only receive their elucidation from a long series of discussion carried out on the same principles, and based upon far more extensive observational data than he then commanded, or than we yet possess. One of the most curious and unexpected results of these inquiries is, that there exist two points in the North Sea, one between Harwich and Amsterdam, the other near the entrance to the Baltic, in which there is no rise and fall of the tides. Of these points, thus first theoretically indicated, the former has been subsequently verified by observation; the other does not appear to have been sought for.

With exception of this series of researches, his labours as a direct contributor to Physical Science may be considered as having terminated with his acceptance of the professorial chair of Moral Philosophy (or, as it is officially designated, of Moral Theology and Casuistry) in 1838. The work of Paley on Moral Philosophy, in which the basis of moral obligation is made to rest on expediency (taken in its largest sense, as that which on the whole, and on a broad and general view of human relations, is most conducive to human happiness), was at that time the text-book followed in the University. This view of the foundation of morals was, however, peculiarly distasteful to him, and the whole tenor of his teaching on this subject was devoted to the expulsion of what has been termed the utilitarian theory of morals, and the substitution for it of the inward teaching of a divinely implanted conscience, enlightened and guided by reason—obeying in this, as in his views of Physical Science, that strong leaning towards the Platonic or ideal system of philosophy which refers all our knowledge, in so far as it assumes a regular and systematic form (as other than the recollection of individual facts), to innate and primarily implanted conceptions coordinated with facts by the operation of the mind. Thus, as in geometry we coordinate our perceptions of the external world in accordance with our innate conception of Space—so, in this view of morality we coordinate our judgment of human action, and of our own emotions and desires in reference to the innate and originally implanted idea of Right; assuming that its fundamental axioms and leading propositions have found their utterance (though hitherto imperfectly, and only in the most usual and simplest cases of their application) in those positive laws which regulate the conduct of man towards his fellows in all civilized communities. These views are embodied in his sermons on the Foundation of Morals, his

'Elements of Morality,' his 'Lectures on the History of Moral Philosophy in England,' and more especially his 'Lectures on Systematic Morality' (1846). The two views are no doubt ultimately reconcileable, or rather essentially those of one body of truth approached from different ends of the vista: since if it could be shown that any *legitimate* conclusion as to moral duty drawn from the *à priori* system were incompatible with the production of general happiness and wellbeing, such conclusion could not but be deemed an insuperable objection to its truth; and since the ultimate reference to the enlightenment and guidance of reason (as distinct from innate or inspired intuition)—if it have any meaning at all—can only mean a reference to what experience teaches the general reason of mankind to expect as the probable result of any proposed course of action on the general happiness. But the difficulty is far greater to ascend to the general axioms of morality from the facts of history and social life in the way of induction, in the face of so much acknowledged confusion in the results of human action in the more complicated affairs of life, than to start from an *à priori* and divinely inspired principle of right, interfered with in its application by the disturbing agency of passion and ignorance, in rendering an account of so much evil intermixed with so much good. Nor does this consideration appear to have been altogether without its weight in his choice of a starting-point, if we may judge from a passage in the work last cited (Systematic Morality, p. 133. Ch. 29 *et seq.*). The just celebrity of these works, and of his philosophical treatises, with his other eminent claims to scientific distinction, procured him the honour of nomination by the French Academy as a correspondent in the department of "Sciences Morales et Politiques—Section de Philosophie."

In 1839 he retired from the tutorship of his college, devoting himself thenceforwards entirely to those pursuits which he felt to be more congenial to the natural bent of his genius and to his personal habits, than the practical routine of education. What, and how expansive, and at the same time how definite and sober were his views on the subject of the higher education in general, and of that which ought to be the more especial object of a University education, may be gathered from a series of essays and treatises published from 1835 to 1845 on this subject, one of which, a brief essay entitled 'Thoughts on the Study of Mathematics as a part of a liberal education' (1835), involved him in a controversy with a very formidable antagonist (if we are not mistaken, the late Sir Wm. Hamilton), who, in an elaborate article in the Edinburgh Review (No. 126), laboured to show that so far from being an essential and important means of cultivating "the noblest faculties in the highest degree," such studies effect this purpose "at best in the most inadequate and precarious manner," and that, in point of fact, they "have less claim to encouragement than any other object of education." Dr. Whewell, however, was quite as

strenuous an advocate for the importance of classical as of mathematical studies as part of a course of liberal education, as well as for admitting, if not insisting on, the study of natural history and other branches of Natural Science as a means of developing the intellectual faculties, and of jurisprudence, not only for its intimate connexion with sytematized morality, but as one of the best exercises in, and exemplifications of, applied logic.

In May 1841 he published his 'Mechanics of Engineering,' a most useful and much-needed elementary treatise on the practical application of mechanical principles to questions of construction and machinery. In the summer of that year he married Cordelia, the second daughter of John Marshall, Esq., M.P., of Hallsteads and Patterdale Hall, Cumberland, and in October of the same year succeeded Dr. Wordsworth in the Mastership of Trinity College, which he retained during the remainder of his life. In this distinguished and important office (to use the words of one excellently qualified to judge), though "there were some who feared that the new Master would be imperious and overbearing, their fears were dissipated by the result. His government was, with scarcely an exception, the government of a constitutional monarch, not of a despot. Of his rights and privileges he was tenacious enough, but he preferred to delegate the active exercise of his power, and its consequent responsibility, to the several college officers, and was best pleased when all went smoothly without reference to him. He did not interpose *nisi dignus vindice nodus*. The Lodge was the scene of generous hospitality, and received a constant succession of distinguished guests." His wife, a most amiable and excellent lady, suffered for several years from a painful illness, during which his care and attention were unremitting though unavailing; and in December 1855 he was left a widower. From the deep grief and distaste for his usual philosophical pursuits, and from the melancholy associations of his college residence, he sought at length relief in a visit to Rome (resigning his Professorship of Casuistry), where, however, with characteristic eagerness for the continual accumulation of mental acquirement, he devoted much of his time to perfecting his knowledge of the Italian language, "taking lessons and writing exercises like the veriest schoolboy." Returning, and resuming his college duties, in 1858 he married Lady Affleck, widow of Sir Gilbert Affleck, a lady whose sweet disposition and engaging manners rendered her universally beloved, and contributed not a little to the increasing popularity with which he at length came to be regarded in the University,—the inherent dignity and loftiness of his character, and the splendour of his reputation, now universally recognized as adding lustre both to his college and to the University, overcoming somewhat of a contrary feeling which, in the earlier part of his career, had prevailed as the result of a certain uncompromising stiffness of demeanour and impatience of opposition.

In 1858 appeared his 'Novum Organum Renovatum,' and in 1860 his 'Chapters Critical and Historical on the Philosophy of Discovery,' being in part a reproduction of particular portions of his 'Philosophy of Inductive Science,' of several essays on Plato, Aristotle, &c., communicated to the Cambridge Philosophical Society, and of a series of 'Remarks on Induction with reference to Mr. Mill's Logic' (1849)—in part comprising several new and very striking chapters on the theological views suggested by physical discovery.

His devotion to the Platonic view of the ideal world seemed to grow with his growth and strengthen with his strength, and may be said to have culminated in his Platonic Dialogues, which appeared in 1860, 1861, and 1862, being in effect a translation of the most important portions of Plato's dialogues, accompanied with a kind of running comment, explanatory of such portions as might be judged irrelevant or tedious if translated at length—a work which he declares to have been "not lightly executed, but the labour of many years; each part gone over again and again."

In 1861 he was called upon by His Royal Highness the Prince Consort to deliver, for the express instruction of the young Prince of Wales (then a student of Cambridge), a short course of lectures on Political Economy. This, as we have seen, was a subject which had engrossed a large share of his attention at an earlier period, and which he never relinquished, being an ardent admirer of his distinguished contemporary and dear friend Professor Jones (whose posthumous works he edited), and whose volume on Rent he considered as the only work in which that subject is treated in its most general aspect, and on truly correct principles. Accordingly we find in these lectures, which he subsequently published, frequent reference to his views, and copious extracts from the work itself.

In 1865 he again became a widower. Stern and somewhat hard as he may have appeared to those who knew him but imperfectly, no man was ever more susceptible of the gentle and tender influences of female society, or had a deeper sentiment of domestic affection, and this last blow for a time completely overcame him. By very slow degrees, and cheered by the society of an attached relative of his former wife, he so far recovered as to be able to resume his philosophical pursuits, and to compose a short but highly interesting article on "Comte and Positivism," which appeared in MacMillan's Magazine. It was his last production. On the 24th February 1866, while riding a horse which had twice before thrown him (for though a bold, and even a reckless rider, he was by no means a first-rate horseman, riding negligently, and, in the present instance, probably greatly enfeebled, and perhaps affected by vertigo), he was observed by the ladies in a carriage, beside which he rode, to have lost the command of his horse, and to be partially unseated. Shortly after the horse appeared without his rider, and he

was taken up senseless, having received a concussion of the brain, unaccompanied however by any fracture. For a time hopes were entertained of his recovery, but they proved delusive. The fatal termination took place March 6, 1866; and on the 10th his remains were deposited, with every possible honour, and with an immense attendance on the part of the University, in the Chapel of Trinity College, at the feet of the statues of Newton and Bacon (the latter his own gift to the college). He left no family, and as he had throughout life identified himself with the University and the College in which he had won fame and acquired wealth, so, at his death, he devoted the bulk of the latter to increasing their efficiency. The particular light in which he regarded systematic morality had led him to a careful study of the principles of jurisprudence and of the law of nations, during the course of which he had been led to publish a translation of the great work of Grotius, 'De Jure Belli et Pacis;' and justly considering that international law as a branch of the higher education was far too much neglected in this country, he provided by his will for the liberal endowment of a professorship and studentships of that science in the University; while, for the future enlargement of his college, he left a large and valuable area of adjacent ground, purchased for the purpose during his lifetime, together with ample funds for building on the site.

To the worth and nobleness of his personal character it is scarcely possible to do justice within the brief compass of a notice like the present. Those who would appreciate it fitly will find it admirably delineated, and with a truth and fidelity which leave nothing to desire, in the biographical notice from which one passage has been already quoted above*. Of his works other than scientific, a brief mention will suffice. His 'Architectural Notes on the German Churches,' and 'Notes written during an Architectural Tour in Picardy and Normandy,' have been pronounced standard works on ecclesiastical architecture. The enumeration of the long list of churches visited, and noted by him according to a regular and systematic plan of annotation, in the course of a summer excursion, in the former of these works, will serve to give some idea of the surprising activity and energy with which he threw himself into everything he undertook. In 1847 he edited a collection of English hexameters and elegiacs by various authors, himself contributing a translation from the German of Goethe's Herman and Dorothea. Of the admission of these metres into our English verse he was always a strenuous advocate, justly apprehending their many and powerful claims to such reception, and turning a deaf ear to the prejudice which would refuse them their merited place in our literature. He translated also Auerbach's 'Professor's Wife.'

The essay on the 'Plurality of Worlds' (attributed to him, though

* William Whewell. In Memoriam. By G. W. Clark, M.A., Public Orator in the University of Cambridge. MacMillan's Magazine, No. 78.

published anonymously), can hardly be regarded as expressing his deliberate opinion, and should rather be considered in the light of a *jeu d'esprit*, or, possibly, as has been suggested, as a lighter composition, on the principle of "*audi alteram partem*," undertaken to divert his thoughts in a time of deep distress. Though it may have had the effect I have heard attributed to it, of "preventing a doctrine from crystallizing into a dogma," the argument it advances will hardly be allowed decisive preponderance against the general impression which the great facts of astronomy tend so naturally to produce.—J. F. W. H.

Nicholas Wood was born at Sourmires, a village near Newcastle-on-Tyne, April 24th, 1795. While at school, in the same neighbourhood, he attracted the notice of Sir Thomas Liddell, and through his influence was placed at Killingworth Colliery, to learn the practice of a coal-viewer. Here he made acquaintance with George Stephenson, who was beginning to develope that skill and ingenuity which afterwards made him famous. The two young men became intimate, and both worked energetically in carrying out the plans of the inventor of the future locomotive. Wood made the drawing of the first safety-lamp, and was fearless enough to accompany Stephenson in a trial of the lamp first constructed therefrom, at a "blower" in the mine. Taking the time and circumstances into account, this experiment must be recognized as a manifestation of a high degree of moral as well as physical courage on the part of the two operators. Some of Wood's earliest scientific papers, published in local journals about the year 1815, were on the use of the lamp in mines and on points in the controversy which its use had originated. In the fact that Stephenson afterwards placed his son Robert under Wood as a pupil, we have a further proof of the confidence existing between them.

In 1825, the year of the opening of the Stockton and Darlington railway, Mr. Wood published a 'Treatise on Railroads,' which, embodying what was then known of the principles and practice of railway construction, has passed through three editions, and is still regarded as a standard work, notwithstanding that in the first edition the author treats as "ridiculous expectations" the views of those who were sanguine enough to believe that locomotives could be made to run twenty miles an hour.

In 1827 Mr. Wood gave evidence before the Parliamentary Committee on the proposed railway from Liverpool to Manchester. His opinion was highly valued, especially on subjects connected with coal-mining, so that in parliamentary inquiries relating to such subjects he was usually summoned as a witness. With a growing reputation as geologist and mining-engineer, he communicated, in 1830 and 1831, to the Natural History Society of Northumberland, Durham, and Newcastle-on-Tyne, two papers, which were published in their 'Transactions' as valuable contributions to the then existing knowledge of the geology and palæontology of the district. The subject was extended and further elucidated in two papers read at the

Meetings of the British Association at Newcastle in 1838 and 1863. In these papers the limits of the several formations—the millstone grit, the mountain limestone, and true coalfields—are defined, the courses of "dykes" are laid down, their effect on the coal-measures is pointed out, and in the numerous sections a clear view is given of the structure of the country. In the paper on "The Wash," a remarkable denudation through a portion of the coalfield of Durham, written jointly with Mr. Boyd (1863), the effect of a great stream in remote epochs, in producing the present geological configuration, is ably traced.

The demands on Mr. Wood's time and practical skill multiplied with the increasing demand for coal for lighting and manufacturing purposes. In 1844 he proposed a registration of mining operations as important alike from the geological as from the commercial point of view. He was a member of the local Committee afterwards formed to draw up a report on the best method of preventing or diminishing accidents in coal-mines, out of which resulted the Mines' Inspection Act of 1851. In the following year, with a view to give effect to practical suggestions, a scheme for a society was proposed which in August of that year took shape as the North of England Institute of Mining Engineers, with Mr. Wood as President. From that time to the end of his days he was annually elected to the same honourable post, and discharged the duties thereof with advantage, during a period in which, besides his inaugural address, he contributed fourteen papers on important professional subjects to the Society's 'Transactions.' In his address, as well as on other occasions, he reiterated his appeal for the formation of a collection of plans and records as a means of promoting mining and geological science. These papers, with one on "Improvements in the Workingof Coal-Mines," communicated to the Society of Mechanical Engineers, and published in their 'Proceedings,' were the latest of his writings.

In 1844 Mr. Wood removed from Killingworth to Hetton Hall, co. Durham, where, while health permitted, he continued his active labours particularly in the attempt to establish a school or College "for the cultivation, improvement, and teaching of mining science, especially as applicable to coal-mines." The attempt failed, but the discussion had a beneficial effect in attracting public attention, and in impressing the Government with its importance.

Mr. Wood was a member of the local societies in his neighbourhood, of the Geological Society, of the Institution of Civil Engineers, and in 1864 was elected a Fellow of the Royal Society, but did not live long to enjoy the honour. He had been for some years a widower, when failing health compelled him to give up the duties which he had so beneficially discharged, and to resort for medical advice to London, where he died December 19, 1865, leaving four sons and two daughters. He was characterized throughout life by a calmness of temper which nothing disturbed; and one who knew him well has recorded concerning him, that, "whether he be viewed

as an employer of labour, as a successful man of business, as a promoter of knowledge, as a friend to education among all classes, as a neighbour, a parent, or a friend, we may safely say that his place in society will not easily be filled."

John, Lord Wrottesley, was born on the 5th of August 1798. His father, the first peer, was the representative of a family which was of distinction when it acquired the estate of Wrottesley, near Wolverhampton, at a date which may be best remembered by the fact that the fourth possessor, and the third who took his name from it, was made a Knight of the Garter at the institution of the order. The subject of our memoir graduated at Oxford with a first class in mathematics, and was called to the bar, at which he practised for several years as an equity lawyer. His tastes were scientific. He showed them when he joined the Society for the Diffusion of Useful Knowledge, in the Committee of which he worked during the whole existence of the body: in its earliest days he wrote a number of the Useful Knowledge Library, on navigation. In 1831 he became one of the Secretaries of the Astronomical Society, of which he was afterwards President (1841–43). He became President of the Royal Society in 1854, on the retirement of Lord Rosse, and held this post until 1858. He died Oct. 27, 1867, aged sixty-nine. No life was more devoid of striking incidents than his: the only exceptions to its even tenour were the loss of two worthy sons, one of whom fell in action at the Cape, the other at Bomarsund.

His characteristics, says a journal obituary, were plain manners, kind feelings, sound judgment, and useful intellect. His knowledge of law, his pursuit of science, and his conduct of life were equally practical and equally unobtrusive. His particular pursuit was astronomy, which he carried on in two small observatories, one at Blackheath, while he was engaged at the bar, the other at Wrottesley, after his accession to the title in 1841. In 1839 he received the gold medal of the Astronomical Society for a catalogue of stars. This work was performed by himself with the aid of Mr. Hartnup, now at the head of the Liverpool Observatory, whom he trained as his assistant. The object of it was to make systematic observations of the right ascensions of all the stars of the Astronomical Society's Catalogue, of the sixth and seventh magnitudes; the higher magnitudes having been undertaken, or rather having been supposed to have been undertaken, by other observatories. Comparison with various cases obtained from public observatories showed that Mr. Wrottesley's catalogue was, as it was styled by Mr. Baily in delivering the medal, of first-rate importance and entitled to implicit confidence.

Lord Wrottesley also communicated two astronomical papers to the Royal Society—one, "On the Results of Periodical Observations of the Positions and Distances of certain Double Stars," published in the Philosophical Transactions for 1851; the other, which was published in the

"Proceedings" for 1859, is entitled "On the Application of the Calculus of Probabilities to the results of Measures of the Position and Distance of Double Stars."

During the four years of his presidency of the Royal Society, Lord Wrottesley was unremitting in his attendance at the meetings of Council, and his conduct of business was guided throughout by the clearness of insight and rectitude of judgment which belonged to his mental character. Among the various subjects which came under his consideration during this period, there was one in which he may be said to have taken a special interest; the question, namely, whether any measures could be adopted by the Government or by Parliament which would improve the position of science and its cultivators in this country. The subject was brought under the consideration of the Parliamentary Committee of the British Association, of which Lord Wrottesley was Chairman; and the Committee, after corresponding with eminent men of science and learning their views on the question, presented a Report to the Meeting of the Association at Glasgow in 1855. In the following summer a motion was made in the House of Commons, by Mr. James Heywood, for a Select Committee to inquire into this question, which was, after some discussion, withdrawn, in order, as it was understood, to allow the matter to be previously considered by scientific men. On this, Lord Wrottesley, without loss of time, brought the matter before the Council of the Royal Society, who referred it in the first instance to the Committee who assisted them in the distribution of the Government grant, and finally adopted a series of resolutions which were communicated to Lord Palmerston, then First Lord of the Treasury: but a change of Government and a dissolution of Parliament having soon afterwards taken place, the matter was not again brought before the Legislature. Both as Chairman of the Parliamentary Committee of the British Association and as President of the Royal Society, Lord Wrottesley conducted the correspondence relating to this question, and took the chief share in drawing up the Reports.

Lord Wrottesley was President of the British Association at its Meeting in Oxford in 1860, and in his Address delivered on that occasion, he earnestly recommended due encouragement of the Physical and Natural Sciences as branches of University study. On a later occasion, in a speech delivered in the House of Lords in 1865, on the Public Schools Bill, he strongly urged the expediency of introducing science as an important branch of school teaching.

Alexander Dallas Bache, For. Memb. R.S., was a son of Richar Bache, one of the several children of the only daughter of Dr. Benjamin Franklin. His mother was Sophia Dallas, the daughter of Alexander J. Dallas, and sister of George M. Dallas, whose names are well known in the history of the United States, the former as Secretary of the Treasury, and the latter as Vice-President of the United States, and subsequently as

Minister at the Court of St. James's. He was born in Philadelphia, on the 19th of July, 1806. At an early age he became a pupil of a classical school of celebrity, and was distinguished by unusual aptitude in the acquisition of learning. Shortly before arriving at the age of fifteen, he was appointed a cadet of the Military Academy at West Point, and graduated in 1825 at the head of his class.

After graduating, he was selected to remain in the Academy as an Assistant Professor, and in this position, which gave him an opportunity of reviewing his studies and examining his reading, he continued one year, when at his own request he was assigned to engineering duty at Newport, R.I. Here he remained two years, engaged in constructing fortifications, and devoting his extra hours to the study of Physics and Chemistry; and supported his mother and her younger children out of his stinted pay as Lieutenant of Engineers. An unexpected change now took place in his circumstances, which enabled him to marry. He was appointed to the Professorship of Natural Philosophy and Chemistry in the University of Pennsylvania, at Philadelphia. Having had, prior to this, some experience as a teacher, he soon gained the entire confidence of the authorities of the University and the affections of his pupils. He became a member of the Franklin Institute, a Society then newly established for the promotion of the Mechanical Arts. This Society was well calculated to exhibit his talents and develope his character. It brought him into intimate association with the principal manufacturers, engineers, and artisans of the city. Facilities were thus afforded him for the prosecution of science which he could not have commanded in any other way. Workshops were thrown open to him, and skilful hands offered ready assistance in realizing the conceptions of his suggestive mind. His descent from the illustrious Statesman and Philosopher, whose name the Institution bears, contributed in some degree no doubt to the influence which he acquired; but it was in the main to his own industry, ability, and courtesy that he owed the favour and distinction which, in assigning to him the principal directorship of scientific investigations, afforded him the means of so greatly contributing to the usefulness of the Society, and of advancing his own reputation. An account of the labours in which he was engaged in his connexion with the Franklin Institute will be found in the volumes of its Journal from 1826 to 1835 inclusive. The results of his investigations relating to the bursting of steam-boilers, after a lapse of more than thirty years, have not yet been superseded by any others of more practical value. These experiments were attended with no small amount of danger, and required in their execution no small amount of personal courage.

He erected an Observatory in the yard of his own house, in which, with the aid of his wife and his friend and former pupil, John Fraser, he determined with accuracy, for the first time in the United States, the periods of the daily variations of the magnetic needle, and in another series of observations the connexion of the fitful variations of the direction of the

magnetic force with the appearance of the Aurora Borealis. He also, in connexion with Mr. Espy, made a minute survey of the relative change of position of the trees and other objects in the track of a tornado which passed over New Brunswick. In connexion with Professor Courtenay he also made a series of determinations of the magnetic dip at various places in the United States. Indeed, terrestrial magnetism was with him a favourite subject, to which he continued to make valuable contributions at intervals during his whole life. He was also much interested in the phenomena of heat, and was the first to show, contrary to generally received opinion, that the radiation and consequent absorption of dark heat is not affected by colour.

In 1836, when Professor Bache had just attained the thirtieth year of his age, the Trustees of the Girard College, an institution munificently endowed by a benevolent citizen of Philadelphia, preparatory to organizing the College, resolved to select Professor Bache as the most proper man for the office of President, and to send him abroad to study the systems of education and methods of instruction and discipline adopted in Europe. It was with difficulty that he could bring himself to regard with favour a proposition which threatened to separate him from the pursuit of science. The consideration of a more extended field of usefulness at length prevailed, and he accepted the proffered position, though not without some lingering regret. No American ever visited Europe under more favourable circumstances for becoming intimately acquainted with its scientific and literary institutions. His published researches had made him favourably known to the cultivators of science, and gave him ready access to intelligent and influential society. He remained in Europe two years, and on his return embodied the results of his researches on Education in his Report to the Trustees of Girard College. This Report is an almost exhaustive exposition of the systems of education and methods of instruction in use at the time in England, France, Prussia, Austria, Switzerland, and Italy. The accounts which are given of the different schools of Europe are founded on personal inspection, the results being noted down at the time with his habitual regard for accuracy.

After completing his report he was prepared to commence the organization of the College, but the Trustees, partly on account of the unfinished condition of the building, and partly from a want of the final adjustment of the funds, were not disposed to put the Institution into operation. In the meantime, Professor Bache, desirous of rendering the information he had acquired of immediate practical use offered his services gratuitously to the municipal authorities of Philadelphia to organize on an improved basis a system of public education for that city. This offer was readily accepted; and he commenced the work with his usual energy, and with the cordial support of the Directors and Teachers of the common schools. The result of his labours was the establishment of the best system of combined free education which had at that time been adopted in the United States.

In 1842, having completed the organization of the schools, and finding that the Trustees of the College were still unprepared to open the Institution, he resigned all connexion with it, and yielded to the solicitations of the Trustees of the University to return to his former chair of Natural Philosophy and Chemistry. During his travels in Europe he provided himself with portable instruments, and made a series of observations on the dip and intensity of terrestrial magnetism at prominent points on the Continent and in Great Britain, with the view of ascertaining the relative intensity of the magnetic force in Europe and America. The observations also served in most instances to settle with greater precision the magnetic condition of the points at which they were made. In the midst of the labour of organizing the schools of Philadelpha he cooperated actively with the British Association in determining the fluctuations of the magnetie and meteorological elements of the globe by contemporaneous observations at places widely separated from each other. He established an Observatory, which was furnished with a complete series of the best instruments by the Girard College, and was supported by the American Philosophical Society, and a number of liberal and intelligent individuals. The observations, which were continued at short intervals day and night for five years, form a rich mine from which, until within the last few years of his life, he drew a highly interesting series of results without exhausting the material. In addition to these observations, he made during his summer vacations a magnetic survey of Pennsylvania.

In November 1843, on the occasion of the death of Mr. Hassler, he was called to take charge of the United States Coast Survey. Though he undertook the task with many misgivings, it may be truly said that no other living man was so well qualified to secure the results which the nation and its commercial interests demanded. His education at West Point, his skill in original investigations, his thorough familiarity with applied science, his knowledge of the world and his gentlemanly deportment were all essential elements in the successful prosecution of the Survey. Besides these qualifications he possessed rare executive ability, and governed and guided the diverse elements of the vast undertaking with consummate tact and skill. Quick to perceive and acknowledge merit in others, he rapidly gathered around him a corps of men eminently well qualified for the execution of the tasks to which he severally assigned them. Up to the time of the appointment of Professor Bache little more than a beginning of the Survey had been made. It extended only as far from New York harbour as Point Judith on the east coast, and southward to Cape Henlopen. The new Superintendent saw the necessity of greatly enlarging the plan so as to embrace a much broader field than it had previously included. He divided the whole coast-line into sections, and instituted under separate parties the essential operations of the Survey simultaneously in each. He commenced the exploration of the Gulf-stream, and at the same time projected a series

of observations on the tides, also of the magnetism of the earth, and the direction of the wind at different seasons of the year. He also instituted a succession of researches in regard to the bottom of the ocean within soundings, and the forms of animal life which are found there, thus offering new and unexpected indications to the navigator. He pressed the electric telegraph into the service for the determination of the longitude, photography for the ready reproduction of charts, and the art of electrotyping for multiplying copperplate engraving. Professor Bache, with his enlightened appreciation of the value of abstract science, kept constantly in view the various problems relative to the physics of the globe which were incidentally connected with the survey of the coast, and ever cherished the hope of being permitted to finish his labours by their solution. Among these was a new determination of the magnitude and form of the earth, the variations in the intensity of gravity at various points on the continent of North America, the discussion of the general theory of the tides, the magnetic condition of the continent, and the improvement of the general map of the United States by determining in relation to the coast-line, the geographical positions of the most important points in the interior.

He was Superintendent of Weights and Measures, and rendered important services as a Member of a Commission to examine the condition of the Light House System. In 1846 he was named one of the Regents of the Smithsonian Institution, and by successive reelection was continued in this office until his death. At the request of the Governor of Pennsylvania, although overwhelmed with other public labours, he planned a line of defences for Philadelphia, and to a considerable extent personally superintended their construction. This work proved too much for his strength, and brought on the malady which terminated his life.

After some premonitory symptoms, which, however, did not diminish his exertions, he was suddenly deprived in a considerable degree of the power of locomotion, and of the expression of his ideas. For several months he was very anxious about the business of the Coast Survey, and it was with difficulty that he could be restrained from resuming the full duties of his office. It was hoped that a voyage to Europe would be of service to him; the journey, however, was productive of no permanent advantage; and after lingering for more than two years, he departed this life on the 17th of February 1867.

But few Americans have been more highly honoured at home or more appreciated abroad. He was President of the American Philosophical Society, of the American Association for the Advancement of Science, and of the National Academy of Sciences lately established by Congress. He was a Foreign Member of the Royal Society of Edinburgh, the Royal Irish Academy, the Royal Astronomical Society, a Corresponding Member of the Imperial Institute of France, of the Imperial Academy of St. Petersburg, of the Royal Academy of Turin, of the Institute of

Bologna, of the Royal and Imperial Geographical Society of Vienna, and the Mathematical Society of Hamburg. He was elected a Foreign Member of the Royal Society in 1860.

The preceding notice has been extracted from a manuscript memoir of Professor Bache kindly supplied by Dr. Joseph Henry.

Georg Friedrich Bernhard Riemann was born on the 17th of September 1826 at the village of Breselenz, near Dannenberg, in Hanover. He was the second of six children born to the Pastor of Breselenz. Under his father's sole tuition till eight years of age, he exhibited great powers of arithmetical calculation. An able tutor, who from this time assisted in teaching him, was forced to make unusual exertions in order to follow the short and original solutions of the problems proposed to his pupil.

In the spring of 1840 Riemann was sent to the Lyceum in Hanoaer, where he remained two years. He was then placed in the Gymnasium of Lüneburg under Director Schmalfuss. The latter soon discovered Riemann's mathematical talent, and not only gave him problems made expressly for him during school hours, but lent him works on the higher mathematical subjects, which he brought back after having thoroughly mastered them in the course of a few days. A week sufficed to make Legendre's theory of numbers his own for life.

He entered the University of Göttingen at Easter 1846, by his father's wish, as a student of theology. Here the lectures of Gauss stirred up in him such a passion for exact science that he sought and obtained permission from his father to devote himself entirely to the studies of his choice. For two years, commencing with Easter 1847, he studied under Jacobi at Berlin. He then returned to Göttingen, and graduated, his dissertation on the foundations of a general theory of functions of a variable complex magnitude obtaining the warm approval of Gauss.

In 1854 he qualified for the post of a teacher by a lecture on the hypotheses on which geometry is founded, and by writing a memoir on the representation of a function by a trigonometric series. In September of the same year he wrote on the distribution of electricity in non-conductors. In 1855 he contributed to Poggendorff's 'Annalen' a paper on the theory of Nobili's coloured rings, and one on the mathematical theory of the galvanic current. During the two following years he suffered much from failing health.

In 1857 he became Professor Extraordinarius, and wrote four papers which appeared in vol. liv. of Crelle's Journal. In 1859 he was elected a Corresponding Member of the Academy of Sciences of Berlin, and contributed to the 'Abhandlungen' of the Academy a memoir on the number of primes below a given number, and was nominated Professor Ordinarius. In 1860 he was elected a member of the Academy of Sciences of Göttingen, and in the course of this and the following year wrote a memoir on the propagation of plane waves of finite amplitude in air, and one

on the motion of a fluid homogeneous ellipsoid. These two memoirs were printed in the eighth and ninth volumes of the 'Abhandlungen' of the Society.

Riemann married in 1862. In July of the same year he suffered from an attack of pleurisy. Through the good offices of Professors Weber and v. Waltershausen he obtained leave of absence and pecuniary assistance from a fund available for such purposes, to enable him to travel in Italy. He quitted Göttingen in November and passed the winter in Messina. His health in some degree restored, he left Messina in March 1863, on his homeward journey, stopping in Palermo, Naples, Rome, Pisa, Florence, and Milan, and making the acquaintance of the most distinguished men of science of Italy. He arrived in Göttingen in July, suffering from a relapse caused by exposure to cold in crossing the Splügen. In the following August he entered upon his second journey to Italy. The Professorship at Pisa, vacant by the death of Mosotti, was offered to him at the suggestion of Professor Betti, but declined by the advice of Riemann's friends on account of the state of his health. He passed two successive winters in Pisa. In the autumn of 1865 he returned to Göttingen, and began to write a paper on the mechanism of the ear, which was published after his death by Professors Henle and Schering. He entrusted the completion of a paper on the surface of least area, having a given boundary, to Dr. Hattendorff. This paper is printed in vol. xiii. of the 'Abhandlungen' of the Society of Göttingen. Desirous of passing some months on the shores of the Lago Maggiore in order to collect strength sufficient to enable him to complete his unfinished works, he left Göttingen on the 16th of June 1866, and after some delay, caused by the events of the war, reached Lago Maggiore on the 23rd of the same month.

Perfectly conscious of his approaching end, and fully prepared for it, he repeatedly urged his physician to tell him how long he had to live, in order that he might thereby be guided in the selection of a labour that it might be possible for him to complete. He died in entire possession of his faculties on the 20th of July 1866, at Selasca, near Intra, on the Lago Maggiore, having only the day before worked on the mechanism of the organs of hearing, whilst he warned his attendants that his death was at hand.

He is gratefully remembered by his pupils for his liberality in imparting to them the results of known, new and important unpublished researches, and for the unwearied zeal with which he strove to impress upon them the whole truth of his lessons.

The materials for the preceding sketch of Professor Riemann's life were obtained from a 'Gedächtniss-Rede,' addressed to the Royal Society of Göttingen by Professor Schering, and some manuscript notes supplied by Professor Clebsch.

Sir Edmund Walker Head was born at Wiarton Place, Kent, February 16, 1805. His ancestor, created a baronet in 1676, resided at the hermitage near Rochester, in Kent; and his seat is said to have afforded shelter to James the Second during the memorable week of December 1688, the last which he spent in England. The grandfather of the subject of our memoir, Sir Edmund, emigrated to America, and settled at Charleston. In the War of Independence he took the Royalist side, lost the greater part of his fortune, and returned to England, bringing with him his son, afterwards Sir John. This portion of the family history was much noted in Canada and in New Brunswick when the descendant and namesake of this Sir Edmund went to administer the government of those provinces, in both of which there are many families descended from the "loyalists" of the last century.

Sir John became a clergyman, and had by his wife, Miss Walker, two children, Sir Edmund and Annette, wife of the Baron de Milanges. He died in 1838.

Edmund was sent to Winchester in 1815, where he became, through his rapid proficiency, a very favourite pupil of Dr. Gabell, the then head master. "It is hard," writes the Doctor to Sir John, in 1822, "to part with so delightful a boy; but there is virtue in parting with him: pray do not detain him beyond the proper time." He entered Oriel College, Oxford, as a gentleman commoner in January 1824, was first class in classics 1827, and elected to a fellowship of Merton.

Thus introduced into the academical world of his day, Edmund Head became familiar, and continued so through his life, with many of the leading men of that remarkable time for Oxford, and especially for Oriel: the two Newmans, the three Wilberforces, Froude, Manning, Wrangham, Edward Villiers, Denison, the Bishop of Salisbury were all more or less nearly his cotemporaries, and members of his Oxford circle. But of the friendships which he formed at that place, the most lasting, and in its consequences to himself the most important, was with George Cornewall Lewis, then student of Christ Church. No two men could be more singularly fitted to love and esteem each other, and, in a certain sense, to supply each other's deficiencies. Both were strongly addicted to the study of the past; but Lewis more in relation to antiquities and politics, Head especially in the province of history and art. Both were classical scholars of mark—Lewis, no doubt, with far more of industry and research, Head with at least equal elegance. Both were early engaged in the same line of political and social speculations; and in both liberal tendencies were accompanied by the same singular candour and modesty of judgment. Some of the epitaph, so to speak, composed by Sir Edmund for his predeceased friend, in his preface to 'Lewis's collected Essays on the Administration of Great Britain' (1864), seems, to those who knew and valued them both, to

illustrate his own character almost as much as that of the subject of his panegyric. "He rarely formed an opinion without looking at all sides of the question before him, or without having recourse to all accessible sources of information, which few men knew so well where to seek as he himself did. He was deluded by no prejudices, and jumped to no conclusions without testing them by the application of sound common sense. When he had thus formed an opinion, he adhered to it steadily, but not obstinately. He was ever open to argument, and he never refused to listen to it because it conflicted with his own view of the case. He had none of those crotchets or fancies lurking in his mind which so often taint the reasoning."

The classical accomplishments of Sir Edmund's early years he retained through life, though multiplicity of business on the one hand, and the prevalence in his mind of other tastes, especially for art, on the other, prevented him from "keeping up" his classics as sedulously as some other politicians of his generation have done. But the readiness with which he could apply his early knowledge was often displayed—never more neatly than in the prefatory quotation which he furnished to Professor Tyndall for his paper on Calorescence (Phil. Trans., Read Nov. 23, 1865).

> Forsitan et roseâ sol altè lampade lucens
> Possideat multum cæcis fervoribus ignem,
> Circùm se, nullo qui sit fulgore notatus,
> Æstiferum ut tantùm radiorum exaugeat ictum.
>
> Lucret. v. 160.

"I am indebted to my excellent friend Sir Edmund Head," says the eminent Professor, "for this extract, which reads like divination."

Not long after his election to Merton in the year 1830–31, Sir Edmund travelled over most part of Spain in company with David Roberts the artist, then engaged in making drawings for his work on that country. Three of the plates in that work, namely the view of Ronda, of the viaduct at Segovia, and the bridge at Toledo, are from his sketches. It was on this occasion that he made acquaintance with Richard Ford (then residing at Seville), the accomplished and genial author of the 'Handbook for Spain,' which ripened into a durable intimacy.

Sir Edmund brought back from his tour in Spain, and also in Italy, an increased devotion to the pursuits of what was formerly termed a "virtuoso," fondness for art, and familiarity with its history and specialties, especially the art of painting. The second part of Kugler's 'Handbook of Painting,' published in English by Murray, that which relates to the German, French, and Dutch schools, was edited by him. The third part (Spanish and French schools, published as a separate work in 1848) is his composition.

It was his intention, on leaving Oxford, to follow the civil law as a pro-

fession; but his scheme of life was altered through his intimacy with Lewis, whose father, Sir Frankland, was at the head of the Poor Law Board. Through the son's influence, Head became Assistant Poor Law Commissioner in 1836. He organized the unions in Herefordshire, the district assigned to him. It was on that occasion that he made the acquaintance of the lady who became his wife, Anna Maria, third daughter of the Rev. Philip Yorke. They were married in 1838. In the same year the baronetcy devolved on him by the death of his father. The issue of the marriage were three children—a son, accidentally drowned in Canada, and two daughters, surviving.

From Herefordshire he was transferred to the London district, and in 1841, on the resignation of Sir John Lefevre, became one of the chief Commissioners, with his friend Lewis and Sir George Nicholl. This memoir does not afford an occasion for recurring to the accusations and Parliamentary discussions, with which, indeed, Sir Edmund had personally little to do, which produced the disruption of that Board in 1847. It was reconstituted by Act of Parliament in that year under a new organization; but Lewis and Head were not included in the latter.

In 1847 Sir Edmund was appointed Lieutenant-Governor of New Brunswick by Lord Grey, then Secretary for the Colonies. The period of his appointment coincided with that of a great constitutional change in the government of the dependencies of Great Britain, the introduction of what is called "responsible government." Canada was already undergoing the transition to the new state of things under the able management of the Earl of Elgin. New Brunswick and Nova Scotia were the next subjects of experiment. Although the field of action thus afforded to Sir Edmund was but a small one, it was one which taxed the abilities and the temper of the administrator to the full extent. He occupied a post exposed to attack, on the one side, from colonial politicians who were bent on pushing their newly gained independence to excess, and British members of Parliament and politicians on the other, who could not for a long time be brought to comprehend the nature of the gift which they had bestowed, nor why a colony, having achieved the right to conduct its own affairs, was to be allowed to conduct them in a way which they disliked. And many points of serious difference were long left unsettled—the management of the public laws, the disposal of the reserved revenue, then called the "civil list," the embarrassing question of the initiation of money votes by the local government instead of at the caprice of individual members. During seven years of government in New Brunswick Sir Edmund Head contrived so far to smooth away these causes of differences, that few portions of the British empire have been better or more quietly governed than that which he conducted through this first stage of self-government. Cautious almost to excess, entirely free from that self-importance which induces colonial governors at times to forget that the maxim of "reigning and not govern-

ing" is to be carefully followed except in peculiar emergences, he united with these qualities a very conscientious loyalty to those with whom he was placed in contact. To let every project which he initiated be thoroughly known beforehand to his local advisers on the one hand, and to his superiors in office at home on the other, was with him a fundamental rule of conduct. And the result was, that, though often opposed on the one hand and sometimes overruled on the other, he was always and thoroughly trusted.

The monotony of colonial life in a remote and thinly peopled province was diversified, for Sir Edmund Head, by a strong love of nature and attachment to outdoor pursuits. He had a taste for geology, and induced the legislature of New Brunswick to employ Professor Johnston to survey the province, and publish a report on its agricultural capabilities based on its geological formation—a work of considerable local value. In later years he took great interest in the geological survey of Canada, accompanied Sir William Logan in his field operations, and took part in many a discussion on the mysteries of the "Laurentian strata." And besides his addiction to landscape-art and the picturesque, he was, moreover, a very eager and accomplished sportsman. Some of his happiest intervals of busy life were spent in the wild backwoods of New Brunswick, with Lady Head for his companion, geologizing, sketching, fishing, and shooting, until the close of the late Indian summer called them back to official employment.

In September 1854 Sir Edmund Head was promoted to the office of Governor of Canada and Governor-General of British North America, on the resignation of Lord Elgin. In this instance Lord Elgin had laid the foundation of that system of government which was afterwards administered by Sir Edmund. But the period of his administration was signalized by serious difficulties and important changes.

Not long after his assumption of the Government of Canada the Russian war broke out (1854–56). It will be in the recollection of those who watched the events of that anxious period into what embarrassment this country was for a time thrown by the difficulty of obtaining soldiers for our immediate demand, and how greatly that embarrassment was increased by the apprehension on that subject, amounting to a panic, which took possession of the public mind. Well-meaning but over-zealous agents suggested, and attempted, the levy of recruits among British subjects, not only in our American colonies, but in the United States. Rightly or wrongly, the jealous spirit of our kindred of the Republic was aroused by the proposal, and by the very slight attempts which were made to carry it into execution. Sir Edmund Head, accustomed as he was to dealing with the Americans, saw at once its extreme danger and checked it immediately, at all hazards to his own popularity, along the precarious line of boundary between his colony and the States.

"I am not in the habit of boasting," he says in a private letter of this date; "nor do I pretend to have foreseen all the consequences; but I sincerely believe that if I had given in to some of the recruiting schemes, and had thus generated a hostile feeling at Buffalo and along the frontier, war would have been upon us before this time." The danger was averted; and Sir Edmund employed himself in promoting the more worthy plan of raising troops openly in Canada itself. This was accomplished, or at least initiated, by the levy of the "hundredth," or "Canadian" regiment. But the termination of hostilities rendered its services unnecessary. It was a strong proof, however, of the popularity of the measure in the colony, that Sir Edward received between two and three hundred applications for officers' commissions.

The next question which engaged Sir Edmund Head's serious attention was one of magnitude in itself, but rendered more difficult by the amount of local interests and rivalries which were engaged in it. Upper and Lower Canada had been united into a single province, some years before, under the administration of the colonies by Lord John Russell. But no decision was arrived at respecting the permanent seat of the future government. Since the union, therefore, the executive had been located, in alternate years, at Quebec and at Toronto—a change attended with much inconvenience to the public service. But to fix on a single capital was a decision which the local ministries shrank from taking, naturally fearful, as they were, of encountering the hostility of the rejected candidates. They therefore acquiesced in the course advised by Sir Edmund Head, of referring the question to the decision of the Queen, and praying her Majesty to select the site of their eventual metropolis. But the corporations of all the towns, which conceived themselves to have a claim to this honour and advantage, were to be first admitted to urge their respective claims. Accordingly, in the course of 1857, Quebec, Toronto, Montreal, Kingston, Ottawa, and Hamilton gave in their several memorials, showing their respective advantages in point of population, commerce, position, and capabilities of defence; and the case in behalf of each was urged with considerable ability. Sir Edmund Head's opinion was in favour of Ottawa; and that place was ultimately selected by the Queen in the Ministry of Lord Palmerston, but not without much careful investigation. The difficulties of the question, however, did not cease here. The youthful democracy of Canada were not easy to hold fast; and a considerable disposition was manifested to repudiate the decision which the Crown had taken on the invitation of the colonial authorities themselves. This opposition was surmounted, however, through the patience and tact of the Governor, as well as the general good sense of the community, and the conviction that (independently of the binding engagement into which they had entered towards the Crown) the problem was really soluble in no other way. Ottawa, situated on the frontier between the two ancient

provinces, became the capital of the colony, and is now that of the confederated "dominion."

In the same year, and in the conduct of those negotiations, Sir Edmund Head visited England, and was appointed of the Privy Council.

In 1860 occurred the "progress" of His Royal Highness the Prince of Wales through British North America. The Governor-General met the royal party at Gaspé, at the mouth of the St. Lawrence, and accompanied them in their lengthened tour through his province. Shortly after this he paid a visit to England, on which occasion he was made a Knight Commander of the Bath.

The last business of public importance in which Sir Edmund was engaged in Canada related to the confederation of the British North-American provinces. The movement towards the attainment of this object originated in Canada. By the terms of the union between the two divisions of that province, Canada East and Canada West had an equal voice in the proceedings of the legislature. But the latter division was rapidly outgrowing the former in population and wealth. The result was a collision of interests which tended to a deadlock in the Government—the reforming party in the West advancing the principle of representation according to numbers, the East clinging to that equality which the existing constitution secured to it. To obtain the union of the so-called Lower Provinces (Nova Scotia, New Brunswick, Newfoundland, Prince Edward's Island) with Canada, and thus to recast the entire political arrangement, was an obvious expedient for getting rid of the difficulty. Other motives concurred, but this was the principal; and in 1858 delegates from Canada waited on Sir Edward Lytton, then Colonial Minister, in order to urge it. The Governor-General, though sincerely anxious to promote the scheme, thought it of the utmost importance that the initiative should be taken by the communities themselves and not by the executive, and therefore confined himself to the exercise of his good offices in removing all difficulties in the way of the negotiation. Personally, he was of opinion that the best course would be to effect a union between between the Lower Provinces first, and then to connect the newly-consolidated dominion with Canada. But circumstances rendered it impossible to proceed in that direction. The whole project, however, was adjourned for some time; nor was it practically revived until after the departure of Sir Edmund from Canada, which took place in 1861. It was finally carried into execution under the government of his successor, Lord Monk.

From the position which he had held in the University of Oxford and his taste for literary pursuits, it might have been expected that Sir Edmund should pay attention to the promotion of education and the extension of science in the provinces which he was called on to govern; and the sequel shows that he successfully exerted his influence to further these objects, both in New Brunswick and Canada.

In New Brunswick he took a lively interest in the extension and improvement of the Provincial University as a centre of liberal education; and in order to overcome local difficulties he appointed a commission of inquiry. Sir Edmund attended the meetings of the Commissioners, and took a part in their discussions; and although he was removed to Canada before the recommendations of the Commission were fully acted on, enough was done to ensure the permanence of the University and enhance its utility to the province.

On entering on his administration of Canada, one of the first acts of Sir Edmund (1854–55) was to promote the efforts of a number of patriotic gentlemen then engaged in the attempt to reorganize the M'Gill University, the only endowed institution in Lower Canada for the higher education of that part of the population which is of British origin. The Governor not only gave substantial aid to their enterprise, which has been highly successful and beneficial in its results, but through his influence secured the appointment of the present Principal, Dr. J. W. Dawson, a Fellow of this Society, under whom a flourishing school of natural science has been established in connexion with the University.

It was also largely due to Sir Edmund's influence that the provincial normal schools of Lower Canada were successfully established; and it was during his administration and under his patronage that the Natural-History Society of Montreal was enabled to erect a new building and greatly to enlarge its collections and other means of usefulness.

In Upper Canada the University of Toronto especially has reason to remember the exertions of the Governor in preventing the division of its endowments, and in furthering the erection of its new and magnificent buildings.

The Geological Survey of Canada under Sir William Logan was regarded by Sir Edmund as an object of special interest and importance. In the first year of his administration he authorized the augmentation of the fund annually voted for its support, raising the amount from eight thousand up to twenty thousand dollars; he took an early opportunity, without solicitation, of considerably increasing the salaries of the staff; and when the welfare and even the continuance of the survey were endangered by political changes, he was always ready to befriend it.

These efforts of Sir Edmund Head made him distinguished as a Governor who understood the subjects and sympathized with the cultivators of science and literature, and occasioned the termination of his government to be viewed by these with sincere regret.

Seven years of official labour in New Brunswick, and seven in Canada, were now to be followed by seven years of comparative rest, which completed his career. But his period of retirement was employed in a variety of public labours. He acted for several years as an unpaid member of the Civil Service Commission for organizing competitive examinations for ap-

pointments in the public service, of the Commission appointed to consider the site of the National Gallery, and (in 1867) of the Trades' Unions Commission. And those who acted with him in these several capacities can bear testimony to the influence which he exercised through his singularly calm and temperate judgment and the unfailing considerateness with which he weighed the opinions of his colleagues. He was for the last five years of his life Chairman of the Hudson's Bay Company. In April 1863 he was elected a Fellow of the Royal Society. Besides his other literary works, of which mention has been made, he was the author of the little philological essay entitled "Shall and Will, two chapters on future auxiliary verbs" (1856 and 1858)—of a translation of 'Viga Glum's Saga' from the Icelandic (1866) (a language of which the study occupied him a good deal towards the end of his life)—of articles on art-subjects in the Edinburgh Review, on Eastlake (1847), Cavalcasella (1865), and Holbein (1866), on the law of settlement, on the American Civil War (1862)—and in the Quarterly on Isaac Taylor's 'Words and Phases,' and the 'Life of Sir John Eliot.' He also occasionally amused himself with poetical composition; his essays in that department, chiefly translations, appeared from time to time in Fraser's Magazine. He was engaged at the time of his death on editing a translation of Van Praet's volume of historical miscellanies, which has since been completed by Sir Alexander Gordon. If the subjects to which his attention was thus devoted appear to indicate rather a discursive mind than serious addiction to any individual study, it must be remembered that his business in life was the public service, and his pursuits in literature and art mere accessories, to supply an active intellect with employment in the intervals of its appropriate labour.

Sir Edmund Head died suddenly, on the 28th of January 1868, probably of disease of the heart, of which the existence was unknown to himself and his friends. With him the old Baronetcy enjoyed by his family became extinct.

John Davy was born at Penzance on the 24th of May, 1790; and he died in his 78th year, at Ambleside, on the 24th of January last. He was the youngest of five children, of whom Sir Humphry Davy, born twelve years before him, was the eldest. He survived his brother thirty-nine years; and one of the most marked features in his character for the whole of this period, and, indeed, of his life, was the well-deserved gratitude and veneration with which he regarded that famous philosopher. His first introduction to scientific life was made at the age of eighteen, in the Laboratory of the Royal Institution, where his brother was then (1808) in the zenith of his fame, lecturing and prosecuting chemical research. Dr. Davy always considered the period of from two to three years during which he acted as an assistant to Sir Humphry as one of the happiest and best employed of his life. On relinquishing this post he studied medicine in Edinburgh, where he graduated in the year 1814, the same year in which he was made a Fellow of this Society. From the year 1815, up to the end of his life, he held various appointments in various parts of the world in the Army Medical Department. He passed a life of great activity, which was but little less varied than this short sketch will show the incidents of his history to have been. He has left behind him numerous papers on purely scientific subjects—chemical and biological; he has written the history of his brother's life, and has also edited his works; his medical experience has been embodied in a volume treating of Army Diseases; and he has written accounts, partly scientific, partly of general interest, of the various countries—Ceylon, the Ionian Islands, and the West Indies—in which he was at different periods of his life stationed in the course of his professional duties. The fact that the Royal Society is now in course of publishing a Catalogue of Scientific Papers, renders it superfluous to specify Dr. Davy's very numerous memoirs individually; and it will be the aim of this notice merely to give the main features of his life in outline, and to mark only the chief points upon which his multifarious labours threw light.

The first remark which a glance at a list of his contributions to science suggests, relates to the length of the period over which his activity in the way of research extended itself. His first paper was published in 'Nicholson's Journal' for 1811, and contained the result of certain investigations undertaken in vindication of the doctrines taught by his brother as to the simple nature of chlorine, or oxymuriatic acid, as it was then named, and as to the incorrectness of the then current views of the composition of hydrochloric, then known as "muriatic" acid. His last paper, one "On the Temperature of the Common Fowl," was read subsequently to his death before the Royal Society of Edinburgh this very year 1868.

During a considerable part of these fifty-seven years Dr. Davy was on actual service as a medical officer of the army. His services began in the campaign of 1815, when he was attached to a General Hospital in Brussels;

he was shortly afterwards sent out to Ceylon, where he continued during the suppression of a rebellion and up to the year 1820. After this he was for several years on Mediterranean stations, in the Ionian Islands first, and afterwards at Malta; and he was sent by Lord Palmerston in the year 1839 on a mission to Constantinople, which lasted nine months, and aiming, as it did, at effecting a reform in the administration of the Turkish hospital system, ended in failure and disappointment. His last public duty was performed as an Inspector-General of Army Hospitals on the West-Indian Station during the three years 1845–1848. In the intervals of foreign employment Dr. Davy was usually on duty at home. An 'Account of the Interior of Ceylon,' a quarto volume published in 1821, 'Notes on the Ionian Islands and Malta, with some account of Constantinople,' two octavo volumes published in 1842, and a volume entitled "The West Indies before and since Emancipation," and bearing date 1854, contain the results of his observations and investigations into the non-medical history of these stations. In a work 'On the Diseases of the Army, with contributions to Pathology,' published in 1862, Dr. Davy has embodied the results of the medical experience which he gained in the discharge of his professional duties at home and abroad. Not the least valuable portions of this volume are those which relate to the ætiology of the yellow and other malarious fevers of the tropical and subtropical countries he was made familiar with. Ten years previously to the publication of this work Dr. Davy had acted as editor to Dr. Blair's volume on 'The Yellow Fever Epidemic of British Guiana.' It is well here to put on record that, whilst discharging the duties of an Inspector-General at Barbadoes, he found time to deliver and publish a course of 'Lectures on the Study of Chemistry,' with especial reference to the agricultural requirements of the island. Dr. Davy had many years previously acted as editor of Sir Humphry's well-known and much read treatise on 'Agricultural Chemistry.'

Two volumes of 'Researches, Anatomical and Physiological,' were published by Dr. Davy in the year 1839; and they were followed by a third on the same subjects in the year 1863. The papers collected in these three volumes are of a very varied character; those on the Torpedo; on the Structure of the Heart of Amphibia; on the Generative Organs of Cartilaginous Fishes; on the Blood-corpuscles of the *Ornithorhynchus*; on the Temperature of Man in the Tropics; on the ova of the *Salmonidæ*, with reference to the Distribution of Species; and especially those on the Blood and the cause of its Coagulation, are the most particularly noteworthy, and the most particularly connected with the author's name.

The debt of gratitude which Dr. Davy owed to Sir Humphry for the assistance and sympathy which he received from him in early life, he discharged, so far as such obligations can be discharged, by the publication in 1836 of 'Memoirs of the Life of Sir Humphry Davy, Bart.,' in two volumes; secondly, by his edition of the works of Sir Humphry in twelve volumes, the first of which is a Biography condensed from the two

volumes just mentioned, the author "carefully abstaining from all that was controversial and vindicatory, trusting that what was before a duty was then superfluous;" and, thirdly, by the volume of 'Fragmentary Remains, Literary and Scientific,' which contained a sketch of his brother's life and was published in 1858. The sixth volume of this edition of Sir Humphry Davy's works, and the second volume of the first of the Biographies of him published by his brother, contain a full statement of the relative claims of Sir H. Davy and George Stephenson respectively to be considered the inventor of the Safety Lamp. Upon another occasion, and as recently as 1864–1865, as may be seen by a reference to the pages of the Philosophical Magazine, Dr. Davy engaged himself in a vindication of his brother's reputation from certain aspersions which had been cast upon it with reference to his conduct when President of this Society.

Dr. Davy was the author of two works on Angling, which have the form of colloquies, and are discursive and digressive, especially in the direction of the various biological bearings of the sport. His liking for this pursuit was, as is well known from the 'Salmonia,' common to him with the author of that work.

Dr. Davy pursued a regular and methodical course of literary and scientific work up to the latest days of his life. His activity, as seen in his later years at the Meetings of the British Association, which he regularly attended, was the wonder of much younger men. Those who saw him in ordinary life gathered from the sight the moral that regular habits in ordinary life are the best guarantee for the possession of a power for putting forth extraordinary exertions upon extraordinary occasions.

The great reputation which, in spite of all efforts to the contrary, has settled round the name of Sir Humphry Davy, has necessarily put Dr. John Davy's claims for scientific distinction somewhat at a disadvantage. The younger brother's main deficiencies were deficiencies affecting his power of imagination and his faculty of exposition, and for excellence in these mental qualities the elder brother was not less preeminently distinguished than for his more strictly scientific abilities. It is much to the credit of Dr. Davy's moral nature that no shadow of mortification or jealousy ever darkened his meditations on his brother's achievements, into comparison with which he was so constantly forced to bring his own. Nor can we close this notice more fitly than by saying what is the literal truth, that his sympathy with the cause of his brother's reputation, showing itself as it did in a repeated and successful championship of it, elevated his whole nature and spread through and over his long series of labours the warm light of a sunny memory.